ABSTRACT

Title of dissertation: ESSAYS ON PUBLIC ECONOMICS

Filippos Petroulakis, Doctor of Philosophy, 2015

Dissertation directed by: Professor Melissa S. Kearney
Department of Economics

Modern states feature an extensive government bureaucracy, whose role is present in virtually all functions of the economy. The unifying theme of the three distinct papers in this dissertation is the role of government in a modern economy.

In the first chapter, I look at taxes on bequests, the transfers of wealth from parents to their children when they pass away, and how they affect the labor supply of the parents, exploiting a policy change that reduced taxes. A tax cut implies a higher net-of-tax estate value, creating a wealth effect, which reduces labor supply, and a price effect, which raises it. Results indicate a clear reduction in participation, with the wealth effect dominating. I find an approximate reduction in participation of around 10% over the baseline.

The second chapter looks at the effect of unemployment insurance (UI) on crime. Crime fell sharply in the United States during the Great Recession, at a time of rising joblessness. This was a puzzle: crime is expected to rise, not fall, when unemployment rises. I show that UI extensions can account for part of the puzzle, explaining why crime did not rise. The higher propensity to commit crimes

associated with higher unemployment was mitigated by the fact that UI was more generous. State-level variation in extension rules provide exogenous identifying variation in benefit length. I estimate that in places with an additional $1,000 rise in UI per-unemployed-person (annually), crime would have been 1.5% higher were it not for the extensions.

The final chapter studies how government policies may have hindered recovery in Greece during the recent crisis. Despite a large reduction in labor costs, Greece failed to engineer an export-led growth. I examine how taxes and trade costs can explain this. Energy tax hikes raised the cost of wholesale energy. VAT also rose, disproportionately affecting tradables, while there was no reduction of the substantial trade costs burdening Greek exports. Using a small-scale New-Keynesian small-open-economy model, I find that a 20% reduction in trade costs would mean 8% more exports within 10 quarters. VAT and energy costs can partially explain why exports fell.

ESSAYS ON PUBLIC ECONOMICS

by

Filippos Petroulakis

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2015

Advisory Committee:
Professor Melissa S. Kearney, Chair/Advisor
Professor Ethan Kaplan, Advisor
Professor Lesley J. Turner, Advisor
Professor Felipe Saffie, Member
Professor Roberton C. Williams III, Member

Dedication

To my parents.

Acknowledgments

I owe my gratitude to all the people who have made this thesis possible.

First and foremost I would like to thank my advisor, Professor Melissa Kearney, for her continuous guidance and support, her thoughtful comments, and the opportunity to work as her research assistant, an invaluable experience in shaping my understanding of empirical research. Such a combination of academic stardom with a down to earth, friendly demeanor is a rare occurrence.

I would also like to thank my co-advisors, Ethan Kaplan and Lesley Turner. Our frequent discussions fundamentally shaped the papers included in this dissertation, and their encouragement was instrumental during times of frustration with the research projects. Thanks are also due to Professor Felipe Saffie and Professor Roberton Williams for agreeing to serve on my thesis committee and for sparing their invaluable time reviewing the manuscript.

Several other members of the faculty in the Department of Economics have given precious feedback in departmental seminars and office visits. I would like to single out Judy Hellerstein, Jessica Goldberg, Robert Schwab, Sergio Urzua, Sebnem Kalemli-Ozcan, Katharine Abraham, Andrew Sweeting, and Soohyung Lee. Special thanks are also due to John Shea for a thorough read of a draft of the first chapter (my job market paper), and his continuous help throughout the duration of the graduate program. He is a phenomenal Director of Graduate Studies.

My fellow graduate students were an invaluable peer network, for giving feedback to my work, discussing ideas, and improving my understanding of key theo-

retical and empirical issues. I would like to especially thank Ben Zou, Pablo Cuba, Daisy Dai, Matija Jancec, Tara Kaul, Jeronimo Carballo, Naomi Utgoff, Stephanie Rennane, and Lisa Dettling,

I would also like to acknowledge help and support from some of the staff members. Vickie Fletcher has been an outstanding graduate student administrator, and her direction has been instrumental throughout the past six years. Terry Davis spend several hours last Spring trying to help arrange a travel grant.

Οφείλω τεράστια ευγνωμοσύνη στους γονείς μου, χωρίς την άνευ όρων υποστήριξη των οποίων δε θα ήταν δυνατή η ολοκλήρωση αυτής της διατριβής. Τους ευχαριστώ που μου ενέπνευσαν την αγάπη για γνώση, και την ευκαιρία να ακολουθήσω 11 χρόνια πανεπιστημιακής εκπαίδευσης χωρίς έγνοια για το οικονομικό κόστος των σπουδών αυτών. Αυτή η διατριβή τους αφιερώνεται. Ευχαριστώ επίσης θερμά τους παππούδες μου Νίκο και Μαρίκα, που συνεισέφεραν όσο μπορούσαν στο κόστος του μεταπτυχιακού μου.

Τέλος, θέλω να κάνω ειδική αναφορά στη Λυδία, για την ανεκτίμητη υποστήριξη και υπομονή της κατά τη διάρκεια μια πολύ δύσκολης περιόδου, υπομονή που ίσως κανείς άλλος δε θα έδειχνε.

Contents

List of Tables

viii

List of Figures

Chapter 1: Labor Supply and Bequest Taxation

1.1 Overview

I study the effects of the Taxpayer Relief Act of 1997, which lowered the unified exemption to the estate tax, on the labor supply of future bequest givers (bequestors). To my knowledge, this is the first paper to empirically document such a response. The hypothesis is straightforward: a tax cut implies a higher net-of-tax estate value, and hence creates a wealth effect, which reduces labor supply, and a price effect, which raises it. Results indicate a clear reduction in participation for a group for which the tax cut represented a pure wealth effect, and a similar reduction for the group which faced opposing wealth and substitution effects, but for which the tax gain as a fraction of total wealth was much higher. I find an approximate reduction in participation of around 10% over the baseline for both groups.

1.2 Introduction

When a person dies, a legal entity called the estate is created, and controls the assets of the deceased. In the United States, the main way through which intergenerational wealth transfers are taxed is through a tax on the estate, which is

1

equivalent to what is more commonly known as a bequest tax. The recent focus on the growing inequality of income and wealth in the United States has sparked new interest in researching the taxation of bequests, and wealth in general (Kopczuk 2013*b*). At the same time, at the onset of the Great Recession, the burden of the federal bequest tax, called the estate tax, in the United States, was continuing its gradual downward trajectory, as enacted by the Economic Growth and Tax Relief Reconciliation Act (EGTRRA) of 2001. The federal estate tax was entirely repealed in 2010, and was reinstated in 2011 at historically-low rates.

Recent papers in the optimal tax literature (Piketty & Saez 2013, Farhi & Werning 2013, Kopczuk 2013*a*) give different rationales for why there is scope for re-distribution, through bequest taxation, within the inheritor generation (i.e. amongst the heirs). This is in stark contrast to one of the classic results in public finance, namely that the optimal taxation of capital (and hence bequests) is zero. Such models are complemented by an empirical literature that has, on the one hand, addressed the motives behind bequests, and, on the other, behavioral responses to bequest taxation. Both these empirical strands are vital in feeding into theoretical models, in order to provide coherent policy recommendations (see Kopczuk 2013*b* for a recent review). The literature on behavioral responses to bequest taxation has focused predominantly on the possible ways individuals avoid bequest taxes; these are primarily inter vivos giving, charitable contributions, and more sophisti-cated accounting techniques, such as minority discounting. An important feature of bequest taxation, its effect on work incentives, has been largely unaddressed. This dimension is crucial because it represents an incentive effect (and so affects efficiency

2

costs) and has revenue consequences. A handful of papers have looked at the labor supply of inheritance *recipients*, but not changes in taxation, and have found non trivial negative effects (Brown et al. 2010, Joulfaian & Wilhelm 1994, Holtz-Eakin et al. 1993).

In this paper, I instead focus on the effect of estate taxes on the labor supply of future *givers*. The logic is straightforward: a reduction in estate taxes would lead to a mechanically higher net-of-tax value of estates to be bequeathed. If leisure is a normal good, the wealth effect would reduce the labor supply of future givers. The price effect of a fall in marginal estate tax rates would have an opposite, counter-balancing effect, as the marginal dollar left in the estate would be taxed at a lower rate. The estate tax in the US is characterized by a high initial exempt amount, and a highly progressive schedule after the exemption, and is thus characterized by a prominent wealth effect. This is in contrast to income taxation, which typically has negligible wealth/income effects, and the focus in on substitution effects.[1] I exploit time-series and cross-sectional variation of the estate tax that followed the Taxpayer Relief Act of 1997 (TRA97), which raised the maximum amount exempt by the estate tax by 66%, but did not change marginal rates. Importantly, even though the wealth gain occurs at death and cannot be directly consumed at the time of the policy change, lower taxes imply that individuals can work/save less now without reducing the net-of-tax value of the estate. The case of income and substitution effects from estate taxation was formally death with very recently in

---

[1] It is standard in the optimal tax literature to impose preferences which are quasilinear in labor, and hence have no wealth/income effects.

Hines (2013), though Stiglitz (1978) does allude to the possibility of such effects.

More specifically, TRA97 raised the lifetime exemption for transfers for $600,000 to $1 million for single decedents, and from $1.2 million to $2 million for couples. Two groups of individuals are affected. The Middle Wealth (MW) group are those whose expected wealth at death was between the two thresholds before the tax change and who thus face counteracting wealth and price shocks. The High Wealth (HW) group are those individuals who were above the new threshold before the reform, and consequently face only a wealth effect. Those whose expected wealth at death was below the original exemption are unaffected and hence form a control group - these individuals are called henceforth the Low Wealth (LW) group. I exploit this classification and conduct a difference-in-differences (DiD) analysis for the two treatments groups relative to the control group.

There are several reasons why it is important to study the effect of estate taxation on the labor supply of givers. First, the amount is of wealth passed down every year as bequests is very large, currently over $2 trillion. At the same time, due to successive reductions, the tax now affects less only the very top, and so receipts are less than $15 billion.[2] The taxation of wealth transfers is hence a potentially important source of revenue, and studying how they affects work incentives is an important step in properly designing such taxes. Second, during the period I am studying, the tax base was broad enough to have meaningful revenue implication. In 1997, the exemption was only $600,000 ($1.2m for couples due to full deductibility of spousal bequest) until 1997. The number of households where the male head was

---

[2]Source: IRS Statistics of Income Division, Tax Stats.

aged between 60 and 70 in 2000 and who were expected to die with wealth higher than the exemption were 1.1 million (7% of households). Note that this number does not include any cohorts of baby-boomers, so it is even higher in future years: in 2003, had the exemption only risen by inflation ($688,000 for singles, $1.37m for couples), the number of eligible households would have been 1.75 million.[3]

Furthermore, since estate taxes are only imposed after death, individuals will need to have engaged in estate planning to be affected by the tax, and it is hence standard to consider individuals at the end of their work-cycle or later. The behavior of workers as they approach retirement has been the subject of a very large amount of scholarly work, due to the demographic implications it has, mostly related to the large social insurance programs for the elderly and retired, Social Security and Medicare. Even though the individuals under study are unlikely to rely on transfers, their behavior can still have effects on Social Security and Medicare finances. Social Security benefits are actuarially fair (the expected lifetime payout is independent of the retirement decision), so the date of retirement is what is relevant for the trust fund, as higher participation implies more contributions.[4] In addition, since the 1990s, there has been a significant reduction in the number of retirees receiving health insurance from former employers (Blumenthal 2006), so delaying retirement also eases strain on Medicare, since individuals can use employer-based coverage instead of Medicare.

The primary contributions made here are on the literature on bequest taxes,

---

[3]Source: Author's calculations from the Survey of Consumer Finances.
[4]The latter is not necessarily true for Medicare, as Part A (Hospital Care) is mostly financed by payroll taxes, but parts B and D are financed by members contributions.

and the literatures that look at the labor supply of the wealthy and those near retirement. A secondary contribution of this paper is in the literature on wealth effects, which are at the core of many interesting issues in economics. For example, on the macro side, changes in wealth can trigger redistributive effects between borrowers and savers, shaping the evolution of inequality over time. On the micro side, wealth effects on consumption and labor supply decisions can be crucial in assessing the effects of tax policy changes. At the same time, estimation of wealth effects has proven a daunting task. For instance, regarding labor supply, even though it is widely accepted that leisure is a normal good and positive wealth shocks should lead to a reduction in labor supply, robust empirical estimates of such effects are difficult to obtain, as labor supply is affected by a number of unobservable characteristics. The literature has mostly focused on unearned income and wealth shocks (see Blundell & MaCurdy 1999 for a survey) as a way to overcome this issue. In this paper, I instead exploit the tax change as an unexpected shock to *earned* wealth. This approach is closest in spirit to that Brown et al. (2010), who look at how unexpected inheritances affect the labor supply of inheritance recipients.

I use two different datasets for this analysis, leveraging their different attributes. First, I use the 1995, 1998, and 2001 waves of the Survey of Consumer Finance (SCF), a stratified sample of household level data on asset and income, which oversamples the wealthy. The SCF contains a substantial sample for the top group, but it has few observations for the middle group. As such, I then turn to the Health and Retirement Study (HRS), a longitudinal sample of individuals over age 50, and their spouses. The HRS is designed to be approximately representative

6

of the population near retirement which makes it suitable for studying the middle group (which occupies approximately the 90th to the 95th percentile of the household wealth distribution).

The results suggest that for males age between 50 and 80, there is a non-trivial reduction in labor force participation as a result of the fall in estate taxes. Participation falls by around 10% for both groups, confirming a long-standing theoretical argument in favor of capital taxation, namely that is raises labor supply (Diamond & Saez 2011). This result also provides partial support to the joy-of-giving over the wealth-in-utility model of giving, as the latter would imply that the givers care about gross estates, and so they would not respond to tax changes.[5]

The paper is organized as follows. Section 1.3 provides an overview of the previous literature and sets the discussion, while also providing some background on the estate tax. Section 1.4 provides the institutional background to estate taxation, and a simple theoretical framework to guide the analysis. Section 1.5 describes the data and outlines the empirical strategy. Section 1.7 provides the results of the analysis. Section 1.8 provides some robustness checks, and Section 1.9 concludes.

---

[5]There are two other motives that the literature has examined. The altruistic motive, where parents fully internalize the utility of their heirs, and the strategic motive, which views bequests as a means by which parents induce proper behavior on the part of their children. These have been less empirically supported in the literature, but it should be noted that the results of this paper are consistent with the altruistic model.

1.3 Previous Literature

1.3.1 Literature on Wealth

Even though the literature on the importance of wealth is very old, identification is so difficult that there is no real consensus even regarding the size of wealth accumulation relative to the economy as whole. In a classic debate, Kotlikoff & Summers (1981) argued that up to 46% of household wealth is comprised of bequests, while Modigliani (1988) estimated that lifecycle savings account for over 80% of wealth.[6] The cause of this discrepancy rested on different assumptions on what constitutes inherited wealth, the most important one being whether the interest earned on bequests should be considered self-accumulated wealth or not (Brown & Weisbenner 2004). Gale & Scholz (1994) look at inter vivos transfers (gifts) and find that they account for over 20% of wealth (32% if college aid is included). They also document substantial underreporting of gifts received compared to gifts given, and this shortcoming of survey data in estimating wealth flows might explain the wide-ranging nature of estimates.

In a recent paper, Piketty (2011) takes a very different approach, and instead considers a very large time series of inheritances for France, from 1820 to 2008. He defines an annual inheritance flow as the total market value of assets either bequeathed or gifted in a single year, and estimates a U-shaped pattern of inheritance flows as a fraction of national income, from around 20-25% in the 19th century, then

---

[6]In a recent editorial, Summers (2012) estimates that of the total wealth of US households of $60 trillion, $1.2 trillion is passed through bequests every year, but revenue amounts to only 1% of this figure.

down to around 5% after WWII, up to over 15% after 2000. He argues that this pattern is well explained by the difference between return on investment and growth.[7] These figures might be slightly lower in the US (though, as of 2012, population growth is higher in France), but crucially, these results are the first to empirically show that the assumption that due to higher upward mobility and the importance of human capital over social status, inheritances are no longer important, and bequests are trivial compared to labor income, is wrong.

In the optimal tax literature, there has been a wave of recent papers (Piketty & Saez 2012, Piketty & Saez 2013, Farhi & Werning 2013, Kopczuk 2013*a*) that overturn the classic result of zero optimal tax on capital. The older literature considered two justifications for this result. In a dynastic infinite horizon setting, Chamley (1986) and Judd (1985) discuss a long-run framework in a model without stochastic shocks, so capital taxation introduces a distortion whose magnitude grows over time, and is trivially set to zero. Atkinson & Stiglitz (1976) instead consider a two-period model with heterogeneous skills, and show that under weak separability, optimal taxes on all commodities (including bequests) is zero if income taxation is optimal. These results break down once certain strong assumptions are relaxed, notably homogeneity in inherited wealth, in which case non-zero taxation is optimal.

---

[7]Piketty (2010) addresses the Kotlikoff-Summers-Modigliani debate by arguing that since both studies rely on a single data point in time and thus have to make steady-state assumptions. Using his data, he shows that using either definition gives a much higher value of the bequest to household wealth fraction than either of those studies, when considering an out-of-steady-state, long run perspective.

1.3.2 Bequest motives and Behavioral Responses to Estate Taxes

The empirical literature on intergenerational transfers is rich and varied, focusing primarily on identifying bequest motives or documenting strategies that reduce the size of taxable estate at death. Such responses can be long before, or right before/after death. As the present paper is concerned with a response long before death, I will not consider the latter, and instead focus on bequest motives, inter-vivos transfers, and wealth accumulation.

Information on bequest motives comes primarily from survey questionnaires. Over 50% of respondents to the SCF (1998) say that leaving an inheritance is important/very important, only 18% say it is not important; 50% expect to leave a bequest. Bequest motives rise in wealth: median household wealth of those deeming bequests very important is \$290,000, versus \$210,000 for the rest. The structural literature on bequest motives has reached a consensus that bequest motives do exist, and are characterized by heterogeneity across and within individuals (Kopczuk 2013b). The motives that have received most attention are the egoistic luxury good motive (wealth-in-utility), the joy-of-giving motive (net bequests enters parental utility), the strategic motive (where bequests are behavior-inducing), and the accidental motive. Kopczuk & Lupton (2007) fit a structural model to the data and find empirical support for the luxury good motive (bequests rising in wealth), but not for the rest. Kopczuk (2007) uses a combined dataset of estate and previous year's income tax returns, and shows that estates grow until shortly before death for the very wealthy, while the composition of assets changes shortly before death in

a manner indicating tax avoidance. He argues that this pattern indicates a trade-off between control and tax liability-minimization (and thus the desire to leave higher bequests), since planning earlier is more tax-efficient.

A prominent margin of response is the timing of transfers, which is influenced by how the tax code treats transfers at death (bequests) and when alive (gifts). First, both taxes are unified into a single lifetime transfer (gifts and bequests) schedule, with a single exemption for all transfers. However, allowance is made for an annual maximum level ($13,000 in 2013) of gifts *per recipient*, for limitless recipients, which are not included in the lifetime exempt amount. In theory, an individual could give enough gifts every year up to the tax-free limit, and a combined sum of bequests and gifts below the lifetime exempt amount, and not pay any estate tax, regardless of wealth. Surprisingly, this strategy is extremely underutilized: up to 90% of individuals who would benefit from making many annual gifts fail to reach the maximum allowable limits (McGarry 2000, Poterba 2001). Second, gifts are taxed on a tax inclusive basis: with estate tax τ, the tax on gifts (if taxable) is $\frac{\tau}{1+\tau}$.[8] Third, estates, but not gifts, benefit from a step-up in basis: if an asset with unrealized capital gains is passed on to the estate, the heir will be required to pay a capital gains tax upon realizing gains *only* on the amount by which the asset has appreciated since the bequest, not since acquisition by the decedent. For a given tax rate, gifts are preferable, unless the asset to be transferred has appreciated significantly, so that the estate tax paid on a smaller amount implies a lower tax liability from the

---

[8]For example, $100 with a tax rate of 33% has a $33 tax liability if bequeathed. If gifted, then the tax applies only to $\frac{1}{1+\tau}$ of the amount, and so the tax liability is $25.

11

capital gains tax on the appreciated amount. Lower estate taxes or hikes of capital gains taxes should hence make gifts unambiguously less attractive.

Bernheim et al. (2004) use TRA97 as a policy experiment, and recognize that the incentives embedded in the policy are systematically different for different groups. Those between the old and new exemption faced a very strong price effect to reduce their gift giving frequency, while marginal incentives were unchanged for those below the original exemption, and those above the new exemption. They find a reduction in gift-giving frequency for those affected of around 10-14 percentage points, which is substantial given that only 38% of households made gifts before 1997. Joulfaian (2005) focuses on the relationship between estate and capital gains taxes. He uses administrative data from the estate tax returns of 1989 with information on gifts made from 1977 to 1989, and, using variation for state capital gains, gift, and estate taxes, he shows that indeed capital gains taxes significantly reduce gift-giving.

Since bequest taxes ultimately tax wealth, the effect of estate taxes on wealth *accumulation* is particularly important. Kopczuk & Slemrod (2001) use administrative data covering the universe of estate tax returns between 1916 and 1996, and focus on the richest 0.5% for each year. They note that it is expected lifetime taxation that should govern the responses of wealth accumulation to expected bequest taxes at death, and use an imputed marginal tax rate at age 45 (based on wealth at death) as a proxy for expected lifetime taxation. As this is likely correlated with wealth, they use exogenous tax measures as instruments for this proxy (the top rate, and rates at 40 and 100 times average wealth), for time series variation, and their

12

lags, for cross-sectional variation. They conclude that the elasticity of wealth with respect to the net-of-tax rate at 45 is 0.16: an estate tax rate of 50% for the top would reduce reported wealth at death by 10%. Similarly, Joulfaian (2006) calculates a measure equivalent to marginal tax rate 10 years before death, and obtains a similar estimate. Both papers identify an effect that is the sum of tax avoidance and wealth reduction, with the latter being the economically important one. Holtz-Eakin & Marples (2001) study the effects of estate taxation during wealth accumulation phase, relying on state-level cross-sectional variation in estate taxes. While they instrument for state-level estate taxes, this might not be enough to deal with the endogeneity of location decisions. In addition, federal estate taxes are much higher than state-level equivalents. Nevertheless, their estimates are in line with the other two papers, which suggests that at least part of the wealth response to estate taxes is during the accumulation phase, and not only a result of avoidance. Importantly, the literature on the timing of transfers and the effect of estate taxes on wealth accumulation indicates that individuals are responsive to bequest taxes long before they die.

1.3.3 Shocks Near Retirement

Estate planners are expected to be close to retirement age, and it is hence crucial to consider the literature on how shocks at older ages affect retirement, in order to illustrate why it is reasonable to expect labor supply to respond to a change in the estate tax.

An obvious such shock naturally concerns pensions. Early evidence from pension shocks came from window plans, strategies employed by large firms in the US, starting in the early 1980s, as a way of incentivizing early retirement by older employees. Lumsdaine et al. (1990), look at the window plan of a Fortune-500 firm in the early 1980s, with early retirement rewards of up to a year's salary for those between 61 and 64. The authors employ the option value method and find dramatic effects of the plan; the probability of an individual retiring at 60 given that he was employed at 50 jumps from 37% to 77%. Blundell et al. (2004) exploit the variation in pension wealth and retirement incentives in the UK and find that an additional £100,000 of pension wealth raises the probability of retirement by around 20%.[9]

Another wealth-related shock concerns inheritance *receipt*. In a classic paper, Holtz-Eakin et al. (1993) use IRS income tax return data of inheritance recipients to test the famous conjecture by Andrew Carnegie, who argued that large inheritances lead to lower work effort. They find significant responses, especially for large amounts: a single individual receiving $150,000 is four times more likely to drop-out of the labor force than an individual receiving $25,000. More recently, Brown et al. (2010) use HRS data, which includes ex ante information on inheritance expectations, as well as expectations of retirement. As such, they are able to get around the potential endogeneity of inheritance to labor supply, and find significant positive effects of inheritance on retirement. Regarding wealth shocks more generally, Imbens et al. (2001) study a sample of lottery winners in Massachusets in the 1980s,

---

[9]The literature on the retirement incentives embedded in Social Security is extensive and I will not consider these here. Interested readers can consult Lumsdaine & Mitchell (1999) or Coile & Gruber (2004).

who were surveyed on their demographic and wealth characteristics, and provided access to their Social Security data. They run regressions of earnings on an annualized metric of the lottery prize and find that the marginal propensity to earn with respect to wealth is -0.12 overall and -0.14 after 6 years. The largest effect is for those between 55-65, and is -0.17 after 6 years. Interestingly, there is no effect after 65.

Non-financial shocks have also been shown to have substantial effects on retirement decisions. Two prominent examples are shocks to health-insurance eligibility before age 65, when Medicare coverage kicks in (Gruber & Madrian 1995, Nyce et al. 2013), as well as shocks to health itself (Coile 2004).

A note on terminology is in order. The income tax literature breaks down income tax changes into income and price (substitution) effects. In the present context, there is no change in how income flows are taxed, but only in the taxation of the transfer of wealth at death. However, the income effect refers to non-labor income, which is a concept akin to wealth, and I will use those interchangeably. Note that the wealth effect refers only to changes in the value of wealth. The literature on savings also uses the term "human wealth effect" to refer to the effect a change in the interest rate has on the calculation of the present discounted value of lifetime wealth. In addition, I will interchangeably use the words givers and donors to refer to individuals that give bequests, foregoing terms more associated with the legal literature, such as legators or bequestors.

1.4 Conceptual Framework Institutional Background

The estate tax in the United States has existed in its current form since 1932; it is characterized by a high initial exemption, and high progressive marginal rates thereafter. The Tax Reform Act of 1976 saw the first rise in the exemption since 1942, from $60,000 to $120,667, as a way of alleviating bracket-creeping, the unwanted, inflation-driven ascent of ever more households in the tax-paying brackets. It also unified estate and gift taxes into a single unified lifetime transfer schedule, as outlined above.

TEFRA of 1982 introduced full (used to be half) marital deduction of bequests, meaning that all transfers through the spouse of the deceased through the estate is fully tax free. This essentially means that, with minimal estate planning, an individual can double their exemption. For instance, if the exemption is X dollars, then, when the individuals dies, he or she can bequeath X dollars to the children through the estate, and all the rest to the spouse (with no tax). Then, when the spouse dies, he or she can also bequeath X dollars for free to the children (plus whatever is left of X, taxable at the respective rates), thereby raising the couple's exemption to 2X. Finally, in 1981 a gradual reduction of taxes was instituted, and by 1987 the exemption had reached $600,000, with rates ranging from 37% to 55%. This was the last change in the estate tax until 1997.

Currently, the federal estate tax schedule has a very large exemption amount ($5.25 million per individual in 2013) and brackets of 37, 38, and 40% above that exemption, so it is approximately flat above the exemption. At the time of the

TRA97 reform it was highly non-linear, with several brackets above the exempt amount (see Table 1.1 for details). However, the two groups affected were both treated in a linear fashion. The middle group faced a reduction in rates from 37% or 39% to zero, and the top group experienced no reduction in marginal rates, only a rise in the exemption, and so a reduction in average taxes. As such, it is convenient to illustrate the intuition of how the estate tax can affect labor supply through a simple linear schedule. I closely adopt the framework of Hines (2013), who uses a simple linear tax schedule. This is the only paper, to my knowledge, that explicitly looks at the income and substitution effects of the estate tax for givers.[10]

Consider a static framework, where individuals work in order to consume or leave a bequest. There is no tax on consumption or a tax on income, only a proportional tax on bequests, given by t. Let y, c, h, w be wealth (or non-labor income in general), consumption, hours of work, and the wage rate. Leisure l is simply equal to $H - h$, where H is the time endowment. Bequests B are positive if labor and non-labor income are larger than consumption. The only innovation compared to the standard income tax model is the fact that the estate tax is (effectively) imposed on earnings *only* if bequests are made. In essence, the estate tax is a consumption tax, not an income tax, and so it is the "price" of bequests that is changing, not a tax on income. The price of bequests p is simply equal to the reciprocal of the net-of-tax-rate, or $p = \frac{1}{1-t}$.

This is seen more easily when the budget constraint is taken into account.

---

[10]Kopczuk (2013*a*) considers implicitly such effects in the context of optimal taxation, but only through a perturbation involving estate and income taxes to study recipients, so that effects on parents cancel out.

Bequests are equal to the net-of-tax difference between income (labor and non-labor) and consumption:

$$B = (1-t)wh + (1-t)(y-c) \Rightarrow \frac{B}{1-t} + c = wh + y$$

$$\Rightarrow pB + c = wh + y. \tag{1.1}$$

This also shows why it is more convenient to consider the tax indirectly, through p; a change in the tax works through a change in p. The individual's problem is then given by

$$\max_{c,h,B} u(c,h,B) \tag{1.2}$$

$$\text{s.t. } pB + c = wh + y$$

The budget constraint will trivially bind, so that $\lambda > 0$. Assuming for simplicity interior solutions, the solutions to utility maximization are of the form $c^* = c(p,w,y)$, $h^* = h(p,w,y)$, $B^* = B(p,w,y)$.[11] As in Hines (2013), the easiest way to consider the effect of the tax on labor supply is through the decomposition from the total derivative:

$$\frac{dh}{dt} = \frac{\partial h}{\partial y}\frac{dy}{dt} + \frac{\partial h}{\partial p}\frac{dp}{dt}. \tag{1.3}$$

The $\frac{dy}{dt}$ term captures the effect of the tax change on after-tax bequest *receipts*.

---

[11]I am assuming that the government budget does not factor into individual decision. Hines (2013) more formally assumes that the government budget enters utility in an additively separable manner, which is qualitatively equivalent.

As Hines notes (see Appendix A.1), if this term is positive, then bequests are an inferior good. It will be zero if and only if the parental generation is not affected by the tax change. I assume it is zero for simplicity, especially since the population of interest is old itself.

The uncompensated effect of the price of bequests on labor supply can be decomposed in a compensated (substitution) effect and an income effect:

$$\frac{\partial h}{\partial p} = \frac{\partial h^c}{\partial p} - \frac{\partial h}{\partial y}B. \tag{1.4}$$

Using (1.3), and the fact that $\frac{dp}{dt} = \frac{1}{(1-t)^2}$, the total effect of the tax change is

$$\frac{dh}{dt} = \frac{\partial h^c}{\partial p}\frac{1}{(1-t)^2} - \frac{\partial h}{\partial y}\frac{B}{(1-t)^2}. \tag{1.5}$$

This is the standard decomposition into income and substitution effects. The first term is negative by assumption of the Slutsky matrix; higher p means higher taxes at the margin, reducing work incentives. If leisure is a normal good, so that $\frac{\partial h}{\partial y} < 0$, the second term is negative; higher taxes reduce income and raise labor supply. Multiply and divide by t and h we finally have the response in terms of elasticities:

$$\epsilon_{h,t} = \frac{t}{1-t}\left[\epsilon_{h,p}^c + \epsilon^I S_{B,y}\right]. \tag{1.6}$$

Here, $\epsilon_{h,t}$, $\epsilon_{h,p}^c$, and ϵ^I are the uncompensated hours elasticity with respect to t, the compensated hours elasticity with respect to p, and hours income elasticity,

respectively, while $S_{B,y}$ is the negative of the ratio of bequests to non-labor income.

The magnitude of the response also depends on $|S_{B,y}|$, the ratio of bequests to non-labor income. There is no clear direction for the theoretical magnitude of this parameter, but if we think that there is a mean reverting relationship between child and parental incomes, due to progressive taxation perhaps (Farhi & Werning 2010), then $|S_{B,y}|$ is likely to be lower for the very wealthy, implying larger income effects for the middle group.

It is intuitively helpful to consider a specific functional form example. The simplest way in which bequests enter individual utility is the wealth-in-utility model, most straightforwardly captured by lifetime utility rising in wealth in an additive fashion. Trivially, estate taxation has no effect in this case, as the individual only cares about wealth, not the net-of-tax value of the bequest. A more interesting general case is the joy-of-giving (also known as warm-glow) model, where net-of-tax bequests enter individual utility, as a consumption good. In this case, individuals care about the post-tax bequest they leave behind: with additive preferences, as long as there is some curvature in the bequest motive, consumption, labor, and bequests will depend on the price of the bequest (the estate tax).

The CRRA form is particularly convenient. Here I generalize Keane (2011), and define

$$U = \frac{c^{1+\eta}}{1+\eta} - \frac{h^{1+\gamma}}{1+\gamma} + \frac{B^{1+\alpha}}{1+\alpha}, \tag{1.7}$$

where $\eta \leq 0, \gamma \geq 0, \alpha \leq 0$. I fully solve the problem in Appendix A.1. Here,

note only that using this functional form, (1.6) can be written as

$$\epsilon_{h,t} = (1+\alpha)\frac{\eta S_{B,y}}{\alpha\gamma/S_y - \eta\alpha S_{w,y} - \eta\gamma S_{B,y}}\frac{t}{1-t}, \tag{1.8}$$

where $S_{w,y}$ and $1/S_y$ is the ratios of wh and c to y, respectively. The direction of the effect depends on α, which is the coefficient of relative risk aversion with respect to bequests. if $|\alpha| > 1$, then $\epsilon_{h,t} > 0$, and the income effect trumps the price effect. See A.1 for details.

1.5 Empirical Strategy and Data

1.6 Estimation

The policy experiment I exploit is the TRA97. Before 1997, the lifetime exemption stood at \$600,000, with a progressive system of several brackets thereafter, starting at 37% and ending at 55%. TRA97 raised the exemption incrementally to \$1 million by 2006, and then indexed it to inflation. It also lowered the capital gains tax from 15% and 28% to 10% and 20% (two brackets depending on labor income). As already mentioned, this reform was superseded by EGTRRA01, which further raised the exemption to \$1 million in 2002, and even more aggressively in the future. EGTRRA01 also lowered marginal income tax rates, thus complicating the analysis. Conversely, OBRA1993 and the 1991 budget raised the top rates. As such, I focus in the period 1994-2000, where the incentives are much more transparent and easy to disentangle than in future years

Though TRA97 was signed in August 1997, it is almost certain that estate planners expected changes to occur at least a few months before that (Bernheim et al. 2004). It is then likely that responses can be seen in 1997. One the other hand, TRA97 was the first change in the estate tax for ten years, and the first act to affect the estate tax since ETRA1981.[12] Before that, TRA76 was the first change since WW2. As such, one can assume that the reform was not anticipated before the debates started, and that it was not expected to be superseded in the near future.[13]

My identification strategy takes advantage of the fact that the reform created three distinct groups in terms of how incentives varied.[14] For convenience, I repeat the classification introduced earlier. Those who expect their wealth at the time of the bequest to be below $600,000 were unaffected, and thus form a natural control group. I consider those individuals with expected wealth from $300,000 to $600,000, so as to have as good a control group as possible, and denote by LW. Second, those who were expecting their wealth to be between $600K and $1m, the MW group, faced a reduction in their marginal tax rate from 37% or 39% to 0. This group thus experienced a fall in both average and marginal tax rates, and thus a wealth effect which would reduce labor supply, and a price effect which would increase it. However, for this group, the tax gain is a potentially very high fraction of total

---

[12]There were minor estate tax provisions in OBRA93, but they involved only the very top brackets and they were formed to resolve issues in ETRA81.

[13]Bernheim et al. (2004) note that there was some discussion in Washington about the estate tax in 1994 and conjecture that individuals could have been expecting a reduction earlier. They use the SCF to look at how the rise in the exemption affected gift giving, and hence their strategy is not affected by changes in income taxes. As such, they are able to use the 1992 wave of the SCF as well and can indirectly test whether there was a change in the frequency of gift-giving before 1997. Their results do not support such a conclusion.

[14]This is similar to Bernheim et al. (2004), who study the same reform.

wealth. For instance, the estate of an unmarried individual with wealth at death of $1 million would have had to pay $153,000 in taxes. For a married couple with $2 million, the amount would have been twice as much, $306,000, and so this gain represents an 18% rise in net-of-tax bequest.[15] Finally, those who were expecting their wealth to be above $1m, the HW group, did not have a change in marginal tax rates (starting at 41%) but they did get an extra exemption of $400K, and so only experience a wealth effect.

I follow a standard difference-in-differences strategy. The estimation involves regressing labor supply participation on the treatment, year and interaction dummies. The basic model is given by

$$y_{igt} = \boldsymbol{\alpha} X_{igt} + \boldsymbol{\lambda_t} + \boldsymbol{\delta_g} + \boldsymbol{\beta I_{gt}} + \epsilon_{igt}, \tag{1.9}$$

where $\boldsymbol{I_{gt}} = \mathbb{1}(g = \text{MW or HW} \ \& \ t > 1996)$.

The i subscript denotes individual, g group, and t year. The groups are defined as above, and the omitted (baseline) group is the LW; the MW and HW dummies are summarized by the 2-dimensional vector $\boldsymbol{\delta_g}$.[16] Similarly, the year dummies are given by the $\boldsymbol{\lambda_t}$ vector. The $\boldsymbol{I_{gt}}$ term is the vector of the DiD coefficients, the interaction

---

[15]Note that technically the system has a bracket structure for any level of estate, starting at 18%, and a unified credit. Until 1997, this credit was $192,800, which worked out to a $600,000 exemption. As such, the total savings of the reform for an unmarried individual who dies with $1 million are $150,000*0.37 from the 37% bracket applying from $500,000 to $750,000, and the $250,000*0.39 on the 39% applying from $750,000 to $1 million, so a total of $153,000. For a married individual with $2m household wealth at death these numbers double, as they occur for both members of the couple (assuming there is no asset accumulation after the death of the first spouse).

[16](I keep married individuals with expected wealth between $300,000 and $600,000 as an additional sample in the control group.

between year and group dummies. The year dummies capture aggregate factors that would cause changes in the dependent variable even absent the policy change. The group dummies capture differences between the groups prior to the change. The interaction terms measure the effects of the policy, and are the variables of interest: negative coefficients in the interacted year-group dummies would be evidence that the raise in the unified exemption led to a reduction in participation for the given group in the given year.

Finally, X_{it} indicates a number of relevant covariates. It includes age and its square, income and its square, educational attainment, self reported health status, marital status, dummies indicating whether the individual had previously received an inheritance and whether he is expecting one, and the fraction of net worth in unrealized capital gains (to control for the fact that the capital gains tax also changed).

In the following section I describe why I rely on multiple data sources. The concurrent use of two different datasets has its drawbacks, most importantly the fact that some of the variables can be non-nested. In order to be consistent when comparing results, I decide to define participation by earnings, where I classify as in the labor market individuals who have non-zero earnings or report having a business/being self-employed. As participation is quite complex to encode (SCF has over 20 different possible responses to the participation question), this is the most straightforward way of comparing the results.

It should be stressed that the sole purpose of this paper is to document whether individuals respond to estate taxes on the participation margin in plausible magnitudes, and discuss the likely consequences. I am not attempting to uncover precise

24

structural parameters underpinning behavioral responses. The atheoretical nature of the DiD approach, which makes it powerful in exploiting policy changes to describe such behaviors, where more structural approaches would not have helped, is also a serious impediment if ones tries to identify exact elasticities. As Blundell & MaCurdy (1999) note, the interaction term of a labor supply DiD regression identifies an estimate that is a weighted income and substitution effect, but the precise interpretation of the substitution effect depends strongly on covariates. For instance, in a standard DiD setting where the treatment group is affected by an income tax change, the DiD estimate would be interpreted as a Marshallian elasticity if a measure of static income was included in the regression and the tax had no differential income effects (Keane 2011). As the wealth effect is so substantial for the treatment group in the present application, this assumption is clearly not satisfied. An additional complication in this setting is that elasticities are by construction local measures, capturing responses for small changes in taxes. Here, the marginal tax rate for the MW group went from 37 or 39% to 0, making the structural interpretation of the coefficients even more difficult. Of course, these issues only apply for the MW group - the HW group only faces a wealth effect and so interpretation is straightforward in this case.

Another implicit assumption in this static framework concerns the timing of the tax. Suppose the exemption rises at time T_n, and recall that the exemption was set to rise with inflation after 2006. But the gain actually occurs at time T_d, where T_d is the expected date of death, and so $T_d - T_n$ is life expectancy at T_n. For a 65 year old male who expects to die in 20 years, and a constant real rate of interest at

2%, the real present value of \$306,000 at date T_d is \$205,000 at date T_n[17].

A standard problem that arises when dealing with the estate tax is the fact that the individuals do not make decisions based on current wealth, but rather on the amount of wealth they expect to bequeath to their heirs. As such, I need a measure of expected wealth at death, and I use a method constructed by Attanasio & Hoynes (2000), and adapted to such problems by Holtz-Eakin & Marples (2001) and Bernheim et al. (2004) (the latter use a very similar method). To calculate expected wealth, I run quantile regressions for ten quantiles (5th to 95th in increments of ten) on income, age (and their squares) interacted with year, a cohort variable, and other relevant covariates. The sample is then split into deciles of wealth for each year, and wealth is adjusted using the age coefficients and the cohort variable for the appropriate decile and year, and the CDC life tables.The implicit assumption for the validity of this approach is that households expect to face the same taxes in real terms at the time of death. As mentioned above, TRA97 was the first change in ten years, and was designed to keep the real value of the exemption constant. \$1m in 2006 is roughly \$600,000 in 1987 dollars. TRA97 provisioned for the exemption to rise with inflation thereafter, and so this assumption is not unreasonable. As a check, I will also use current wealth, which confirms the results. Table 1.3 shows detailed summary statistics of projected wealth at death, together with current wealth, for comparison purposes.

Finally, the validity of my strategy rests on the assumption that the control

---

[17]This is the tax gain for a married individual with expected wealth at death of \$2 million or more.

group is not affected by the policy change. Given that decisions are made based on expected wealth at death, it is possible that individuals at the top of the control group believe that they will increase their wealth by enough so that they enter the MW group by the time they die, based on information not captured in my data (e.g. trends in local house prices). The identification strategy would then be wrong if that were the case, as the control group would be invalid. It is also conceivable that individuals in the control group could respond to the change by raising their participation in order to take advantage of the elimination of the next bracket. While this is plausible for individuals close enough to the cut-off (especially if labor indivisibilities are present), it is improbable for the majority of the control group. For example, consider an individual who is at $1 million, through a combination of skill and leisure preferences. Such an individual could raise his wealth by an additional $300,000 by working (and/or saving) more after the policy change, versus $260,000 before the policy change.[18]

It is still important to test for this assumption, and one way to do this is to conduct a "placebo" test within the control group (the top and bottom half); for my strategy to be valid, there should be no differential effect on the two sub-groups. I conduct such an experiment in the robustness test section. Another way of showing this is by restricting the level of expected wealth in the control group (results not shown).

---

[18]Note that the opposite case, where individuals fall from the middle to the low group as result of the policy change, is implausible, as in that case bequests would be Giffen goods (their consumption falls as their price falls.

1.6.1 Data

The nature of the question and the sample of interest in this paper means that data issues are not trivial. Specifically, I need a large enough sample for all three groups, with HW and MW being the more difficult ones. Ideally, one would obtain a large longitudinal sample from an administrative dataset. In the absence of such data, the next best alternative is to use survey data. As no single survey dataset can give a large enough sample for both treatment groups, I resort to using two complimentary surveys, and leverage their specific attributes.

Specifically, for the HW group, I use the Survey of Consumer Finances, the largest and most comprehensive study of asset and liability stocks and flows in the United States, together with information about income, labor supply, and other relevant variables. The survey is conducted every three years, and consists of a random stratified cross-section of households. I use the waves of 1995, 1998, and 2001 (which cover the previous respective calendar years), and hence construct a repeat cross-sectional sample. Crucially, for the purposes of this study, SCF oversamples the very wealthy: using a two step algorithm, the survey designers select the respondents based on locality and wealth, and construct corresponding probability weights.[19]

The dataset contains a set of five imputed values (implicates) of missing variables, which were constructed by the SCF designers using bootstrap techniques from

---

[19]Note that this survey design ensures that the sample is drawn with the same probability distribution before and after the reform, a requirement for the validity of DiD with repeated cross-sectional data (Blundell & MaCurdy 1999). This is analogous to the no-attrition requirement for panel data (Angrist & Krueger 1999).

a conditional distribution of the data. This allows both a more efficient estimation that a single imputation, as well as a precise formula to calculate the imputation variance. Since the survey is protected by strict confidentiality, information about the stratification of the sample (such as locality) is not available. To remedy this, the SCF also provides 999 sample weight replicates designed to capture sampling variation. I use all this information in the analysis presented in this paper, and standard errors are adjusted according to the formula provided by the SCF designers, which is given by $\sqrt{6/5 * \text{imputation variance} + \text{sampling variance}}$.[20] Due to the nature of data, it is necessary to use weights both when constructing summary statistics and when running regressions.

However, the sample size for the MW group is quite poor in the SCF. This is due to the structure of the survey. The SCF is explicitly designed to provide as good as possible a snapshot of wealth holdings in the US. It is well documented that the concentration of wealth is extremely skewed. In addition, the MW group is particularly small and much harder to define, as it is bounded above and below. Hence, the SCF has a large sample of individuals at the very top, a considerable number at the LW group, and few at the MW group.

As such, to gain a better measurement of the MW group, I resort to the Health and Retirement Study (HRS), a longitudinal survey that is representative of Americans over the age of 50 (and their spouses). The HRS also has detailed information on wealth holdings and labor force participation. In contrast to the

---

[20]The algorithm is as follows: I run the regression for each of the five implicates, using the 999 replicate weights to bootstrap the standard errors. The average of the errors from each of the five regression gives the sampling variance, while the variance of the coefficient estimates from the five implicates is the imputation variance. See Kennickel (1998) for a detailed discussion.

SCF, it is meant to be a representative study of older-age Americans, not to capture the distribution of wealth. As such, it contains few very high MW individuals, but more HW than the SCF. In addition, since the HRS sample is randomly selected and not capturing wealth, it is much easier to construct the MW group.[21] The HRS began in 1992 and is conducted every two years, and I use the waves from 1994 to 2000 (wave 2 to 5).

For simplicity, I focus on males and drop female-only households. This is due to the fact that female labor supply was typically much lower for the demographics and the era under study, but also was on the rise, and so the DiD assumption would be much more strict in this case. I further restrict the sample of interest to be aged between 50 and 80, as, on the one hand, I want to focus on individuals with a high number of work year, who were thus more likely to engage in estate planning, but, on the other, were expected to live through 2006 (when the full provisions of TRA97 would have been in effect). Summary statistics are given in Table 1.2, where I include a column of unweighted means for the SCF, to illustrate the need for weighting.

1.7   Results

Consider first the results from the SCF sample. Recall that this is used mostly for the HW group, due to the small sample for the MW group. The results are shown in Table 1.4, and confirm the central hypothesis of this exercise. Four major themes

---

[21]Intuitively, because the wealth distribution in the SCF has extremely fat tails, the distribution of group sizes is hump-shaped, while in the HRS it is single peaked.

emerge.

First, notice that the coefficient for MW in 2000 is robust in magnitude and negative and significant across all models, including model (1), which has no covariates expect age. Once covariates are included, the coefficient for HW in 2000 becomes significant and rises in magnitude. Second, for 1997, the coefficients for both groups are negative, and smaller in magnitude that in 2000, suggesting that there was perhaps some response to the news of the impending tax change, but the coefficients are not significant. One possible explanation for this are administratives issue involved with leaving the labor force, and one would expect a certain lag to occur before some changes take effect. In any case, the coefficient in 1997 is not statistically significantly different from 2000, and still quite sizable. Third, the uninteracted coefficients for both groups are positive and significant, indicating that participation was higher compared to the control group before the tax change, suggesting the reform was significant in altering participation patterns. Finally, the coefficients for MW after the reform are consistently larger in magnitude (more negative) than for HW. This confirms the original hypothesis that although MW faced, in theory, opposing price effects, the wealth effect is more important than for HW, since the tax gain was a higher fraction of pre-reform net-of-tax wealth. Keep in mind though that HW coefficients certainly suffer from small sample bias.

The covariate estimates are more or less in line with what theory would suggest. Age is naturally highly negatively and significantly related to participation, as is bad health (a categorical variable with four values, from excellent to poor health). More educated men are likelier to be in the labor force compared to individuals who did

not finish high school (the omitted category), and so are married men compared to their single counterparts. Interestingly, while participation is rising in the number of children living at home, the coefficient for the number of children away from home is negative (though not statistically significant). Receipt or expectation of inheritance have no effect on the decision to participate. The coefficient for the fraction of net worth in unappreciated capital gains is positive and significant. Recall that capital gains tax was reduced in TRA97 as well, so this might indicate that the substitution effect dominates for capital gains, but it is well beyond the scope of this paper to make any value judgments concerning capital gains taxation. See section 1.8.3 for a formal test regarding capital gains.

As already mentioned, married individuals can double their exemption with elementary estate planning.[22] As such, a potential issue with my strategy is that once I assume this is the case, then it is likely that the treatment groups are not well defined. For instance, the MW group consists of single individuals with projected wealth ranging from $600,000 to $1 million, while for married individuals the range from $1.2 million to $2 million. The LW, on the other hand (the one omitted in the regressions), includes men with projected wealth from $300,000 to $600,000 for singles and up to $1.2 million for married. Simply controlling for marital status might not be enough, as the construction of the groups in the first place might be problematic.

In order to address this drawback, Table 1.5 presents results of running the

---

[22]What is required is for the first decedent of the couple to die to leave all the estate to their spouse, except an amount equal to the exemption, which given to the heirs tax-free. Then when the spouse die, the rest of the estate is passed on to the heirs, with the exempt amount untaxed, hence doubling the exemption.

same regressions as in Table 1.4, but only for the married sub-sample, thus ensuring the groups are well defined. The coefficients for the HW groups are larger in magnitude and the errors are smaller, while those for the MW groups are essentially unchanged. This makes intuitive sense, as it is the HW that mostly suffer from such error. As the groups are much better defined in this formulation, all following estimation will focus on the married subsample. Overall, the coefficient in the year 2000 was approximately -0.10 for the HW group, indicating the policy change led to a rise in the probability of being outside of the labor force of approximately 11 percentage points, or an approximately 13% rise from the baseline, a sizable number. To put this number in perspective, it is helpful to juxtapose it with reports from other studies. Stock & Wise (1990) and Lumsdaine et al. (1994)[23] report that a $5,000 increase in the value of pension wealth leads to a rise in the retirement hazard of 11.1% for individuals between 55 and 64. In addition, in a study closely related to the present paper, Brown et al. (2010) find that receipt of $100,000 inheritance raises the probability of retirement by 10-20% relative to the baseline, for individuals close to retirement. These numbers are not directly comparable with mine (more below) but they are indicative of the large responses of individuals towards retirement incentives.

Consider now the HRS sample. As previously mentioned, I use the HRS to overcome the problem posed by the small sample in the SCF for the MW group. An additional benefit of the HRS is that, because it is a longitudinal survey, it allows me to incorporate fixed effects, which can overcome unobserved heterogeneity. On the

---

[23]As reported in Gruber & Madrian (1995).

contrary, the HRS has very few individuals in the HW group, much fewer than the SCF for the MW group, a consequence of the nature of the survey (representative of individuals over 51 and their spouses). As such I will not use the HW group in my estimates using the HRS.

Focusing only on the married subsample, Table 1.6 gives the results of the same model as Table 1.5 (pooled OLS), but using the HRS sample. As one can see, the estimates are broadly and qualitatively similar. The interaction coefficients are less precisely estimated, but still show the same pattern across specifications. The estimates are not significant for the last year, but this is actually sensible. For one, the estimates are not statistically significantly different between waves 4 and 5. Second, recall that the HRS is a longitudinal sample of elderly individuals. As such, average age increases in each successive wave, and by the 5th wave (the last wave used), the average age in the sample is 65. It is a stylized fact that retirement rates spike at 65, and so we should expect a large fraction of either group to leave the labor force at 65 regardless of treatment. The higher average age in the HRS is also the reason why the absolute magnitude of the coefficient is smaller compared to the SCF. The proportional change though is quite similar, at around 13% compared to the baseline.

Table 1.7 shows the results of the basic regression from the HRS sample, but now exploiting the panel nature of the data, adding fixed effects at the individual level. The interaction terms are now much more precisely estimated than previously, and slightly larger in absolute magnitude compared to the pooled OLS model. This reinforces the message of the previous table, and indicates a significant reduction in

participation as a result of the policy change.

1.7.1   Other margins

A standard finding in the labor supply literature (see e.g. Keane 2011, Blundell & MaCurdy 1999) is that individual responses tend to be located on the extensive margin (participate or not participate), rather than the intensive margin (how many hours to work). The indivisibility of labor or fixed costs of work are typical explanations for this phenomenon. On the other hand, the population near retirement is also much more likely to work part-time than the prime-age adult population, typically defined as 25 to 54 years of age (Friedberg 2000). It is then likely that the intensive margin is non-trivial for the population under examination.

A standard issue arises when estimating limited continuous dependent variables, such as hours, concerning the estimation procedure. As is well known, corner-solution (or censored) variables have a mass at zero, which renders least squares inconsistent. While it can be shown that this can indeed by a significant problem for continuous regressors, the regressors in this setting are binary. If we abstract from covariates and consider the simplest case of a simple difference in means between control and treatment groups, then least squares gives us a causal estimate of the treatment-on-the-treated. In the present case, this is equal to the difference of the product of the means conditional on participation and the probability of participation, as shown in Angrist (2001).[24] Furthermore, the alternatives of generalized

---

[24]Using the Rubin Causal Model (RCM) language, this can be written as the difference in $E[Y_i|Y_i > 0, D_i] \times P[Y_i > 0|D_i]$, where D denotes treatment.

Tobit models have serious shortcomings; their reliance on strict distributional and functional form assumptions notwithstanding, there is a conceptual issue that Angrist (2001) notes. Namely, Tobit models have an implicit latent variable specification, which means that some people's desired hours are negative, which is intuitively problematic. The two-part model of Cragg (1971) is an attempt to overcome both of these problems with models, by separately estimating the participation and effort equation, but it suffers from another problem. As shown by Angrist (2001), the "conditional-on-participation" estimation has a fundamental flaw in that as it does not give a causal estimates of the treatment effect, as it compares different groups.[25] The two-part model falls victim to this problem. Since the regressors of interest are binary, I will focus on least squares with the dependent variable in levels. The results are given in 1.8 for both datasets, and broadly match the participation results.

Finally, it is also interesting to consider the effect on earnings. The income tax literature has shifted to estimating the response of taxable income, and not simply participation or hours, to tax changes, as a broader measure of the response of individuals to taxes (Saez et al. 2012). The argument is that individuals can also respond on the margin of effort, avoidance, or evasion, and so the elasticity of taxable income or earnings (ETI) with respect to the net-of-tax rate is a more complete metric of behavioral responses. Here, an-ETI like measure is quite difficult to define conceptually. In the income tax literature, ETI measures the response of taxable income to the net-of-tax rate, as opposed to gross income, precisely to

---

[25]Intuitively, even in an experimental setting, the conditional on participation approach is problematic because the treatment and control groups have fundamentally different probabilities of participation, if the treatment is to have any effect. This gives rise to a selectivity-bias problem akin to the standard problem arising with observational data.

account for evasion and avoidance. However, taxes on estate taxation are not paid until death, and so a measure of taxable estate would be a misnomer. As such, the more appropriate measure would be that of the marginal propensity to reduce earnings per dollar of unearned income, or MPE (Imbens et al. 2001).

The limitations of the DiD approach are more stark here. The variance of earnings, both within and across groups, is much larger than the variance of participation or hours, which are naturally bounded. This is more so for the HW group, especially since the timeframe under study was characterized by high growth and a disproportionate rise in top incomes. Furthermore, this rise in top incomes, to the extent that high wealth is correlated with high incomes, is a confounding factor in my study; even if my hypothesis is correct and individuals do respond in the way described, then the rise in top incomes creates similar income and substitution effects. However, this is only a problem if the trends differ during the timeframe that I study. As Piketty & Saez (2003) (working paper version) note, the share of income accruing to the 90-95 percentiles of the wage income distribution was almost completely flat throughout the 1990s; the 95-99 group shows a small rise but is flat after 1995 (less than 2% rise). Higher fractiles show larger gains, but this is concentrated at the very top - for instance, the share of the top 0.1% rose by almost 50% from 1995 to 1999. The very top is not expected to have been affected by the reform[26]. By contrast, the 99-99.5 group showed a mere 6% rise. In addition, the income effect of higher earnings will occur only after the earnings have been achieved, while the

---

[26]In the following section, I conduct a robustness check where I drop individuals with over $5 million in expected wealth - if they were driving my results, it would be suggestive of spuriousness.

substitution operates before (it is the opportunity cost). As such, it seems natural that rising top incomes would, if anything, bias my results upwards. [27]

Since the magnitude of interest is the MPE, log transformation of the dependent variable would be the standard choice. Logarithmic transformations are often used for earnings regressions, as they provide a convenient shorthand to interpret coefficients: the interacted DiD coefficients give the percentage change of the treatment over the control group as a result of the policy. The problem in this case is that using logs automatically drops the individuals who do not participate in the labor market, which is problematic because, on the one hand, it suffers from the conditional-on-participation problem outlined above, and, on the other, because the sample is significantly reduced. This is likely to be a problem in this setting.

As an alternative to a log specification, I estimate a Poisson regression. Though Poisson is more frequently associated with count data, it was proposed by Santos Silva & Tenreyro (2007) as an alternative to using logs.[28] Results are given in Tables 1.9. As hypothesized, the estimates are a lot less precise in the SCF compared to the HRS, though the magnitudes are similar, and they accord very well with those from the participation and hours regression. In addition, the estimate I get for the wealth shock is around -0.2, very much in line with typical estimates for the MPE. The smaller estimate for HRS in the final year is here, too, a result of the

---

[27]Note that since the rise in top incomes at the time had been secular, not a result of a shock, standard models of intertemporal labor supply, as in Macurdy (1981), would predict a smoothed lifecycle response, already built into the labor choice.

[28]Poisson regression is essentially a restatement of a log regression by means of a monotonic transformation. Instead of estimating a model of the form $\log y = X\beta + u$, one can simply take an exponential transformation and estimate $y = exp(X\beta + u)$. Note that Poisson assumes that the variance is equal to the mean, and so robust standard errors are necessary to obtain consistent errors.

fact that the HRS sample gets progressively older in later waves, and the differences between the groups become smaller, regardless of the policy change.

1.8 Robustness Checks

1.8.1 Data limitations

Since the validity of the preceding discussion rests firmly in the accuracy of the wealth projection method, it is important to check that it sensible. One way of doing that is to run the model using treatment and control groups constructed by current net worth. The obvious downside of this simple technique is that a lot of information is lost, and that the construction of groups might be misleading. It should be less of an issue for the high wealth group, as the sample of HW in this approach is fully nested in the corresponding sample of the wealth projection method (since the wealth projection method increases average wealth, so any person who is in HW with current wealth will by construction be in HW with project wealth). The first column of Table 1.10 shows the results of running the model with the full set of covariates, for the married subsample. As expected, the coefficients for MW, though negative, have extremely high errors. However, the coefficients for HW in 2001 are significant in both regressions, and broadly similar to those of the projected wealth model, suggesting that the projected wealth approach is suitable for this exercise.

Another potentially problematic data issue is related to the stratified and limited nature of the data. Since the sample size for the whole SCF is around 4,000-5,000 households per year, and the scope of of the survey is such that a tremendously

large number of information is needed to construct appropriate wealth measures[29], the authors of SCF construct, using complex sampling techniques, five imputations per households. It is worthwhile to make sure that the results are not driven by errors in the construction of imputations.

Due to the sheer size of the survey, only 15% of households have no imputations in the variables used in the regression analysis. Running the models using only those households (380 observations) yields qualitatively similar results, in terms of sign and significance, for the HW group, but the coefficient naturally suffers from small sample bias. The MW group, as expected, vanishes almost completely, a result of the sampling process and the creation of the groups, a problem already alluded to earlier. A better solution is to simply focus on the households for which imputations of the underlying variables do happen, but do not change the group assignment - that is, a given household belongs to the same group (LW, MW, HW, or has projected net worth less than $300,000) in each imputation. This shall be conveniently referred to henceforth as the non-imputed subsample. Running the model for the non-imputed sub-sample (for married men) yields very similar results as the full sample for the HW group. The MW sample is still considerably smaller and thus has much higher errors, as expected, and the MW coefficient interacted with survey year 1998 is actually positive (but almost zero). Note that there is still a statistically insignificant discrepancy in coefficients across the five imputations due to the small difference in weights across imputations. Results are shown in the first

---

[29]More than two hundred variables are needed to construct just the net worth measure. For more information, see Kennickel (1998). The code to construct the wealth measures was taken and adapted from www.federalreserve.gov/pubs/oss/oss2/bulletin.macro.txt.

column of Table 1.13.

Finally, another issue relating to the stratification of the SCF is that most of the sample in the SCF is comprised by individuals so wealthy that are unlikely to be have tax gains significant enough to be affected. In the unweighted sample of the married individuals in the HW group (where the cut-off is expected wealth of $2 million), median expected wealth at death is $9.1 million, with the mean being $29 million, as a result of the very fat tail characterizing the distribution of wealth. The wealth gain of $306,000 (for married individuals) due to the tax change is unlikely to much affect those individuals, so a concern arises as to whether the result is driven by their behavior. If so, this would be evident of a spurious result and cast doubt on the validity of the estimates. As all regressions are weighted, this should be less of a concern, but it is still worthwhile to check the robustness of the estimation.

The second column of Table 1.13 shows the results of a regression of the model for the married subsample only for those households whose expected wealth at death is less than $3 million. Here also I only consider the households that have the same group assignment across imputations. The reason for doing so is that imputations are designed with the full universe of observations in mind, and as I reduce the sample imputation bias rises. The estimates for the HW group are very similar to those of the whole sample, and actually more negative, which makes intuitive sense, as the tax gain is much higher (in relative terms) for the group between $2m and $3m, than for the over $3m. The MW group thus has the same problems as mentioned previously in this section, and the coefficients are the same as those of the non-imputed subsample, as the wealth restriction does not affect the MW group.

As the HW group is is quite broad, it is interesting to further explore inside that group. A straightforward way to do so in this context is through interactions with the wealth variable. As such, I run the main model again, this time adding a full set of interactions of wealth with the year dummies, the group dummies, and the group-year interaction, as well as wealth in levels. Results (not shown) indicate small and not statistically significant coefficients for all the wealth interaction terms.

1.8.2 DiD assumption for the HW group

The identification strategy employed in this paper is difference-in-differences. The validity of this approach requires that the treatment and control groups would not have experienced different underlying trends before and after the policy was enacted, in the absence of the policy (common trends assumption). In the case at hand, I require, for instance, that the HW group was not on a downward trend in participation compared to the LW group. If this did not hold true, then the estimates would merely reflect this difference in underlying trends, rather than the effect of the policy.

The usual method of testing for the common trends assumption is to check for differential trends before the policy was enacted. If I can see that there is no change in differential participation before the policy was changed, this would be evidence of the validity of the assumption. As such, data on two (or more) pre-treatment periods are needed. The HRS does provide such data, and so the test is straightforward for the MW group. One issue with using earnings as a classification

42

scheme for participation is that earnings in the HRS are not reported for the wave year, but for the previous calendar year. This poses a problem when testing the DiD assumption in the HRS: we need two pre-treatment periods, but earnings data for the 1994 wave correspond to 1993, a year of rising income taxes, which can potentially confound my estimates. As such, the DiD test has to be conducted using self-reported definitions of participation. The results of this test are show in 1.11 and show that, indeed, the common trends assumption is satisfied, since the coefficient in the 1996 interaction term is zero.

I still need to check for common trends in the HW group, for which HRS provides a negligible sample. This is not possible using the SCF dataset: changes in labor income taxes in 1991 an 1993 imply that the 1989 and 1992 will not provide clean identification and use of other surveys is warranted, in order to obtain data for the period 1993-1996. The best source of labor supply data in the United States is the Current Population Survey (CPS), and specifically the CPS March Supplement. The downside of the CPS is that it has no information on assets, though it still preferable to other similar surveys, such as the the Panel Study of Income Dynamics (PSID), due to its large size (and hence the high number of wealthy individuals included).

The challenge is to identify the groups affected by TRA97 in the CPS sample, and construct them in a manner similar to the one shown here. In the absence of assets, I have to resort to incomes. Using the expected wealth measure and breaking the population in 20 quantiles, the median household in the LW group in the 1995 SCF (1994 calendar year) for the subsample of households where the male head is

between 50 and 80 years old is in the 13th quantile (65th income percentile), so I include the 11th-15th quintiles (55th to 75th percentile) in the LW group. Similarly, the second decile (85th and 90th percentiles) comprise the MW group, and the top 10% is the HW group.

Table 1.12 shows the results of this regression, for the married sample, for the period 1993-2000. The group dummies are highly positive and significant, and the group-year interaction dummies for 1993-1996 are all insignificant, indicating that the common trends assumption is valid. Furthermore, I manage to replicate the post-1997 change in participation for both groups, ensuring that the CPS provides a proper alternative to check the common trends assumptions. The picture is somewhat less satisfactory for the MW group, for the very same reasons as for the SCF, since it is by construction less well defined.

The treatment coefficients are significant for the HW group, though smaller in magnitude than the SCF - approximately -6% over the baseline in the CPS versus around 13% in the SCF for the year 2000. There are likely two reasons for this. First, and more obviously, as the groups in the CPS are constructed based on income, the treatments groups contain individuals that were not actually treated and should not have had a response, and so it is sensible that the magnitudes are smaller. Second, the HW group in CPS is by construction composed of individuals with higher participation than in the SCF (as they have higher income). Similarly, the HW group in the SCF differs from that in the CPS in containing individuals with (weakly) lower incomes, so the average price of leisure (the wage rate, conditional on hours) is lower for them, and it is more likely that the the negative income/wealth

effect they face will lead to a reduction in participation.

Similarly, the SCF classification includes in the MW group relatively more individuals with lower current income than the MW group in the CPS, who face a lower price for leisure and are thus more likely to have larger income than substitution effects.[30] Another consequence of this classification is that the results are almost identical between the full sample (not shown) and the married subsample. Finally, recall that the common trends check was successful for the MW group using the much better suited HRS sample.

Furthermore, the research design is quite different from the SCF, and the control variables non-nested. For instance, the CPS does not have data on the total number of children of individuals, only of those living at home, which is why I use both children and grandchildren in the household as controls. In addition, the sample in CPS in younger (approximately 58 years of age on average, versus 62 in the SCF) with fewer high school dropouts and more college dropouts, so slightly fewer college graduates. Nevertheless, this test is only meant to be indicative and measure rough trends, not have a causal interpretation, and as such these issues are of minor importance.

---

[30]The MW group in the CPS is only comprised of individuals in the second decile of the income distribution, while the MW in the SCF also includes people higher and lower than the second decile. But since the second decile is towards the tail, there exists a much larger mass to its left than to its right, so SCF has a larger proportion of middle wealth/low income households than the CPS.

1.8.3 Further robustness checks

A few additional robustness checks are worth considering. First, the more interesting question this paper seeks to address is whether the reduction in the estate tax led to earlier retirements. In the absence of panel data this is not directly answerable, but I can look at the labor supply behavior of the more relevant group, those aged between 60 and 70 years of age. This provides also a further robustness check: ex-ante, theory would suggest higher responses for those aged between 60 and 70 than for those 50 to 60 and 70 to 80. The former group is still considered of prime working age, and would not even be eligible for early Social Security benefit if they retired before the age of 62. The latter group already consisted mostly of retirees even before the reform, while those individuals who still were in the workforce past age 70, and who thus experience is an actuarial loss in terms of the presented discounted value of their Social Security receipts, are likely to have very high taste for work.

As such, there would be a suspicion of spuriousness to the results if I did not observe larger responses for the 60-70 group, than for the 50-80 group. Column 1 of Table 1.14 presents the results for this subsample. Indeed, responses are larger for both groups, and much larger for the MW group, though not significant. The coefficient for the HW is still significant for the year 2000, though there is much more noise than before, a result of the significant reduction of the sample size by almost 65%. Overall the pattern of the results is the same as in all other models.

Second, it is important to ascertain that the definition of the dependent vari-

able is meaningful. The income groups under analysis are hardly typical workers or retirees, and since the labor supply variable is based on survey data one should be cautious about how to interpret it. For instance, a former finance professional might be officially retired but still be engaged in day-trading. Conversely, a retired law professor could be serving in a non-executive ceremonial position in the board of directors of an institution or a corporation, and receive non-trivial remuneration for very few hours of work.

The former does not seem to pose a problem as the SCF designers accounted for this potential complication. All individuals who are recoded by the interviewer to be out of the labor force have zero of work (I utilize the recoded labor supply questions). To control for the latter, I repeat the analysis replacing the self-reported work status with a binary indicator equal to 1 if the respondent works at least 1000 hours per year (roughly 20 hours per week). Column 3 of Table 1.14 presents the results of this estimation, and the coefficients of interest change little.

Third, it is important to make sure the control group is well defined. It would be problematic if individuals within the control group were experiencing differential employment trends. A straightforward way to test this is to conduct a placebo treatment test. I split the control group into two groups: those with expected wealth at death between $300,000 and $425,000 if single ($300,000/$750,000 if married), and those with expected wealth at death between $425,000 and $550,000 ($750,000/$1.1m if married). The second group is the "fake" or placebo treatment group - if the year-fake treatment interaction terms are not zero then the control group is not well defined. Column 4 of Table 1.14 presents the results of this regres-

sion. The relevant fake dummy - year interactions have both p-values of over 50%, indicating the control group is well defined.

Finally, as previously mentioned, TRA97 also included a broad reform of capital gains taxation. First, the law reduced the marginal tax rates from 28% to 20% and from 15% to 10% (for assets held at least 18 months, longer otherwise). Second, an exclusion of $500,000 dollars on the capital gains incurred from the sell of principal residences replaced a more complex system that featured an exclusion of $125,000.[31] The extent to which capital gains taxes affect work decisions is unclear and not the object of this study. However, it is clear that capital gains taxes do affect capital gains realizations. A classic example of that is the surge in capital gains realizations in 1986, as individuals scrambled to sell appreciated assets before the new higher rates of TRA86 kicked in the following year. It is thus useful to control for any such behavior. Similar complications arise from the fact that the period under studied was characterized by a stock market rally. Table 1.15 shows several extensions of the basic model to explore these issues. In all cases the results remain robust.

Note that using unrealized capital gains for the period after the reform is hardly ideal, as realizations are likely to have already taken place (short term responses are typically larger than long term responses for capital gains). The nature of the data does not allow us to do this, but note that the (weighted) mean fraction of wealth in all unrealized capital gains in the sample falls from 31.6% in 1997 to 26.9% in

---

[31]Anecdotally, this feature of the tax code was blamed for the subsequent housing bubble of the 2000s.

2000^{32}, consistent with the aforementioned stylized fact. In any case, the addition of those variables is only meant as a control for possible confounders, and it seems that the capital gains tax reform does not change the results.

1.9 Discussion and Conclusion

Just like income taxes, changes in bequest taxes have income and substitution effects. In this paper, I provide the first empirical examination of such effects. I use a difference-in-differences identification strategy, exploiting the Taxpayer Relief Act of 1997, which raised the initial exemption to the taxation of bequests by two thirds. I find a statistically significant effect, with participation falling by around 10% relative to the baseline. As a corollary, I also find that the wealth effect (which reduces labor supply in this case) is stronger than the price effect (which would raise it).

The findings in this paper are consistent with a small substitution effect, a common finding in the labor supply and tax literature for men, and a sizable wealth effect, an outcome of the unique structure of the bequest tax. As income taxes, on which the labor supply literature typically relies, has negligible wealth effects, the more relevant literature to compare these results is the literature that has looked at the response of individuals around retirement age to wealth shocks, which was analyzed in Section 1.3.3. The more relevant benchmark comparison is with Brown et al. (2010), who look at the effect of unanticipated inheritance on the labor supply

---

[32]This is even more striking given that in 2000, the financial markets were at the peak of the dot com bubble

of the recipient. They find that an extra \$100,000 of inheritance raises the probability of retirement by 12.5% relative to the baseline. The wealth effects are larger than this for both groups affected by TRA97, so my estimate is somewhat smaller than for Brown et al. (2010), albeit not directly comparable.

The auxiliary specification with a Poisson regression with earnings on the left hand side, instead of a binary for participation, yielded a marginal propensity to earn out of unearned income (MPE), which gives the reduction in earnings for a one dollar rise in unearned income, of -0.19. A related paper to assess how this result fits into the literature is the work of Imbens et al. (2001), who analyze a panel sample of lottery prize participants in Massachusetts, matched with earnings data from Social Security for six years before and after participation in the lottery. They regress log earnings on an annualized measure of lottery payments, and find an average MPE of -0.17 for the 55-65 age category, a number very close to my result.

The contribution of this paper is the identification of this previously undocumented margin of response. It follows that the estate tax can be used as a policy instrument in this way as well. The policy implications of the finding depend on what the problem or goal is. First, regarding labor supply, one might want to encourage higher participation/delay retirement of upper-middle class professionals at the last stage of their work-cycle. Higher participation implies more social insurance contributions and fewer pension checks. On the other hand, it is plausible that the government might want to reduce participation of these groups. For example, increased automation and advances in medicine may mean that the top workers will have longer work-lives than middle workers in the future, and encouraging earlier

retirement may be inequality-reducing.

Second, it is important to note that the individuals affected by the policy change used in this paper are those just below the top of the wealth distribution. In his recent influential book, Piketty (2014) uses the term "petits-rentiers", or small rentiers, to define a group that has significant wealth holdings to be considered wealthy, but not enough to completely live-off capital income, and roughly defines this group as having wealth of around $1 to $3 million. The interesting empirical observation that he makes (using French data) is that this group is larger currently than at any other point in history. Concretely, the metric he uses is the fraction of individuals (broken down by year of birth), who inherit at least as much as the average lifetime income of the average individual in bottom half of the income distribution. For the cohort born between 1970 and 1980, this figure stands at 14%, much higher than earlier in the 20th century, and slightly higher than any point in the 19th century. This is then an increasingly large and politically important group, and, notably, since it gets incomes from labor, not rents, the correlation of income and wealth rises with the size of the petits-rentiers. As such, it seems plausible that within a few decades we may end up with an upper class of many small estates, as opposed to one with a non-trivial, but nevertheless small number of very large estates. In that case, the $90^{th} - 99^{th}$ percentiles of the wealth distribution will hold and bequeath a substantial amount of total national wealth, and, the estate tax can then be a useful tool in reducing wealth inequality.

Finally, as previously mentioned, we do have some evidence that wealth accumulation responds to estate taxes. However, this reduction in accumulation could

be either (for given rate of return) a fall in savings , which becomes comparatively cheaper, or a reduction in labor supply. The research design laid out in this paper could have been useful in looking at this other dimension as well, but unfortunately neither of the datasets used have information on consumption. More importantly, the ultimate interest is on whether the reform affected bequests, and hence wealth. The arguments laid out in this paper show that this could have happened through i) a reduction in labor supply and hence a reduction in wealth ii) a substitution effect on savings, raising wealth and iii) an income effect, which raises consumption and reduces wealth.[33] Conceivably, such a question could be answered in a few years, when the individuals affected by this policy change have died and made bequests, and administrative data on such bequests could be used. The successive changes in the estate tax in the 2000s, which were initially accompanied by simultaneous income tax cuts and were fully anticipated initially make identification very difficult, but the present framework could be used in the future, perhaps using another country as a case study.

---

[33]This is for national wealth (including the government), as dynastic wealth will rise as long as consumption does not rise by more than one for one.

Table 1.1: Marginal Estate Tax Rates for 1997

Estate Value			Tax Rate (%)	Estate Value			Tax Rate (%)
0	–	10	18	500	–	750	37
10	–	20	20	750	–	1,000	39
20	–	40	22	1,000	–	1,250	41
40	–	60	24	1,250	–	1,500	43
60	–	80	26	1,500	–	2,000	45
80	–	100	28	2,000	–	2,500	49
100	–	150	30	2,500	–	3,000	53
150	–	250	32	3,000	–	10,000	55
250	–	500	34	10,000	–	21,040	60
				over		21,040	55

Source: McGarry (2000).

Notes:

1. Estate Values are in $1,000s.

2. The top rate of 60 for those between $10 million and $21.04 million was introduced to offset the lower rates applied to smaller amounts. The upper cut-off was lowered to $17.184 million in 1998 (McGarry 2000).

3. The exemption is officially a tax credit. For instance, in 1999, when the credit was $211,300, making the first $650,000 untaxable. That is why the schedule maintains brackets up to exemption, even though they are actually not operative.

Table 1.2: Summary Statistics

	All			Men aged between 50 & 80, Wealth>$300K		
	Weighted		Unweighted	Weighted		Unweighted
	μ	σ	μ	μ	σ	μ
Age	48.72	17.24	49.95	62.61	8.49	62.02
No high school degree	0.172	0.378	0.12	0.064	0.245	0.044
High school degree	0.31	0.462	0.243	0.223	0.417	0.139
Some college	0.173	0.378	0.155	0.146	0.353	0.127
College degree+	0.346	0.476	0.482	0.566	0.496	0.690
Labor Force	0.737	0.440	0.775	0.621	0.485	0.737
Head has bad health	0.055	0.228	0.039	0.032	0.176	0.021
Head of HH is married	0.807	0.395	0.842	0.906	0.292	0.913
HH has children at home	0.476	0.499	0.461	0.25	0.433	0.256
HH has children total	0.804	0.397	0.837	0.931	0.253	0.643
HH NW (in $1000)	315	1783	6009	1433	4446	16029
HH NW (median)	80.58		218.16	600		2574
HH income (in $1000)	59.1	209.8	439.8	135.4	533.74	1064.5
HH income (median)	38		59	74		195
% of NW in capital gains	0.268	0.263	0.308	0.332	0.231	0.388
R inheritance	0.177	0.382	0.223	0.372	0.483	0.367
AMT if R (in $1000)	71.1	325.6	347.1	142.3	586.7	561.7
AMT if R (median)	20		45	60		100
HH made gifts	0.13	0.336	0.194	0.242	0.428	0.331
AMT if gave (in $1000)	5.9	15.4	23.5	12.6	28.6	37.2
AMT if gave (median)	2.5		7	6		15
Year =1994	0.304	0.460	0.328	0.257	0.436	0.325
Year =1997	0.329	0.468	0.324	0.312	0.463	0.316
Year =2000	0.371	0.483	0.348	0.431	0.495	0.358

Table 1.3: Deciles of Current and Projected Wealth (SCF, in $1,000s)

		Decile 1		Decile 2		Decile 3		Decile 4		Decile 5	
Year		Cur	Proj	Cur	Proj	Cur	Proj	Cur	Proj	Cur	Proj
1994	mean	-1.03	26	26	69	59	174	84	188	127	295
	std. dev.	113	113	10	30	7.5	64	9	54	14	62
	med	1.47	27	26	64	60	159	82	180	127	286
1997	mean	-0.04	20	30	82	69	211	103	278	151	344
	std. dev.	28	29	11	44	11	83	11	67	17	79
	med	1.01	20	30	73	72	190	102	272	150	336
2000	mean	2.73	27	36	99	78	241	129	369	192	461
	std. dev.	14	20	10	54	13	94	19	95	19	94
	med	2	24	35	84	7 6	219	127	360	191	465

		Decile 6		Decile 7		Decile 8		Decile 9		Decile 10	
Year		Cur	Proj	Cur	Proj	Cur	Proj	Cur	Proj	Cur	Proj
1994	mean	174	400	235	573	348	806	565	956	2869	3565
	std. dev.	13	78	24	121	37	153	103	173	6668	6674
	med	174	397	235	566	343	807	545	961	1310	2077
1997	mean	215	471	305	605	421	828	710	1017	3927	4464
	std. dev.	22	97	27	114	44	127	149	182	9239	9240
	med	211	470	304	589	415	837	670	1003	1954	2570
2000	mean	272	561	393	660	596	899	1020	1268	5422	5849
	std. dev.	25	129	42	125	82	136	182	223	9662	9669
	med	268	546	395	642	580	887	1000	1249	3165	3584

Summary statistics on current and projected wealth at death for the SCF, by decile. Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death.

Table 1.4: OLS Regression of Labor Force Participation for All Men Projected Net Worth (SCF)

	(1)		(2)		(3)	
	β	s.e.	β	s.e.	β	s.e.
MW	0.0777	0.0504	0.0765*	0.0461	0.0773*	0.0441
HW	0.1240***	0.0433	0.1034**	0.0402	0.0968**	0.0415
MW* Year=1997	−0.0294	0.0796	−0.0421	0.0745	−0.0404	0.0736
MW* Year=2000	−0.1210*	0.0720	−0.1278*	0.0676	−0.1271*	0.0683
HW* Year=1997	−0.0289	0.0542	−0.0308	0.0531	−0.0308	0.0526
HW* Year=2000	−0.0666	0.0455	−0.0865**	0.0421	−0.0872**	0.0420
Age	−0.0590***	0.0159	−0.0536***	0.0168	−0.0545***	0.0168
Age Squared	0.0002	0.0001	0.0002	0.0001	0.0002	0.0001
High School			0.0410	0.0409	0.0486	0.0402
Some College			0.0624	0.0434	0.0713*	0.0416
College			0.0739*	0.0385	0.0871**	0.0376
Married			0.0910***	0.0285	0.0859***	0.0296
Health Bad			−0.0813***	0.0108	−0.0811***	0.0108
Kids Away			0.0039	0.0055	0.0036	0.0055
Kids Home			0.0142	0.0100	0.0110	0.0099
R Inheritance					0.0028	0.0038
Exp Inheritance					−0.0008	0.0059
% NW in KG					0.0871	0.0474
Year=1997					0.0391	0.0257
Year=2000	0.0784***	0.0225	0.0814***	0.0219	0.0801***	0.0219
R^2	0.3792		0.4058		0.4094	
Observations	18640		18640		18640	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death. The third models includes income, equity holdings and net worth, and all are zero. All models include all five imputations and are calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5} * \text{imputation variance} + \text{sampling variance}$.

Table 1.5: OLS Regression of Labor Force Participation for Married Men
Projected Net Worth (SCF)

	(1)		(2)		(3)	
	β	s.e.	β	s.e.	β	s.e.
MW	0.1174**	0.0483	0.0939*	0.0479	0.0947**	0.0471
HW	0.1730***	0.0332	0.1247***	0.0332	0.1180***	0.0334
MW* Year=1997	−0.0518	0.0917	−0.0716	0.0870	−0.0675	0.0857
MW* Year=2000	−0.1249**	0.0745	−0.1347*	0.0744	−0.1334*	0.0745
HW* Year=1997	−0.0984*	0.0573	−0.0948*	0.0566	−0.0937*	0.0561
HW* Year=2000	−0.1153***	0.0417	−0.1193***	0.0396	−0.1171***	0.0396
Age	−0.0455***	0.0176	−0.0383*	0.0179	−0.0401**	0.0179
Age Squared	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
High School			0.0340	0.0426	0.0406	0.0423
Some College			0.0581	0.0440	0.0673	0.043
College			0.0791*	0.042	0.0925**	0.0416
Health Bad			−0.0797***	0.0118	−0.0797***	0.0119
Kids Away			−0.0119	0.0104	−0.0091	0.0105
Kids Home			0.0157	0.0105	0.0129	0.0104
R Inheritance					0.0024	0.0043
Exp Inheritance					0.0034	0.0063
% NW in KG					0.0802*	0.0455
Year=1997					0.0335	0.0264
Year=2000					0.0855***	0.0216
R^2	0.3790		0.4017		0.4055	
Observations	16851		16851		16851	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death. All models include all five imputations and are calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5} *$ imputation variance + sampling variance.

Table 1.6: Pooled OLS regression of Labor Force Participation for Married Men
Projected Net Worth (HRS)

	(1)		(2)		(3)	
	β	s.e.	β	s.e.	β	s.e.
MW	0.0572**	0.0226	0.0297	0.0224	0.0310	0.0223
MW* Year==1998	−0.0549*	0.0308	−0.0595**	0.0304	−0.0553*	0.0302
MW* Year==2000	−0.0486	0.0322	−0.0491	0.0317	−0.0452	0.0316
Age	−0.0508***	0.0076	−0.0510***	0.0075	−0.0476***	0.0076
Age Squared	0.0001*	0.0001	0.0001**	0.0001	0.0001*	0.0001
Education			X		X	
Health Bad			X		X	
Kids			X		X	
Inheritance					X	
% NW in IRA/Stocks					X	
Year=1996	−0.0360***	0.0106	−0.0380***	0.0104	−0.0371***	0.0104
Year=1998	−0.0447***	0.0102	−0.0391***	0.0101	−0.0367***	0.0101
Year=2000	−0.0488***	0.0107	−0.0508***	0.0106	−0.0473***	0.0107
R^2	0.3790		0.4017		0.4055	
Observations	10230		10230		10230	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death.

Table 1.7: Fixed Effects Regression of Labor Force Participation for Married Men
Projected Net Worth (HRS)

	(1)		(2)		(3)	
	β	s.e.	β	s.e.	β	s.e.
MW* Year==1997	−0.0639**	0.0263	−0.0653**	0.0265	−0.0638**	0.0265
MW* Year==1999	−0.0484	0.0306	−0.0497	0.0308	−0.0478	0.0307
Age	−0.0321	0.0216	−0.0335	0.0217	−0.0291	0.0218
Age Squared	0.0003**	0.0001	0.0003***	0.0001	0.0003**	0.0001
Health Bad			−0.0050	0.0058	−0.0047	0.0058
Kids Total			−0.0262	0.0179	−0.0271	0.0177
R Inheritance					−0.0468*	0.0251
Exp Inheritance					0.0003*	0.0002
% NW in Stocks					0.0045	0.0228
Year=1997	−0.0877***	0.0219	−0.0855***	0.0221	−0.0833***	0.0221
Year=2000	−0.1842***	0.0422	−0.1822***	0.0423	−0.1780***	0.0421
R^2	0.1353		0.1353		0.1353	
Observations	10230		10230		10230	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death.

Table 1.8: Regression of Hours Worked for Married Men
Projected Net Worth

	(1) SCF		(2) HRS	
	β	s.e.	β	s.e.
MW	257.92*	152.44		
HW	518.22***	97.48		
MW* Year=1997/8	−282.48	235.09	−123.99*	67.23
MW* Year=2000	−356.43	220.68	−168.80**	76.48
HW* Year=1997	−269.65**	138.01		
HW* Year=2000	−422.43***	112.74		
Age	−252.19***	43.33	−213.70***	61.30
Age Squared	1.30***	0.34	1.51***	0.42
High School	−2.90	93.04		
Some College	62.01	106.49		
College	123.87	98.11		
Health Bad	−169.50***	30.61	−26.53***	15.21
Kids Away	−26.68	29.20		
Kids Home	34.44	30.86		
Kids Total			−7.77	36.42
R Inheritance	6.74	9.65	−82.86	64.36
Exp Inheritance	24.33	17.86	0.91**	0.39
% NW in KG	214.77*	128.03		
% NW in Stocks			11.73	76.88
Fixed Effects			X	
Year=1997/8	58.20	73.12	−147.35***	55.35
Year=2000	178.33***	62.25	−285.13***	106.42
R^2	0.3851		0.1546	
Observations	3370		10230	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death. The SCF model includes all five imputations and is calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5} *$ imputation variance + sampling variance.

Table 1.9: Poisson Regression of Earnings for Married Men
Projected Net Worth

| | (1) | | (2) | |
| | SCF | | HRS | |
	β	s.e.	β	s.e.
MW	0.5814***	0.1934		
HW	1.0406***	0.1509		
MW* Year==1997	−0.0041	0.2344	−0.1934**	0.0827
MW* Year==2000/1999	−0.1848	0.2224	−0.0850	0.089
HW* Year==1997	−0.0875	0.2361		
HW* Year==2000	−0.1246	0.1925		
Age	0.3279***	0.0766	0.5696***	0.1071
Age Squared	−0.0035***	0.0006	−0.0043***	0.0008
High School	0.1744	0.1112		
Some College	0.2448**	0.1060		
College	0.6617***	0.1067		
Health Bad	−0.1410***	0.0594	0.0179	0.0175
Kids Away	0.0152	0.0147		
Kids Home	0.0074	0.0236		
Kids Total			0.0483	0.0449
R Inheritance	0.0161	0.0121	−0.0502	0.0692
Exp Inheritance	−0.0012	0.0181	0.0006	0.0005
% NW in KG	−0.0518	0.0803		
% NW in Stocks			0.0362	0.0526
Fixed Effects			X	
Year=1997	0.0630	0.0720	−0.1585**	0.0712
Year=2000/1999	0.1695**	0.0715	−0.3383**	0.1413
Observations	3370		10230	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death. The SCF model includes all five imputations and is calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5 * \text{imputation variance} + \text{sampling variance}}$.

Table 1.10: OLS Regression of Labor Force Participation for Married Men
Current Net Worth (SCF)

	SCF	
	β	s.e.
MW	0.0799	0.0542
HW	0.0930**	0.0420
MW* Year=1997	−0.1390	0.1194
MW* Year=2000	−0.0342	0.0706
HW* Year=1997	−0.0655	0.0531
HW* Year=2000	−0.1013**	0.0484
Age	−0.0090	0.0197
Age Squared	−0.0002	0.0002
High School	0.0427	0.0418
Some College	0.0292	0.0466
College	0.0716*	0.0394
Health Bad	−0.0741***	0.0345
Kids Away	−0.0237**	0.0107
Kids Home	0.0225**	0.0107
R Inheritance	0.0035	0.0052
Exp Inheritance	−0.0020	0.0067
% NW in KG	0.1392***	0.0424
Year=1997	0.0238	0.0319
Year=2000	0.0548*	0.0317
R^2	0.3978	
Observations	2802	

MW: Middle Wealth, current wealth $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, current wealth over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in current wealth. The model includes all five imputations and is calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5 * \text{imputation variance} + \text{sampling variance}}$.

Table 1.11: Fixed Effects Regression of Labor Force Participation for Married Men
Projected Net Worth (HRS) - DiD Test

	HRS	
	β	s.e.
MW* Year==1996	-0.0065	0.0251
MW* Year==1998	-0.0612**	0.0313
MW* Year==2000	-0.0542*	0.0337
Age	-0.0157	0.0216
Age Squared	0.0001	0.0001
Health Bad	-0.0107*	0.0055
Kids Total	-0.0209	0.0104
R Inheritance	-0.0428*	0.0251
Exp Inheritance	0.0003*	0.0001
% NW in Stocks	-0.0194	0.0191
Year=1996	-0.0703***	0.0234
Year=1998	-0.1258***	0.0427
Year=2000	-0.1600***	0.0626
R^2	0.2586	
Observations	11252	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80, with at least $300,000 in expected wealth at death.

Table 1.12: OLS Regression of Labor Force Participation for Married Men
Current Income (CPS)

Panel A - interactions

	MW		HW	
	β	s.e.	β	s.e.
Group	0.0819***	0.0111	0.0937***	0.0125
Group* Year=1994	−0.0091	0.0156	0.0063	0.0168
Group* Year=1995	−0.0092	0.0156	−0.0073	0.0170
Group* Year=1996	−0.0161	0.0160	0.0037	0.0175
Group* Year=1997	−0.0346**	0.0158	−0.0399**	0.0171
Group* Year=1998	−0.0261*	0.0156	−0.0405**	0.0174
Group* Year=1999	−0.0522**	0.0156	−0.0608***	0.0169
Group* Year=2000	−0.0405**	0.0155	−0.0540***	0.0173

Panel B - Controls

	β	s.e.
Age	0.0082**	0.0038
Age Squared	−0.0003***	0.0000
High School	−0.0054	0.0065
Some College	−0.0107	0.0067
College	−0.0163**	0.0066
Health Bad	−0.3704***	0.0082
Kids Home	−0.0037*	0.0016
Grandkids at home	−0.0148***	0.0049
Year=1994	0.0019	0.0107
Year=1995	0.0088	0.0104
Year=1996	0.0080	0.0108
Year=1997	0.0294***	0.0106
Year=1998	0.0118	0.0105
Year=1999	0.0317***	0.0106
Year=2000	0.0211**	0.0105
R^2	0.3773	
Observations	44958	

The model is a regression of labor force participation on group dummies, year dummies, group-year interactions, and controls. Panel A gives the interaction estimates, broken down for convenience into two columns, the left for the MW group, and the right for the HW group. Panel B gives the estimates for the controls. Sample is married men aged 50 to 80. The regression is weighted by the sample designed weights to account for stratification and sampling error.

Table 1.13: OLS Regression of Labor Force Participation for Married Men
Projected Net Worth (SCF)

	(1)		(2)	
	No Imputations		Less than $3m	
			in projected net worth	
	β	s.e.	β	s.e.
MW	0.0353	0.0943	0.0440	0.1037
HW	0.1315***	0.0391	0.1719	0.1170
MW* Year=1997	0.0339	0.1052	0.0232	0.1056
MW* Year=2000	−0.1522	0.1257	−0.1698	0.1264
HW* Year=1997	−0.0950*	0.0526	−0.1890**	0.0866
HW* Year=2000	−0.1326***	0.0445	−0.1728**	0.0828
Age	−0.0509***	0.0186	−0.0519***	0.0191
Age Squared	0.0001	0.0001	0.0002	0.0002
High School	0.0192	0.0399	0.0156	0.0405
Some College	0.0486	0.0378	0.0425	0.0386
College	0.0553	0.0366	0.0428	0.0371
Health Bad	−0.0796***	0.0121	−0.0759***	0.0131
Kids Away	−0.0171*	0.0102	−0.0156	0.0111
Kids Home	0.0191*	0.0109	0.0189	0.012
R Inheritance	0.0058	0.0044	0.0075	0.0048
Exp Inheritance	0.0069	0.0067	0.0055	0.0075
% NW in KG	0.1507***	0.041	0.1441***	0.0457
Year=1997	0.0536*	0.0288	0.0540*	0.0296
Year=2000	0.0980***	0.0245	0.0952***	0.0242
R^2	0.3898		0.4061	
Observations	2798		1327	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80 with at least $300,000 in current wealth. Both models include income, equity holdings and net worth, and all are zero. Both models include all five imputations and are calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5}$imputation variance + sampling variance.

Table 1.14: OLS Regression of Labor Force Participation for Married Men
Further Checks, Projected Net Worth (SCF)

	(1)		(2)		(3)	
	60-70		1000 Hours		Placebo	
	β	s.e.	β	s.e.	β	s.e.
MW	0.1825*	0.1024	0.0349	0.0514		
HW	0.1922***	0.0645	0.1323***	0.0365		
MW* Year=1997	−0.0551	0.2471	−0.1022	0.0856		
MW* Year=2000	−0.2322	0.1716	−0.1175	0.0837		
HW* Year=1997	−0.1175	0.1117	−0.1054**	0.0529		
HW* Year=2000	−0.1401*	0.0802	−0.1277***	0.0453		
Fake					0.0026	0.0401
Fake*Year=1997					0.0265	0.0578
Fake*Year=2000					−0.0062	0.0602
Age	−0.9873***	0.2706	−0.0948***	0.0162	−0.0668***	0.0212
Age Squared	0.0073***	0.0021	0.0005***	0.0001	0.0003	0.0002
High School	0.1219*	0.0622	−0.0254	0.0411	0.0293	0.0422
Some College	0.1874**	0.0761	0.0161	0.0441	0.0743	0.0465
College	0.1602**	0.0674	0.0272	0.0414	0.0671	0.047
Health Bad	−0.1060***	0.0200	−0.0717***	0.0123	−0.0926***	0.0136
Kids Away	−0.0077	0.0100	0.0006	0.0048	0.0071	0.0059
Kids Home	0.0530	0.0382	0.0077	0.0114	0.0040	0.0138
R Inheritance	0.0118	0.0084	0.0043	0.004	0.0067	0.005
Exp Inheritance	−0.0029	0.0162	0.0077	0.0063	0.0032	0.0082
% NW in KG	0.1134	0.0717	0.0599	0.04	0.0530	0.0536
Year=1997	0.0017	0.0501	0.0385	0.0244	0.0133	0.0348
Year=2000	0.1333***	0.0457	0.0869***	0.0256	0.0888***	0.0325
R^2	0.167		0.420		0.400	
Observations	1192		3370		1197	

MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men.
HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80 with at least $300,000 in current wealth. Both models include income, equity holdings and net worth, and all are zero. Both models include all five imputations and are calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5}$imputation variance + sampling variance.

Table 1.15: OLS Regression of Labor Force Participation for Married Men
Projected Net Worth (SCF)

	(1)		(2)		(3)		(4)		(5)	
MW	0.104**	0.050	0.096*	0.052	0.102*	0.053	0.104**	0.051	0.102**	0.050
HW	0.145***	0.035	0.134***	0.043	0.145***	0.036	0.144***	0.037	0.132***	0.034
MW* Year==1997	−0.087	0.091	−0.081	0.088	−0.077	0.091	−0.079	0.090	−0.091	0.094
MW* Year==2000	−0.125	0.080	−0.137	0.087	−0.132	0.083	−0.134	0.082	−0.136*	0.079
HW* Year==1997	−0.070	0.057	−0.065	0.067	−0.051	0.059	−0.050	0.060	−0.077	0.057
HW* Year==2000	−0.108**	0.042	−0.130***	0.047	−0.119***	0.043	−0.118***	0.044	−0.115***	0.042
Age	−0.047***	0.018	−0.047***	0.018	−0.047***	0.018	−0.048***	0.018	−0.047***	0.018
Age Squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
High School	0.024	0.041	0.024	0.041	0.023	0.041	0.024	0.040	0.022	0.041
Some College	0.048	0.044	0.046	0.045	0.046	0.043	0.048	0.044	0.047	0.044
College	0.081*	0.042	0.080*	0.043	0.078*	0.041	0.080*	0.041	0.079*	0.042
Health Bad	−0.084***	0.012	−0.084***	0.012	−0.085***	0.012	−0.084***	0.012	−0.085***	0.011
Kids Away	0.005	0.005	0.005	0.005	0.005	0.005	0.005	0.005	0.006	0.005
Kids Home	0.007	0.011	0.007	0.011	0.008	0.011	0.008	0.011	0.006	0.011
R. Inheritance	0.003	0.004	0.003	0.004	0.003	0.004	0.003	0.004	0.003	0.004
Exp Inheritance	0.001	0.006	0.000	0.006	0.001	0.006	0.001	0.006	0.002	0.006
% NW in KG (all)	0.113*	0.068			0.057	0.041	0.113*	0.068		
—"—Year=1997	−0.050	0.097					−0.056	0.097		
—"—Year=2000	−0.073	0.106					−0.070	0.107		
% NW in stocks	−0.269***	0.071	−0.274***	0.070	−0.267*	0.147	−0.255*	0.145		
—"—Year=1997					−0.184	0.202	−0.197	0.201		
—"—Year=2000					0.138	0.170	0.122	0.171		
% NW in KG (xh)			0.141	0.090						
—"—Year=1997			−0.060	0.119						
—"—Year=2000			0.014	0.111						
% NW in KG (h)			0.081	0.093						
—"—Year=1997			−0.036	0.130						
—"—Year=2000			−0.101	0.140						
% NW in KG (xs)									0.125*	0.067
—"—Year=1997									0.067	0.057
—"—Year=2000									0.055	0.078
% NW in KG (s)									−0.133	0.088
Year=1997	0.044	0.036	0.041	0.038	0.034	0.027	0.053	0.036	0.044	0.034
Year=2000	0.111***	0.036	0.114***	0.040	0.083***	0.024	0.106***	0.037	0.106***	0.039
R^2	0.384		0.386		0.384		0.385		0.382	
Observations	3370		3370		3370		3370		3370	

Notation: h stands for "housing", xh stands for "except housing", s stands for "stocks", xs stands for "except stocks". MW: Middle Wealth, projected wealth at death $600,000-$1,000,000 for single men, $1,200,000-$2,000,000 for married men. HW: High Wealth, projected wealth at death over $1,000,000 for single men, over $2,000,000 for married men.

Sample is men aged 50 to 80 with at least $300,000 in current wealth. Both models include income, equity holdings and net worth, and all are zero. Both models include all five imputations and are calculated using the replicate bootstrap weights, and errors are adjusted according to the formula $\sqrt{6/5}$imputation variance + sampling variance.

Chapter 2: Crime and Unemployment Insurance in the Great Recession

2.1 Overview

Crime fell rapidly and across the board in the United States during the Great Recession, at a time of deep economic decline, and rising joblessness. This was a puzzle: crime is expected to rise, not fall, when unemployment rises. I show that unemployment insurance (UI) benefit extensions can account for part of the puzzle, explaining why crime did not rise. The idea is straightforward: the higher propensity to commit crime associated with higher joblessness was mitigated by the fact that UI to those unemployed was more generous, hence replacing a larger portion of pre-unemployment income for a longer time. Different UI extension rules at the state level provide exogenous variation in the length of benefits, which I use to identify the impact of unemployment benefits can affect crime. I estimate that in places with an additional $1,000 rise in UI per unemployed person, crime would have been 1.5% higher were it not for the extensions, on an annual basis. Back of the envelope calculations suggest that UI extensions can account up to half of the reason why crime did not rise. Importantly, while my strategy only allows me to

68

consider variation in the length of benefits, it is conceivable that UI also prevented crime from rising through higher benefits for all recipients.

2.2 Introduction

The onset of the Great Recession was accompanied by a broad and substantial fall in crime in the United States. Property crime fell by 5.4% in 2009 over the previous year, and by a further 3.1% in 2010. Crimes against persons followed the same trajectory, driven by an even larger fall in robberies of 8.8% in 2009 and 10.4% in 2010.[1] Crime had steadily declined throughout the 2000s, falling approximately 2% per year, and this further decline occurred against the backdrop of the deepest recession of the previous 80 years.

This presents two puzzles. Typically, we think that there is a positive relationship between unemployment and crime, and a conservative consensus estimate is that a one percentage point increase in unemployment leads to a one percent rise in crime (Freeman 1995). As such, it is a puzzle that crime *fell*, but also that it did *not rise*. In this paper, I test whether unemployment insurance (UI) benefits generosity can partially account for the second puzzle. The institutional features of unemployment insurance, together with special provisions in the American Recovery and Reinvestment Act of 2009 or ARRA (commonly known as the Obama stimulus), greatly expanded the *length* of unemployment benefits. ARRA also raised

---

[1]The FBI, through its Unified Crime Reporting System, classifies major crimes in two categories, violent and property. Property crime consists of burglary, larceny-theft and auto-theft, while violent crime consists of murder, forcible rape, and robbery. The distinction between theft and robbery involves the physical presence of the victim.

the *level* of benefits for all recipients by \$25. If work and crime are substitutes, for at least some individuals, then, for a given level of joblessness, more generous and longer benefits should be associated with a lower probability of committing crimes, by replacing a larger portion of pre-unemployment income, and for a longer time. It follows then that for rising unemployment, UI generosity should mitigate the expected rise in the probability to commit a crime.

I identify a statistically significant causal effect of the rise in UI benefits per unemployed individual on property crime. I use county level crime data from the UCR program of the FBI, the UI extension database from the Department of Labor, labor market data from the BLS, and demographic data from the Census, and focus my analysis on 2009, a year where there is enough variation to give me statistical power. I find that in places with an additional \$1,000 annual rise in UI per unemployed person (over a pre-crisis mean of around \$3,000), crime would have been 1.5% higher were it not for the extensions.[2] Back of the envelope calculations suggest that UI extensions can account for up to half of the counterfactual rise in crime that previous literature suggested should have taken place, implying UI can have an important role in mitigating the effect of unemployment on crime. Importantly, while my identification strategy only allows me examine the impact of the *length* of UI benefits on crime, higher UI benefit *levels* may also have affected crime, as would be expected, through higher benefits for all recipients.

A regression of unemployment benefits per unemployed person on the crime rate is likely to suffer from omitted variable bias, to the extent that factors related

---

[2]In 2008, the crime rate in the United States was 3,214.6 per 100,000 people.

to both crime and unemployment benefits or labor market conditions cannot be accounted for. Simultaneity bias is also likely to exist, since high unemployment areas typically have higher crime, together with non-trivial measurement error. As such, it is necessary to identify an instrument that is highly correlated with unemployment benefits, and uncorrelated with the error term.

My identification strategy exploits the fact that different states use different rules to extend unemployment benefits during high unemployment times, and this institutional variation is sufficient to retrieve a causal estimate of unemployment benefits on crime. I use two different instruments, each of which is valid under the assumption that the policies are exogenous ("as-good-as randomly assigned") to the crime and labor market conditions of the states.

The main instrument for UI per recipient is the so-called TUR rule, which triggers the provision of Extended Benefits (EB) for an additional 13 to 20 weeks, over and above the standard 26 weeks available in all states, once state-level unemployment is over 6.5 percent and over 10 percent higher than the either of the previous two years (for details about the extension rule see Section 2.4). Twelve states have this rule, which was instituted in the 1970s, and so is plausibly uncorrelated with local crime and labor market conditions, or any other variable potentially affecting crime which cannot be accounted for. This instrument hence addresses omitted variable bias, while also correcting for measurement error.

In order to increase precision, I use a measure of total weeks of extensions from EB and the Emergency Unemployment Compensation (EUC) program as a separate instrument. A minimal additional assumption is necessary to ensure the validity of

71

the exclusion restriction for the continuous instrument (MEW). Namely, I need to assume that a county is not too large compared to the rest of the state, so that its unemployment rate is not driving the extensions. This is due to the complex non-linear nature of the overall extension program; this assumption is not needed for the TUR instrument because it is (plausibly) unconditionally as-if-randomly assigned. The MEW instruments is assumed to be as-if-randomly assigned only conditional on state unemployment.

My paper is part of a growing literature that uses instrumental variables techniques to study crime (see Mustard 2010), and is naturally placed at the intersection of two related subfields: on the one hand, papers looking at how local labor markets affect crime, and, on the other, how transfer programs affect crime. The former, in particular, has recently made strides in estimating much larger effects of unemployment or wages on crime using IV, compared to the previous literature had with OLS. This is less so for transfer payments, primarily due to the fact that, in the United States, as well as other developed countries, there is little spatial variation in transfer payments due to institutional differences across jurisdictions.[3] In addition, my paper combines a policy variation (the UI extensions) which is activated by a natural experiment (a large negative aggregate shock to employment), and thus provides an identification strategy which has been suggested as being a particularly attractive in the context of crime, as it sidesteps the problem of covariate identification, which has been blamed for the conflicting conclusions taken by different

---

[3]Exceptions are Foley (2011) for welfare payments in the United States and Machin & Marie (2006) for UI in the United Kingdom.

researchers (Durlauf et al. 2010). Finally, there now exists an extensive literature on the effects and incentives of the expansion of unemployment insurance in the Great Recession on job search (Rothstein 2011), on poverty (Bitler & Hoynes 2013), and on participation in other programs (Mueller et al. 2013). I contribute to this literature by examining another, unintended effect, of the UI expansion.

My paper underlines a significant positive externality of unemployment insurance in general. The benefits of UI had been traditionally viewed in terms of smoothing consumption (Gruber 1997), alleviating liquidity constraints (Chetty 2008), or improving job quality (Caliendo et al. 2009), while creating small job search disincentives (Farber & Valletta 2013). I show here that an unintended benefit of UI is to moderate the effect of unemployment on crime. This has important consequences in terms of policy design, especially at a time when some states have reduced the length of standard UI below 26 weeks, the EUC program has not been renewed, and EB triggers are deactivated since unemployment is falling, even though it still high in many states. In addition, as of early 2014, job finding rates are 30% lower than before the crisis.[4] The social cost of crime in the United States is very high. In terms of direct monetary costs, expenditure on corrections, police protection and legal services were estimated by the Department of Justice to be up to $261 billion for 2010 (Kyckelhahn & Martin 2013). Furthermore, crime can have a devastating long term impact on minorities, especially children, either because their parents are in prison (Kearney et al. 2014), or because they go to prison themselves, in which

---

[4]Source: http://www.hamiltonproject.org/papers/importance_of_unemployment_insurance_for_american_families_economy

case they are at a significantly higher risk of not finishing high school or recidivating as adults (Aizer & Doyle 2013). Total unemployment insurance expenditure for 2010 was approximately $140 billion, suggesting that even moderate impacts of UI on crime can have substantial fiscal benefits, all the while improving outcomes for poor families.

The following discussion is scheduled as follows. Section 2.3 describes the previous literature. Section 2.4 describes the UI extension program and the data. Section 2.5 discusses the empirical strategy, and Section 2.6 presents the results. Section 2.7 provides an interpretation of the results, and Section 2.8 concludes.

2.3   Previous Literature

The economic analysis of crime goes back to Becker (1968) and Ehrlich (1973), who argue that the decision to commit a crime involves a cost-benefit analysis, which is consistent with the rising incomes and rising crime in the 1960s. More recent literature has emphasized that a) national or state level data are not appropriate for the analysis of crime, an inherently local phenomenon and b) that crimes are committed by the employed as well, so a binary choice model between crime and work will not fit the data (Freeman 1995).

The particular complication of studying crime and economic incentives is that standard problems of unconfoundedness can bias results in indeterminate directions - that is, even the direction of the bias is typically unknown. Consider reverse causality: on the one hand, crime can negatively affect employment at the individual

level, with lower employability for past offenders (Freeman 1995). By contrast, at the local aggregate level, reverse causality can have an ambiguous effect (Mustard 2010): high crime can lead to wealthy residents leaving the jurisdiction (Cullen & Levitt 1999), but can also lead employers to offer higher wages (Roback 1982). Omitted variable bias is also especially pronounced in the crime context, as we know very little about what causes crime (Durlauf et al. 2010), and if the omitted variables are correlated with the variable of interest, results will be biased. For instance, Raphael & Winter-Ebmer (2001) show that consumption of criminogenic substances, such as drugs and alcohol, is pro-cyclical (and so correlated with unemployment), so omission of a relevant variable in a regression of crime on unemployment will result in downwardly biased estimates. Some observers (e.g. Horowitz 2004) suggest that natural experiments are the only way of working around this issue, and it is this strategy that I implicitly employ in this paper. Finally, measurement error is a pronounced concern with crime data (see Appendix A for details). Though classical measurement error in the dependent variable does not bias estimates in linear models, crime regressions invariably use log transformation, in which case errors will lead to biased estimates.

Concerning the labor market, the literature has found larger effects of unemployment on crime with IV than OLS (Mustard 2010), suggesting that the bias is mostly downwards. In the US, Lin (2008) uses real exchange rate changes to instrument for changes in the the state level manufacturing sector GDP, and finds an elasticity of property crimes with respect to unemployment of around 4 to 6. Raphael & Winter-Ebmer (2001) use defense contracts and oil shocks at the state

level and find similar estimates. Gould et al. (2002) argue that the wages of low skilled men are a better measure of labor market conditions, given the cyclicality of unemployment, but still show elasticities of property crime with respect to unemployment of around 2.[5] Fougere et al. (2009) find significant effects for youth crime in France. Notably, all these papers explicitly consider groups relatively more likely to engage in crime.

Work on the effects of social programs on crime has been comparatively slimmer, despite the theoretical link: if we expect poorer labor markets to lead to higher crimes because individuals substitute between crime and legal employment, then we should also expect social programs to mediate that link. A substantial challenge is the lack of spatial variation in welfare programs. Indeed, virtually all relevant work in the United States has exploited variation caused by the 1996 welfare reform (PRWORA), which allowed for state-level initiatives through waivers, and hence created cross-state variation.[6] This leads to further identification problems, if the adoption of state-level programs is in any way correlated with pre-existing crime trends. Monte & Lewis (2011) overcome this issue by using individual level panel data and document a rise in female criminality that coincided with PRWORA. They estimate a hazard rate model, and show that higher unemployment and non-receipt of welfare raise the hazard of criminal behaviour.[7] Beck et al. (1980) provide evidence from a social experimental in 1970s, which gave welfare payments to former

---

[5]See also Grogger (1998) for an early examination of youth and wages in the US, and Machin & Meghir (2004) for the UK.

[6]This obstacle has also been reported in the UK context by Machin & Meghir (2004).

[7]Broadly similar results are reported elsewhere in the literature, see Defronzo (1996), Defronzo (1997), Allen & Stone (1999), Hannon & Defronzo (1998b), Hannon & Defronzo (1998a).

convicts, and reduced crime.[8]

Foley (2011) considers instead high-frequency temporal variation. He looks at monthly welfare payment cycles in 12 large US cities across which there is variation at the time of the month when welfare payments (TANF, Food Stamps/SNAP, SSI) are disbursed. He considers whether smoother payment cycles lead to smoother crime cycles, compared to staggered payment cycles (i.e. once or twice a month), and indeed finds strong evidence that financially motivated crimes, but not other crimes, rise with the number of days since the last payment. Crime is higher after the first ten days of the month in cities where payments take place early in the month, compared to cities with smoother payment cycles. This is evidence that crime supplements welfare income, consistent with the standard model.

Little is known about the effects of unemployment insurance on crime, also due to the lack of substantial variation. Machin & Marie (2006) study a policy which simplified the British unemployment insurance system and introduced the Jobseeker's Allowance (JSA), a simpler and much more stringent regime, which reduced duration and mandated monitoring to ensure search effort. Their results are consistent with the Beckerian model, showing that areas with more exposure to the new policy exhibited a relative rise in crime. Note that JSA is much less generous than the UI system, with benefits capped at £71 per week, while welfare payments are typically more generous in the UK, so comparisons are not straightforward.[9]

---

[8]Technically the program envisioned unemployment benefits, but the transfers were given to individuals with no labor force attachment, and were taken away once the individuals found, so the program was identical to welfare with 100% benefit withdrawal rate.

[9]In the US, average weekly benefit amount was approximately $300 in 2009, and the lowest in the country was $196 (for Mississippi).

Fougere et al. (2009) also show suggestive evidence of a negative relationship between UI and crime for France, but they do not have access to instruments for UI and so their estimates are quite low (compared the large IV estimates of the effects of unemployment on crime that they report).

Finally, in the early literature, crime and work were considered as mutually exclusive choices, and ignored the possibility of crime as illegal work. Fagan & Freeman (1999) argue that one should instead consider a continuum of possible options. It is thus more intuitive to think of the decision between legal and illegal activities as solutions to a time-use problem. One could think then of the early literature as focusing on the subset of choices leading to corner solutions, ignoring the possibility of interior ones. When referring to crime and work as substitutes, or as crime income complementing work income, it is this framework that I have in mind.

2.4   The UI system and Data

Unemployment insurance (UI) in the United States is composed of three tiers. The first tier is the Unemployment Compensation (UC) program, the "normal" UI program, which has traditionally provided unemployment benefits to eligible individuals for up to 26 weeks.[10] Such benefits are financed through state payroll taxes, and vary in generosity (the replacement rate) across states, in a very stable manner.

---

[10]Several states now have fewer than 26 weeks of coverage, but these changes took effect in 2011 or later.

In times of high unemployment, two further tiers may become operational. The first is the Extended Benefits (EB) program, a permanent program triggered if state-level unemployment passes a specific threshold, which varies across states. EB provides an additional 13 or 20 of benefits to individuals who exhaust normal UI, and financing is typically divided between state and federal governments.

EB can be activated by three possible "ON" triggers. The first is common to all states, and requires that the insured unemployment rate (IUR) be on average at least 5% for 13 consecutive weeks and be 120% or more of the average IUR over the same period of the preceding two years. IUR is the unemployment rate of UI eligible individuals, that is, the fraction of regular UI recipients as a percentage of the labor force covered by UI.

Most states have a second trigger, activated if IUR is over 6% on average over the preceding 13 weeks, irrespective of old IUR. Finally, a third trigger, operating in 12 states, involves the 3-month TUR (total unemployment rate, the standard metric given by the ratio of the unemployed over the labor force) averaging at least 6.5% and being at least 110% of the 3-month TUR of the same period for either of the two previous years. If TUR is above 8%, extensions can go up to 20 weeks. Crucially, the 3-month TUR rule is the easiest to activate and the 6% IUR the toughest, providing the inter-state variation necessary for identification.

Finally, Emergency Unemployment Compensation (EUC) legislation can be passed by Congress during times of exceptionally high unemployment and extend benefits further, with the federal government typically being responsible for the costs. Congress initially passed EUC08 in July 2008 (before any states had activated

EB), and provided an extension of 13 weeks for all states. It was reinforced in November and extended through ARRA to provide 20 weeks for all states, and an additional 12 weeks to states with very high unemployment (see Appendix B.3.1 for details). ARRA also uniformly raised all UI checks by 25$ weekly.

2.4.1 Data

Data on UI extensions on a weekly basis come from the Department of Labor.[11] Crime data appear in annual format, so the most natural way to construct my extension measure for the continuous instrument is to calculate the average number of weeks of extensions for each state. For instance, if a state has no extensions for the first half of the year, and 30 weeks for the other half, it will be recorded as having an average of 15 weeks for the year.

Data on crime come from the centralized Unified Crime Reporting (UCR) database from the Federal Bureau of Investigation. The FBI collects reports from local law enforcement agencies on a voluntary basis from cities with a population of 10,000 or more, and from counties with a population of 25,000 or more. The raw data are then imputed into a county level aggregate, by the National Archive of Criminal Justice Data (NACJD) at the University of Michigan, and that is the data source I will be using. A detailed discussion of various issues concerning the crime data can be found in the appendix.

County unemployment data come from LAUS and county employment and wages data from QCEW, both from the Bureau of Labor Statistics. Data on per-

---

[11]http://workforcesecurity.doleta.gov/unemploy/claims_arch.asp.

sonal characteristics come from intercensal estimates for demographic variables, provided by the Census, and cover detailed information of the age, gender, and race distributions. Finally, annual data on UI spending at the county level comes from the Bureau of Economic Analysis regional program.

2.4.2 Sample

As I more fully explain in the next section, too few states had UI extensions in 2008 and for too little time, while exactly the opposite is true for 2010. As such, I focus my analysis on 2009, the year when ARRA was enacted and the UI system expanded substantially.

My final sample includes counties with population over 50,000 in 2008, and with full data from 2005 to 2010. I drop all counties from Illinois. Out of 102 counties, only 6 had non-zero crime data in 2005-2007 (including 5 of the 7 largest), and another 43 in 2008. The ICPSR codebooks do mention Illinois as having particularly problematic crime data. Including the 6 correct counties (which includes Cook County, the second largest county in the United States) in my estimation does not affect my results at all, but I nevertheless drop them for consistency. I also drop New York City, as ICPSR collects crime at the Police Department level, which covers all 5 boroughs (each a separate county), and then apportions crime data to each borough by population. This seems unlikely to be valid, and although the NYPD reports separate counts for each precinct on its website, I prefer to be consistent. Here as well though, my results are not changed if I include New York City. Finally,

Virginia has 95 counties and independent cities. On multiple occasions, such cities are completely contained within the surrounding county, and BEA releases UI data for the whole region. I thus have to drop those counties from my analysis. I also drop the whole state of Virginia as a check, and the results are unchanged, so I decide to keep the available counties in my sample.

Table 2.1 gives summary statistics for my final sample of 907 counties.

2.5   Crime in the Great Recession

Graphical evidence of crime in the Great Recession can help build intuition. We are interested in showing that UI extensions prevented a rise, but since crime fell almost everywhere, this is equivalent to showing that crime fell more in places that had higher extensions conditional on unemployment. As I show below, it turns out that places affected by the binary instrument had unemployment that was virtually identical, on average, with the national unemployment level, and so conditioning on unemployment is not necessary for the graphical exercise. Though I will not be estimating a differences-in-differences model, which would require common crime trends between high and low unemployment counties and states before 2009, visual evidence will help strengthen the econometric results to follow.

I break down my sample of 907 counties into two bins, according to mean extension weeks in 2009. If intuition is correct, we expect to see sharper falls in crime in 2009 for counties in high extension states, compared to counties in low extension states.

Figure 2.4 plots the average crime trends for the two groups (weighted by county population), setting 2005 crime rate equal to 1 for ease of interpretation. The intuition seems indeed valid: after a similar downward trend before the recession, counties in high extension states exhibited a much larger fall in 2009, with the trends converging again in 2010. This image is even starker if we consider only the larger counties, and this is shown in figure 2.5, where only the counties with population of at least 500,000 are shown. Figure 2.6 shows the same story, only in this case I restrict the sample to cover only those counties whose population is less than 10% of the state population, in order to make sure that a county is not too large to affect the state extension rate.

2.5.1 Empirical framework

The figures present a story of crime declining substantially more in 2009 in counties at higher-than-median-extension states, suggesting that 2009 saw a shock that differentially affected those counties. In light of the graphical evidence, and the discussion in the previous section, it becomes apparent that the variation required to identify the effects of UI on crime must come as a state-level shock. The differential rise in UI extensions, as a result of a spike in unemployment, indeed provides such variation.

My empirical framework is given by the following model:

$$\Delta crime_{i,2009} = \gamma \Delta UI_{i,2009} + \zeta' \Delta X_{i,2009} + \Delta \epsilon_{i,2009}, \tag{2.1}$$

where X is a matrix of local demographic characteristics, to be defined below. This is a standard first differences model, which exploits the panel nature of the data.[12] The crime variable is in logs, so that its first difference has a percentage change interpretation.

First differencing accounts for unobserved heterogeneity, which is presumably substantial. In addition, extensions provide exogenous variation for 2009 only. In 2008, only 4 states had EB activated at one point or another (Alaska, North Carolina, Oregon, Rhode Island - all of them through the optional TUR trigger addressed below) and for not enough time to generate substantial difference in UI. A few more activated the 2nd tier of EUC in late November. In contrast, in 2010, while there is still substantial variation in mean extension weeks, though lower than in 2009[13], there is too little variation year-on-year, since states with high extensions in 2009 had high extensions in 2010. Since I have to account for unobserved heterogeneity, 2010 does not provide enough variation.[14]

The model in (2.1) is valid only if unobserved heterogeneity is time invariant, so that first differencing removes any correlation between error and regressors. The short time-span I am considering makes this a reasonable assumption for area effects (social interactions), which are very important for crime (Freeman 1995). Glaeser et al. (1995) show that only 30% of the substantial geographic variation in crime is driven by demographics. However, time-varying heterogeneity, especially during

---

[12]Note that a fixed-effects (within) model with a year dummy is statistically equivalent to first differences when $T=2$.

[13]The coefficient of variation for mean extension weeks is 0.22 in 2009 and 0.16 in 2010, falling to 0.12 once I drop the three small states (NE, ND, SD) which did not activate tier 3 of EUC in 2010.

[14]Put another way, the first stage is too weak for 2008 and 2010.

this time-frame, is sure to be present. For instance, in areas with relatively low wages (and so low UI), consumption of durables could have fallen so much so as to reduce profitable crime opportunities. Criminals could have left crisis ridden areas in low unemployment states in search for better opportunities. In contrast, the proliferation of smart phones made profitable petty crime easier, and coincided chronologically with the recession. It is possible that the recession itself altered people's attitudes towards crime. This is actually one of the arguments put forward about why crime fell during the Great Depression (Wilson 2011), and is addressed below. Moreover, as already mentioned, omitted variable bias is as much of an issue as simultaneity. In addition, the use of a log transformation for the dependent variable means that if the dependent variable is measured with substantial error, this can feed into the right hand side error term and give inconsistent estimates (see the appendix for details). Finally, standard measurement error of UI is very likely, attenuating estimates. In any case, it is likely that OLS estimation of (2.1) will be biased, with an unknown direction of the bias.

2.5.2   Instruments

To remedy this, I instrument for the change in UI using extensions as the state level. I will use two different but related instruments. The first, which I call TUR instrument, is a binary variable set to 1 for states with a permanent TUR rule in law, and 0 otherwise. As mentioned earlier, the TUR rule is the easiest to trigger, with the mandatory rule being the hardest. Indeed, out of the 15 states with active EB

programs in early March 2009, 6 had used the TUR trigger, 9 the IUR trigger and none the mandatory trigger[15], although only 12 states have the TUR option in law, and these 12 states did not have higher than average unemployment in 2009. The TUR instrument is an excellent exogenous source of institutional variation (as those rules were passed into law in the 1970s), but it can only give us enough variation for the first quarter of 2009. Almost all states have a provision that activates the TUR option if federal financing reaches 100%, which is exactly what ARRA achieved, effective end of March. This quarter generated substantial variation (on average, UI per recipient rose by around $1,000 a year more in TUR states compared to other states), but one might worry that it does not give high enough precision. Table 2.2 presents the reduced form regression, that is, the regression of the change in crime on the TUR instruments, and shows that crime fell by almost 2 percentage points more in the TUR states compared to the rest, giving support to the underlying hypothesis of this paper, assuming the instrument is valid.

Ideally, one would implement a regression discontinuity design, comparing counties in states just below and above the cutoff. This is not possible here as I have neither the statistical power nor the high frequency UI variation to do this, so an alternative to increase efficiency and hence the strength of my instrument is to condition on the change in state unemployment. This is valid assuming the change in state unemployment is uncorrelated with the error term. It is unclear whether this is true. It should ostensibly be the case for counties small enough compared to the state, or with counties whose unemployment (or change in unemployment) is

---

[15]http://www.urban.org/publications/411851.html.

substantially smaller than the state total. In any case, this does not affect the validity of the instrument, which is independent of state level labor market conditions (no correlation of TUR and state unemployment).[16] Conditioning on the change in state unemployment does not affect the first stage estimates at all and markedly improves precision. Restricting the sample to counties with less than 10% of the state population, or those whose unemployment change is half a point smaller (or less than 80% of) the change in state unemployment also gives the same estimates (but with lower precision). The first stage results are given in the top panel of Table 2.4.

The binary instrument is inefficient because it ignores the other parts of the emergency UI framework. EUC resulted in further increases in extensions, in a non-linear fashion, whereby states were granted further extensions if unemployment passed certain cut-offs (see the appendix for details). So the combination of TUR, IUR, EUC and the mandatory rule resulted in a high degree of variation in the mean weeks of extensions states received in 2009, and so mean extension weeks (MEW) for the state (or its change over the previous year) is my second instrument. The benefit of using this continuous instrument is a gain in efficiency and instrument strength, due to the much higher level of variation.

However, conditioning on the change in state unemployment now becomes essential: MEW is the result of a highly complex system of trigger rules, and is thus

---

[16]To see why this is the case, the covariance of the model in (2.1) with the instrument Z_i is

$$Cov(\Delta Y_i, Z_i) = Cov(\gamma \Delta UI_i + \zeta' \Delta X_i + \Delta \epsilon_i, Z_i) = \gamma Cov(\Delta UI_i, Z_i) + \zeta Cov(\Delta X_i, Z_i) + Cov(\epsilon_i, Z_i)$$
$$= \gamma Cov(\Delta UI_i, Z_i) \rightarrow \gamma = \frac{Cov(\Delta Y_i, Z_i)}{Cov(\Delta X_i, Z_i)} \tag{2.2}$$

highly correlated with state unemployment, and ignoring this latter will invalidate estimation.[17] This was not an issue with the TUR instrument because TUR states had unemployment equal to the national mean. Adding state unemployment on the covariate list removes it from the error term, but then the previous issue rises again of whether it is endogenous or not. Rothstein (2011) employs a similar approach to estimate the employment effects of UI extensions. He faces a very similar problem, as he wants to remove the labor demand component from his extensions measures, and employs cubic polynomials for the state unemployment rate. I also experiment with a cubic polynomial, as well as 5-piece splines, and the results are unchanged. He uses individual-level data from the CPS, and so endogeneity is less of a concern for his approach. In any case, the coefficient estimate of the effect of UI on crime is almost identical to the binary instrument without conditioning, with further gains in efficiency and instrument strength, thus alleviating such concerns.

2.5.3   IV assumptions

Following the standard program evaluation notation (e.g. Angrist & Pischke 2010), let Y_{1i} denote the value of Y_i if county i receives treatment, and Y_{0i} if it does not. Furthermore, let D_{1i} denote the value of treatment D_i if the instrument $Z_i = 1$

---

[17]With the vector of X's fully exogenous, the estimate will have probability limit $\gamma + \zeta \frac{Cov(\Delta X_i, Z_i)}{Cov(\Delta UI_i, Z_i)}$.

and D_{0i} if $Z_i = 0$. These can be written as

$$D_i = D_{0i} + (D_{1i} - D_{0i})Z_i$$

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i, \tag{2.3}$$

where Y and D correspond to $\Delta crime$ and ΔUI respectively. For Z_i to be a valid instrument, we need that i) $Y_{0i}, Y_{1i}, D_{0i}, D_{1i} \perp\!\!\!\perp Z_i$, ii) $\mathrm{E}(D_{1i} - D_{0i}) \neq 0$, and iii) $D_{1i} \geq D_{0i}, \forall i$.

The first condition combines the independence and exclusion restrictions. The independence restriction, alternatively stated as saying that the instrument is un-correlated with $\Delta \epsilon_i$, says that the instrument is as good as randomly assigned, independent of potential outcomes and potential treatments assignments. That is, the assignment of Z_i is unrelated to the change in crime that would have occurred were it not for the instrument. Similarly, the assignment of Z_i is unrelated to the change in UI that would have occurred were it not for the instrument. The exclusion restriction says that the only way the Z_i induces a change in crime is through its effect on the change in UI.

The exclusion restriction is trivially satisfied. It could only be violated if the implementation of extensions *by itself* induced a change in crime. While this is untestable, it seems implausible to have been the case. The independence assump-tion is satisfied as long as a county is not too large compared to the state. If a county is large enough so that its unemployment level can affect the state unemployment level, then the instrument might not be independent of potential outcomes. In my

regressions, I control for this issue by restricting the sample in some models to counties that contain less than 10% of the state population. Note that in the case of the binary instrument, as long as the non-TUR states are reasonable comparison groups, having a dominant county is only an issue if it is too different from the rest of the state. For example, take the extreme where shocks are perfectly equi-correlated within states (or simply where there is only an aggregate shock). In this case, we are simply comparing averages across TUR and non-TUR states.

The independence assumption is slightly more subtle in this application than in a standard IV setting. While the instrument is plausibly as-good-as randomly assigned, its activation depends on state unemployment. If, for whatever reason, only the TUR states had high unemployment, then the instrument would be correlated with state unemployment, in which case validity of the instrument would depend on whether state unemployment is correlated with the error term. Luckily, there seems to be no correlation, as can be seen from the reduced form regression in Table 2.2.[18] In addition,

The second condition states that a first stage exists. That is, Z_i is correlated with ΔUI_i. This assumption is testable and will be shown in the results section. Finally, the last condition is the monotonicity restriction, which requires that assignment of the instrument monotonically affects treatment. In this context, it says that ΔUI_i be higher with the extensions than without. It is also non-testable (as we do not observe counterfactual outcomes), but is also trivially satisfied, given

---

[18] The t-test for equality of means of state unemployment for TUR and non-TUR states has a p-value of over 90%.

the institutional setting previously described, and the fact that no state made the conditions for UI more stringent in 2009.

The monotonicity assumption is only required if outcomes are allowed to be heterogeneous, and is a requirement for the LATE theorem of Imbens & Angrist (1994). LATE identifies the effects of the treatment from the population of those who are induced by the instrument to take the treatment (the "compliers"), who in this case are the unemployed receiving UI. There has been considerable interest in IV under heterogeneity lately (see Heckman & Vytlacil (2007)) and the conditions under which IV can identify an interesting treatment effect. Heckman & Vytlacil (2006) show that if there is "selection on gain", that is, if the treatment is chosen by those who would disproportionately benefit from it, then LATE does not identify a meaningful effect, even under monotonicity. Assuming assignment is random there is no selection to be concerned with, except for potential differences in take-up rates across counties.

My strategy estimates what Angrist & Imbens (1995) call the average causal response, and is the variant for the general case of a multivalued treatment. For the TUR instrument, the estimate has a transparent decomposition as a standard Wald estimator. Obviously, the treatment is hardly discrete, but it is straightforward to discretize it, say into bins of $1,000 or $500. The IV estimate is thus the weighted average of causal responses at each unit of the treatment for counties induced by the instrument to increase UI by at least one unit compared to the previous year. The case where the instrument is multivalued as well is much more complicated but this is not important here, as the estimates are virtually identical with either

instrument.

2.6 Results

To begin with, Table 2.3 reports several models based on (2.1), without instrumenting. It is interesting to consider first the correlations in the data. Across all models, we see that the coefficient of ΔUI is negative but small and varies in significance. Precision is improved as more covariates are added, but the p-value is never below 15%. Note that ΔUI is in \$1000's, so a coefficient of -0.003 indicates that a \$1,000 dollar annual rise in UI is associated with a 0.3% lower crime rate. The coefficient of unemployment is negative and highly significant throughout, be it a the state or the local level (though adding county level makes state insignificant). This is likely due to the substantial rise in enrollment and outlays of the Supplemental Nutritional Assistance Program (SNAP) during the Great Recession. Ganong & Liebman (2013) show that the 5 million rise in participation (18% rise) and almost 50% rise in outlays (from \$34.6 billion to \$50.4 billion) in 2009 over the previous year is almost entirely due to the rise in unemployment (together with a uniform rise in expenditure due to ARRA). Overall, these structural regression results are not informative, and are only meant to underline correlations. The fact that the coefficient of ΔUI has at least a negative sign is encouraging, but no conclusions should be drawn from it. Note that all variables mentioned here are in first difference form unless otherwise stated.

Next I turn to the IV results, first presenting the models using the TUR

instrument in Table 2.4. Even with no covariates and a binary instrument that has a value of 1 for only 20% of the sample, the coefficient is much higher in absolute value, and significant at the 10% level. Moreover, the first stage is quite strong, with an F-test of 12.39. This is below the threshold of 16.38 defined by Stock & Yogo (2005) as indicative of at most 10% bias, but above 8.96 indicating 15% bias.[19] The coefficient of ΔUI is -0.015, meaning that a rise in $1,000 in UI per unemployed person leads to a 1.5 percentage points fall in property crime.

Adding covariates changes nothing in the interpretation of the results, and barely affects the coefficient, but significantly improves precision, as it adds more points of support. Adding state unemployment raises the significance of the UI estimate (p-value is now almost 2.2%), and the strength of the instrument, but leaves the coefficient unaffected, due to the unconditionally as-if-random nature of the instrument (as shown in 2.2). Adding demographic covariates and state wage alters slightly the coefficient, due to the inevitable small sample bias in the reduced form, which gives rise to weak correlations between the instrument and covariates. This goes away if I add state unemployment, and the first-stage F-stat is now comfortably higher than the highest Stock-Yogo value. Overall, the robustness of the estimate is pronounced, a result expected from the highly significant difference in means in the dependent variable for counties with and without TUR (Table 2.2).

Turning now to the continuous MEW instrument, recall that we have to condition on state unemployment. MEW is correlated with it, in a highly non-linear

---

[19]Note that errors are clustered at the state level, as I am considering the effects of state-level policies. With errors clustered at the county level or not at all, the F test is above 100.

fashion, and ignoring this correlation will yield inconsistent estimates, as the first-stage fitted values will pick up the correlations of state unemployment with the instrument, UI, and crime (see footnote 17 for the analytical expression). This issue is illustrated in column one of Table 2.5, where no covariates are included, and the estimate is completely different than before. Once I add state unemployment though, the coefficient is very close to the binary instrument case, with higher F-tests for the instrument. Adding demographics variables and state wage, or allowing for time-varying coefficients for state unemployment increases the significance of the instrument and leaves the coefficient unchanged. Notice that adding labor market and income covariates other than unemployment (log wage, log retail wage, mean or median household income, ratio of mean to median household income) does not affect the estimate (only slightly reduces precision, as all these variables are insignificant), which highlights the fact that the correlation of MEW and state unemployment is only due to the trigger rules. If state unemployment is correlated with the error term, then so should be other labor market and income variables.

2.7 Interpretation

In the previous section I showed that due to the expansion of UI during the Great Recession, an additional dollar rise in UI led to a larger fall in crime, with the coefficient being around -0.012 to -0.015 for UI. This means that an additional $1,000 rise in UI led to approximately 1.5% fewer crimes committed, all other things remaining equal. As the crime rate in 2008 was approximately 3,215 per 100,000

people, this translates into a reduction of around 50 crimes per 100,000 in areas treated. As I am agnostic on why crime fell (but offer some possibilities in the next section) and focus on why it did not rise, the interpretation of the result is that in places with more generous extensions the UI system played a more important role in mitigating the higher probability to commit crimes that result from higher unemployment. As crime fell everywhere, the mitigating effect of UI in this context imply a fall in crime.

To quantify the overall effect the extensions had on crime, the variation exploited has to be considered. ΔUI was around \$3,500 on average in my sample, but I have to take into account the fact that there was a substantial uniform increase in benefits from two different sources. First, ARRA instituted a \$25 weekly uniform rise in benefits per recipient. Second, the EUC program had some uniform extensions with no unemployment requirements, which grew from 2008 onwards (see Appendix B.3.1), resulting in a minimum rise of 13 weeks of extensions for every state compared to the previous year.[20] Note that this issue is a problem because our interest is in the amount *individuals* received, not overall transfers to each locality, as the fundamental economic problem is at the individual level. Unfortunately it is impossible to correct for these issues, together with the certainly non-trivial measurement error in UI, without very strong assumptions. But it is possible to conservatively bound this effect, by noting that since more than 75% of the labor force in 2009 had spells lasting more than 5 weeks, 60% longer than 10 weeks, and

---

[20]To see why this has to be accounted for, assume there are two counties, with two types of unemployed each. Type A receive 5 dollars more in both counties, and type B receive 5 dollars more in county 1 and 10 dollars more in county 2, due to policy differences. Then there is variation for only 5 dollars, but just taking the mean would show 10.

about a third for 27 weeks or more[21], we can assume that there is a significant part of the unemployed in every locality that receive what was essentially a large lump sum. The 1st percentile of ΔUI in 2009 was \$853 and the 5th \$1,760, so assuming a lower bound on the size of the lump sum of \$1,500 leaves us with at most \$2,000 of variation, which can explain at most about 45 to 55% of the documented fall in crime. Raising the maximum value of the lump-sum to \$2,500 (25th percentile) restricts the magnitude that can be explained to 27%.

A lower bound on the magnitude of the change in crime that UI can explain can also be given by the R^2 of the first-stage for the binary instrument, which is 0.11, meaning that TUR alone can explain 11% of ΔUI, or around \$380. This gives a lower bound on the explanatory power of UI of 8.2%. While these calculations are necessarily imprecise, it is important to consider the variation exploited here. In the simplest case, for the TUR instrument, variation comes only in terms of extensions, and thus the long-term unemployed, for the first quarter. With the MEW instrument, variation in extensions is much broader so the first-stage R^2 is 0.34, and assuming the independence assumption holds then UI extensions explain 25% of the fall in crime. A similar figure is obtained from exploiting the fact that the average rise in MEW is 3 weeks higher in TUR states, which, multiplied by the average weekly benefit of \$325 in those states (slightly higher than for non-TUR states), gives a \$975 relative rise (more than three quarters of the observed difference between the two types of states is accounted for).[22]

---

[21]http://www.bls.gov/cps/tables.htm

[22]This last exercise can exploit part of the flat \$25 payment. While the \$975 figure assumes the average unemployed person enjoyed those extra 3 weeks of UI, average benefits were more than \$3,000 higher per person for the TUR states, so it certainly seems plausible.

Finally, unemployment extensions could have only affected unemployed individuals, while a large fraction of crimes are committed by the employed. It has been suggested that focusing on the unemployed might give one an incomplete view of how labor market conditions affect crime (Machin & Meghir 2004). While this is certainly true in general, it is not the case for the period studied, as the unemployment ranks almost doubled in 2009.

2.7.1 Other factors

It is important to consider other factors that might have helped in preventing the crime rise and also to the reduction in crime, and how those could have confounded my estimates. The crucial issue here is the counterfactual: what would the crime trajectory have been without UI, conditional on local employment conditions. While I have shown that places that received higher benefits fared better, I cannot give a clear answer to this question, because benefits went up essentially everywhere by a significant amount. The nature of my strategy is necessarily local, thus while it can still give an important insight, namely that UI benefits mitigate the effect of unemployment on crime, it is difficult to deduce the counterfactual.

As mentioned before, there were almost surely other factors at play, most importantly the rise in SNAP enrollment and compensation (20$ or more uniform rise in benefits[23]), so that previous enrollees received more. SNAP is especially relevant to crime as it is targeted groups at high risk of committing crimes, such as single

---

[23]The precise number depends on family size, see http://www.cbpp.org/cms/?fa=view&id=3899.

mother-headed households with juveniles. Indeed, juvenile crimes (as measured by arrests) fell markedly throughout, and mostly in the areas with high unemployment, which saw most of the aggregate crime reduction. Similarly, high unemployment areas had about 10-15% more households headed by single mothers.[24] Unfortunately, while there is some cross-state variation in SNAP rules, it is not sufficient to provide identifying variation, especially since the rise in participation was almost exclusively due to the rise in unemployment (Ganong & Liebman 2013). SNAP and UI also compensated in terms of social protection for the sharp fall in scope, compared to previous recessions, of the Temporary Assistance for Needy Families (TANF) program, which used to be the backbone of the welfare system until 1996 (Bitler & Hoynes 2013). Even TANF, however, though not as comprehensive as it was in the past, provided very targeted support, in the form of 250,000 subsidized jobs, roughly equally split between needy adults and youth. Data is not available so one can only conjecture, but it is likely that this could have potentially played led to a reduction in crime.

We can thus assume that, ceteris paribus, crime would have risen were it not for the these programs. If we further assume that the counterfactual increase crime would have been 3%, a conservative estimate given that the literature gives a lower bound of 2 for the elasticity of property crime to unemployment, then UI explains up to half of why crime did not rise.[25]

---

[24]Source: UCR and the Decennial Census, author's calculations.

[25]On the contrary, one could argue that crime was on a downward trajectory, so would have fallen anyway. While the first part of this statement is true, I note that the the falling trend had moderated substantially by 2008 (from over 2% in the previous four years to only half of that), the first year of the crisis, even before the large rise in unemployment. So it does not seem unlikely that whatever factors were leading to this secular fall had dissipated.

It is also interesting to consider crime in the Great Depression, as crime fell during that time, and various theories have been suggested pertain to cultural factors, such as closeness with families and supervision for young children (Wilson 2011). This would suggest that factors specific to unusually deep downturns turn people away from crime, and cast doubt on the present estimates. There are a number of problem with this interpretation however. First, the Depression came with the abolition of Prohibition, an era typically considered to be rife with crime. To my knowledge, there is no work done on the impact on crime of the 21st amendment, which officially ended Prohibition in late 1933 (though wine and beer sales had become legal earlier in the year), but it is uncontroversial to argue that crime should have fallen as a result.[26] Second, the New Deal implemented a very large expansion of social assistance, primarily through work relief programs, and recent work by Fishback & Kantor (2010) argues that those programs did indeed reduce crime. Even though federal transfers did crowd out private transfers (Gruber & Hungerman 2007), federal assistance resulted in an increase of spending by almost 13 times, and the authors estimate that such assistance had an elasticity of around 0.15. In addition, a significant part of the work relief programs, the Civilian Conservation Corps (CCC), directly removed 3 million men aged 18-25 from 1933 to 1942 from their localities and employed them in rural areas, thereby directly removing a significant part of the most crime-prone demographic from the cities.[27]

---

[26]The rise in crime, especially organized crime, during Prohibition was the principal reason for repealing it.

[27]Fishback & Kantor (2010) mention that they do not have detailed data for the CCC but report some results using state-level information and obtain similar results. The bias is sure to understate the effect of the program.

Compared to the last major recession, that of the early 1980's, crime rose significantly initially and then fell just as significantly. UI also expanded substantially at the time. Data for this period do exist but in significantly different form than after 1994, and so I will not attempt and make comparisons. TUR rules had been passed relatively recently compared to that period so perhaps the instruments would not be valid. Also note that crime rates were almost 60% higher than during the Great Recession, implying that comparisons might be futile. Again, my strategy does not allow for global generalizations: it only allows me to deduce that UI extensions were responsible for a non-trivial portion of the fall in crime in 2009.

2.7.2   Limits to the identification Strategy

It is important to be more specific about the intuition the results give us, and about the limits of IV.

I showed previously that counties in TUR states exhibited a fall in crime of almost 2% relative to counties in non-TUR states, even though unemployment was roughly the same. So it is straightforward to start with the thought experiment where I assume that there was no rise in unemployment and TUR states gave benefits to the *already* unemployed for a longer time than the non-TUR states. Even in good labor market years, the fraction of the unemployed who would qualify for extensions is not trivial. In 2005, about 20% of the unemployed nationwide had durations lasting for more than 27 weeks.

The binary instrument employed here provides an excellent framework of

thinking about the mechanism at play, because it is essentially randomly assigned, and can be thought of, without loss of generality, as a randomized experiment. So the analysis can be boiled down to the simplest possible case, where we have two identical states, and one is "shocked" with lengthier UI benefits, and shows a fall in property crime. The affected population, the long-term unemployed, would thus respond to receipt of UI. In the no-extension state, some long-term unemployed engage in property crime when their benefits run out. In the TUR state, the long-term unemployed receive benefits for longer, and we can think of them committing fewer crimes either because they find jobs during their extensions and so do not have to commit crimes to supplement their income, or because they can live-off their benefits for longer before resorting to crime. This is consistent with the view of crime as supplementing other income in the study of Foley (2011) for welfare payments, especially since the long-term unemployed are probably more common with the welfare population he studies in low unemployment years, than during crises.

Second, the $25 extra payment raised benefit levels for all, even those below the extension limit. An extra $100 dollars per month is not a trivial increase when put into perspective. For instance, average SNAP payments were $146 for one person per month in 2011 (Ganong & Liebman 2013), which includes a $20-25 monthly rise in benefits through ARRA. TANF payments in 2010 were an average of $392 per family. In terms of generosity, this increase raised average UI payments by up to 14% compared to the previous year. As such, fewer crimes would be committed as a result of this increase, by any unemployed person, through the receipt of benefits. The present empirical framework does not allow me to capture the full extent of this

effect, as the instrument induces variation only in the length, not the level of the treatment, but it is likely that if UI does indeed mitigate the effect of unemployment on crime, then this effect would have been substantial. I can only partially capture it through the difference in extension weeks, multiplied by the average payment, and hence only for those with very long spells.

Now consider the case where unemployment also rises in a similar manner in both states. Both the extensions and the benefit increase would then affect the newly unemployed in the same way, compared to a regime with lower benefits and shorter benefit durations. Absent any dynamic feedback effects for the newly unemployed who committed crimes while employed, the expected effect of UI here would have been to *prevent* crime from rising. That is, for the population who did not commit crimes while employed, higher and longer UI payments would have prevented them from engaging in crime, for either short spells (the extra payment) and longer spells (the extensions and the extra payment).

My strategy can thus capture, in a transparent and straightforward fashion, the effect of TUR on average extension length, and average annual level of benefits. On average, TUR states had 3 more weeks of extensions compared to 2008 that non-TUR states. At an average level of around $325 dollars weekly, this amounts to almost $1,000 dollars more over 2008, compared to non-TUR states. It can capture a small part of the higher weekly payments, which are expected to have been substantial.

Can we think of UI as potentially affecting the decision to commit crimes if employed? While it is outside the scope of this paper to answer such a question,

conceptually, if the decision to commit crimes is a dynamic one, and the future loss of income due to an arrest is non-trivial and is factored in the individual decision making, then higher UI can indeed serve so as to reduce the crimes of the employed, by raising income in the unemployed state.[28] [29]

2.7.3 Robustness checks

I conduct two sets of robustness checks. First, for the MEW instrument to be valid, the county should not be too large relative to the state. If it is, then the instrument is not as good as randomly assigned, as its value depends on county level conditions, which may not be fully accounted for by conditioning on county unemployment (which would make the instrument as-good-as-randomly-assigned conditional on county unemployment). One way to check this is to restrict the sample only to relatively small counties. Columns 1 to 3 of Table 2.6 show the results this test, restricting the sample to counties who account for less than 10%, 5%, and 1% of their total state population, respectively. There is little change in the coefficient of ΔUI (the different is not-statistically significant) as the sample is restricted. The strength of the instrument significantly drops in the 1% sample, which is quite small, but it still larger than the 25% maximal bias threshold of Stock and Yogo. Overall, the results do not seem to be driven by the very large counties.

---

[28]The opposite case is also theoretically possible: For employees individuals, higher UI implies higher incomes if they lose their jobs, so they might be more willing to take risks and commit crimes. However, unemployment insurance is typically "no-fault", given only to individuals losing their jobs through no fault of their own, which certainly would not be the case if they are dismissed after commission of a crime.

[29]Engelhardt & Rupert (2008) posit that in their equilibrium search model, higher UI leads to lower crimes for the employed if average jail sentence is longer than average job duration.

Second, I conduct a falsification test. The identifying assumption is that counties in states with the TUR rule would not have had different crime trajectories than those in states without the TUR rule in the absence of the large changes in UI spending. An indirect way of checking the validity of this assumption is to consider the reduced form relationship for the years before the crisis (when activation was very rare), and test the relationship between crime and the TUR rule. Columns 4 and 5 of Table 2.6 show the results of these checks for the change and levels of log property crime, respectively, for the period from 1994 to 2007 (1995 for the change in crime regression). There is no evidence of a robust relationship between crime and the TUR rule before 2008, conditional on covariates.[30]

2.8   Conclusion

In this paper, I test whether unemployment insurance extensions can partially account for the significant drop in crime during the peak of the Great Recession in 2009. I use two different but related instrumental variables from state-level policies in order to instrument for the potentially endogenous UI benefits per unemployed person. The instruments are each valid under different assumptions, and are thus complementary.

My IV estimate of change in UI to change in crime is around -0.015. As this paper is agnostic about why crime fell and the focus is on UI as a mechanism through

---

[30]Another common test in the literature is to run the main regression on the pre-treatment sample for relevant subsamples of the pre-period (in this case, a regression for every year). The results of this test (not shown) are very similar, with the mean of the coefficient centered around zero, with large standard deviation in both cases.

which a rise was prevented, this estimate means that in places with an additional $1,000 rise in UI per unemployed person, crime would have been 1.5% higher were it not for the extensions. Back of the envelope calculations suggest that the extensions account for up to half of the expected counterfactual rise in crime.

These findings add to the debate and our knowledge about crime and public policy in two ways. First, they underline the crime-preventing capabilities of transfer programs. This has been shown in other studies for welfare in the US, and for UI in Europe, but this is the first paper document a causal effect of UI on crime in the US, and it is thus a contribution to the economic of crime literature. Second, they highlight a potentially important additional benefit of unemployment insurance, and provide an additional issue to be considered in the discussion of UI extensions, a highly topical issue in the US.

Figure 2.1: Crime, countries over 50,000, High versus Low Unemployment
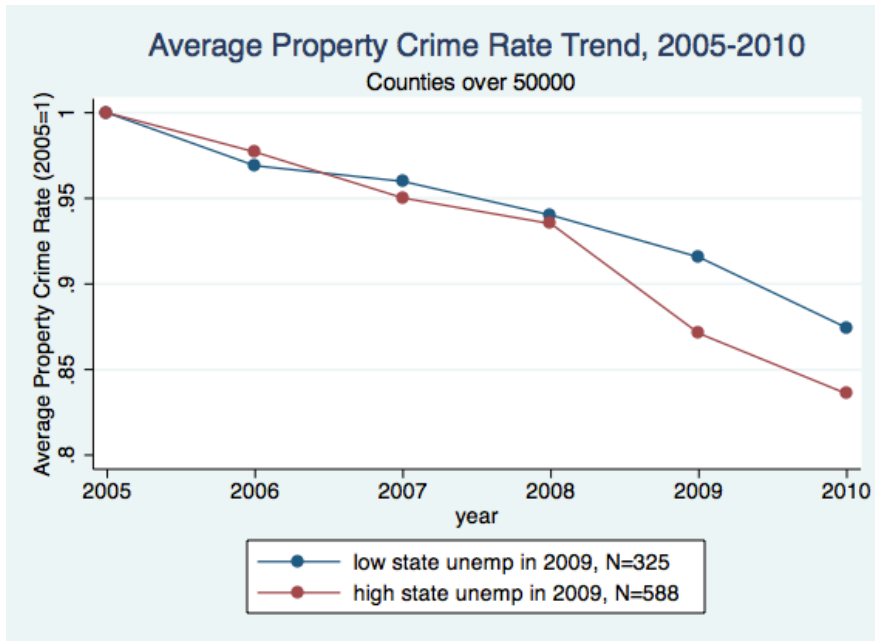

Figure 2.2: Crime, countries over 500,000, High versus Low Unemployment
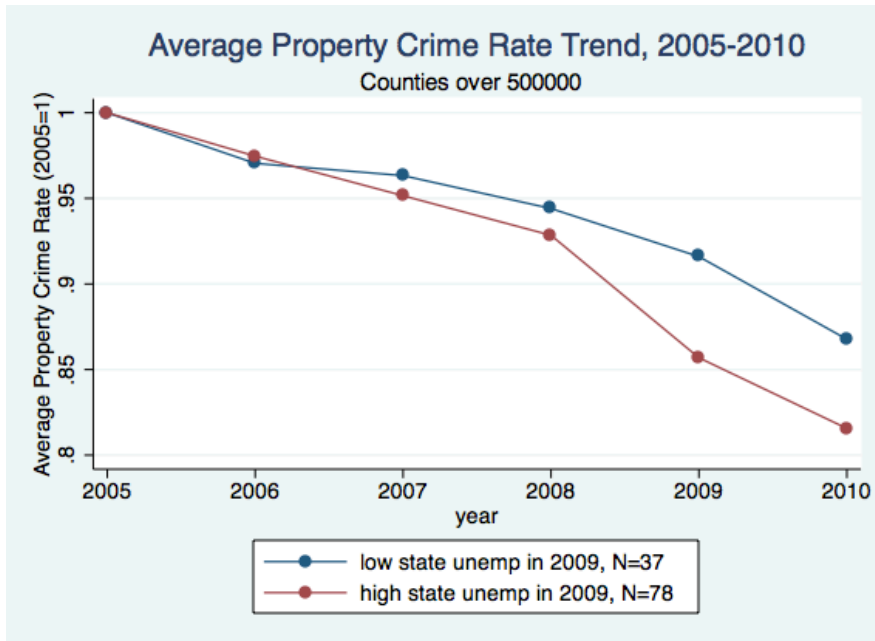
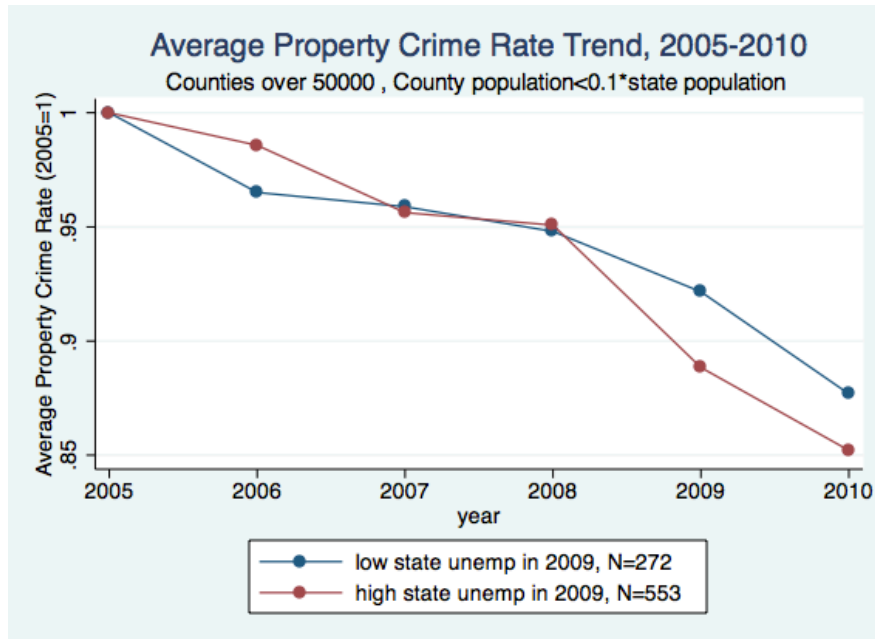Figure 2.3: Crime, small counties over 50,000, High versus Low Unemployment

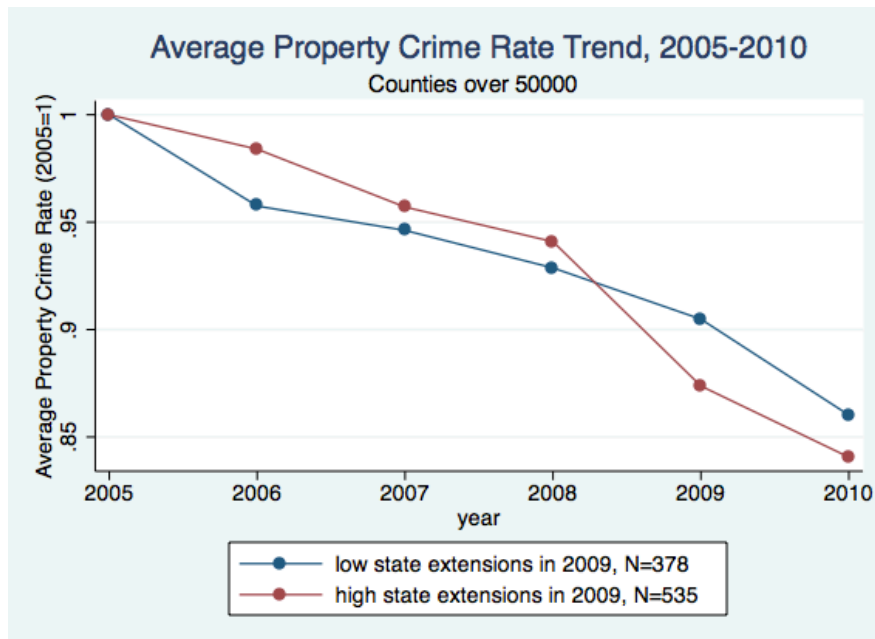Figure 2.4: Crime, countries over 50,000, High versus Low Mean Extension Weeks

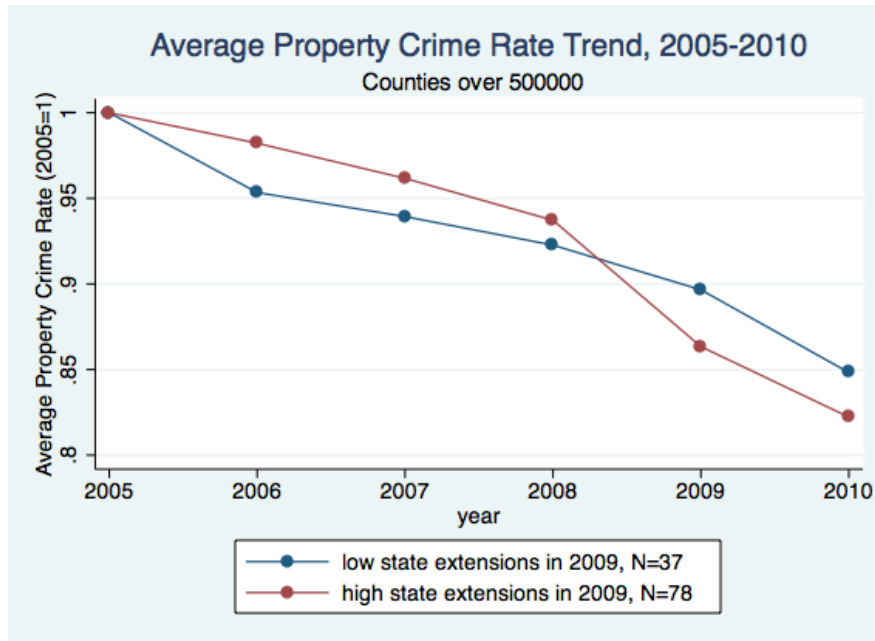Figure 2.5: Crime, countries over 500,000, High versus Low Mean Extension Weeks

Figure 2.6: Crime, small counties over 50,000, High versus Low Mean Extension Weeks
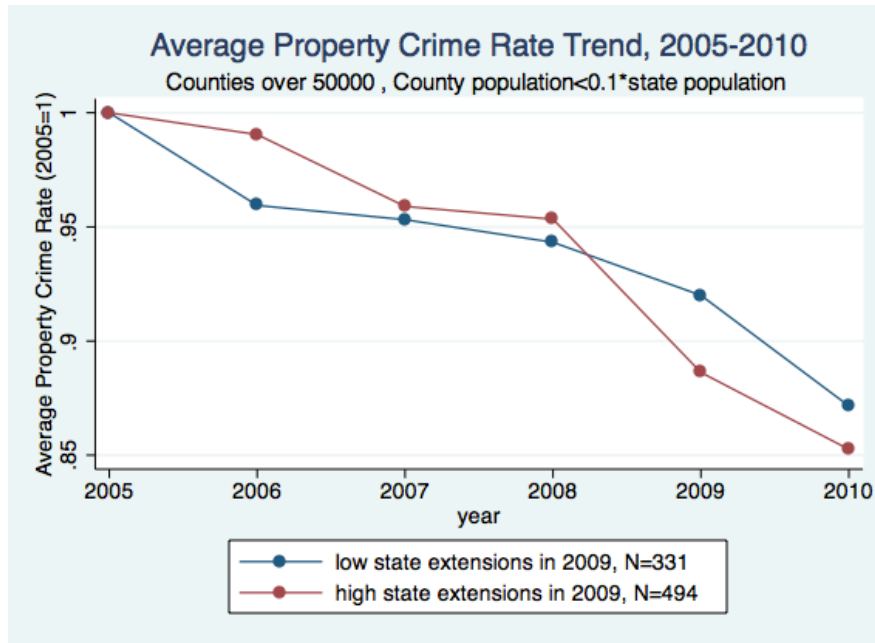
Table 2.1: Summary Statistics - Selected Sample of 907 counties in 2009

	μ	σ	min	max
Property Crime Rate (per 100,000)	2,995	1257	140	10,315
% Change over 2008	-4.68	12.65	-99.54	134.78
UI per unemployed person ($)	8,998	3,253	2,325	32,059
Change over 2008	3,349	1,462	-1,327	23,557
Population (1,000s)	252	449	50	9,787
Unemployment Rate	9.2	2.6	3.6	27.9
Unemployment Rate (State)	9.1	1.8	4.3	13.3
Income per capita ($)	35,282	7,861	17,217	79,902
State avg. wage ($, weekly)	833	117	641	1490

Population Shares

Under age 10	13.0	2.0	5.1	21.4
Age 10-14	6.7	0.9	2.6	9.9
Age 15-19	7.3	1.0	2.8	13.3
Age 20-24	7.0	2.8	2.8	26.3
Age 25-44	25.7	2.7	15.7	43.1
Age 45-64	26.8	2.8	14.5	35.5
Over 65	13.5	3.6	5.2	42.1
Male	49.3	1.2	46.4	60.9
Non White	25.4	18.5	2.3	97.0

Summary statistics for my final sample. The sample consists of counties with population over 50,000 in 2009. Details about the construction of the sample can be found in the Data Appendix.

Table 2.2: Reduced Form Relationship - Change in Log Property Crime

	TUR			Δ MEW	
	(1)	(2)	(3)	(4)	(5)
Instrument	-0.01835*	-0.01778**	-0.0187**	-0.0025***	-0.0027***
	(0.0111)	(0.0089)	(0.0089)	(0.0009)	(0.0009)
Δ State Unemployment		X	X	X	X
Controls			X		X
Observations	907	907	907	907	907

Each column is a separate regression of the change in log property crime rate (the dependent variable) on the instrument and covariates, at the annual level. Standard errors clustered at the state level, and regressions weighted by total county population.

Table 2.3: OLS - Change in Log Property Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔUI	-0.001	-0.002	-0.002	-0.002	-0.002	-0.002	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Δ State Unemployment		-0.0171**			-0.0140**		
		(0.006)			(0.006)		
State Unemployment			-0.0143*			-0.014	-0.015*
			(0.0081)			(0.008)	(0.008)
Lag State Unemployment			0.0114			0.013	0.013
			(0.0128)			(0.013)	(0.014)
Income				X			X
Demographics					X	X	X
Observations	907	907	907	907	907	907	907
R^2	0.000	0.024	0.024		0.035	0.051	0.051

Each column is a separate regression of the change in log property crime rate (the dependent variable) on the change in unemployment benefits per unemployed person, at the annual level. Standard errors clustered at the state level, and regressions weighted by total county population.

Table 2.4: 2SLS with TUR instrument - Change in Log Property Crime

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage - ΔUI					
TUR	1.221***	1.231***	1.307***	1.158***	1.282***	1.258***
	(0.347)	(0.315)	(0.318)	(0.306)	(0.283)	(0.272)
Δ State Unemployment		-0.296				
		(0.222)				
State Unemployment			-0.563*		-0.477*	-.431
			(0.298)		(0.264)	(0.260)
Lag State Unemployment			0.838*		0.693*	0.629*
			(0.419)		(0.393)	(0.370)
Income				X		X
Controls				X	X	X
	Second Stage					
ΔUI	-0.015	-0.014**	-0.015**	-0.015	-0.015**	-0.014**
	(0.001)	(0.006)	(0.006)	(0.010)	(0.007)	(0.007)
Δ State Unemployment		-0.021***				
		(0.007)				
State Unemployment			-0.020***		-0.018**	-0.018**
			(0.007)		(0.007)	(0.007)
Lag State Unemployment			0.020*		0.020*	0.018
			(0.011)		(0.012)	(0.012)
Income				X		X
Controls				X	X	X
Observations	907	907	907	907	907	907
1st Stage R^2	0.114	0.146	0.170	0.199	0.218	0.264
1st Stage F-stat	12.392	15.281	16.863	14.276	20.553	21.418

Each column is a separate IV regression of the change in log property crime rate (the dependent variable) on the change in unemployment benefits per unemployed person, at the annual level. The instrument is a binary {0,1} variable, set to 1 for states with the TUR rule. Standard errors clustered at the state level, and regressions weighted by total county population. The 1st Stage F-statistic is the Kleinbergen-Paap statistic, which accounts for clustered errors.

Table 2.5: 2SLS with MEW instrument - Change in Log Property Crime

	(1)	(2)	(3)	(4)	(5)	(6)
First Stage - ΔUI						
ΔMEW	0.071***	0.145***	0.174***	0.065***	0.173***	0.168***
	(0.023)	(0.029)	(0.032)	(0.024)	(0.030)	(0.034)
Δ State Unemployment		-0.940***			-0.8818***	
		(0.235)				
State Unemployment			-0.713***		-0.726***	-0.701***
			(0.233)		(0.230)	(0.238)
Lag State Unemployment			0.346		0.404	0.374
			(0.357)		(0.366)	(0.347)
Income				X		X
Controls				X	X	X
Second Stage						
ΔUI	-0.035**	-0.013*	-0.013*	-0.043**	-0.013*	-0.014**
	(0.018)	(0.007)	(0.007)	(0.021)	(0.007)	(0.007)
Δ State Unemployment		-0.020***				
		(0.007)				
State Unemployment			-0.019***			-0.018**
			(0.007)			(0.007)
Lag State Unemployment			0.018*			0.018
			(0.011)			(0.012)
Income				X		X
Controls				X	X	X
Observations	907	907	907	907	907	907
1st Stage R^2	0.107	0.316	0.350	0.186	0.366	0.391
1st Stage F-stat	10.046	25.505	30.220	7.352	32.634	24.421

Standard errors clustered at the state level. Regressions weighted by total county population.

1st Stage F-stat is the Kleinbergen-Paap statistic, which accounts for clustered errors.

Each column is a separate IV regression of the change in log property crime rate (the dependent variable) on the change in unemployment benefits per unemployed person, at the annual level. The instrument is the change in the mean number of weeks of extensions. Standard errors clustered at the state level, and regressions weighted by total county population. The 1st Stage F-statistic is the Kleinbergen-Paap statistic, which accounts for clustered errors.

Table 2.6: Robustness Checks - Log Property Crime

	(1)	(2)	(3)	(4)	(5)
	IV - MEW instrument				
	Δ				
Δ UI	-0.0174**	-0.0208***	-0.0157**		
	(0.0068)	(0.0080)	(0.0093)		
Δ State	-0.0215***	-0.0211**	-0.0268		
Unemployment	(0.0066)	(0.0088)	(0.0166)		
Income	X	X	X		
Controls	X	X	X		
Observations	825	709	229		
1st Stage R^2	0.371	0.336	0.284		
1st Stage F-stat	28.654	20.099	6.112		
				OLS	
				Δ	Level
TUR				-0.0080	0.0449
				(0.0071)	(0.0744)
Δ County				0.0016	
Unemployment				(0.0071)	
County					0.0251**
Unemployment					(0.0111)
Income				X	X
Controls				X	X
Observations				11502	12474
R^2				0.007	0.318

Standard errors clustered at the state level. Regressions weighted by total county population.

1st Stage F-stat is the Kleinbergen-Paap statistic, which accounts for clustered errors.

In the top panel, each column is a separate IV regression of the change in log property crime rate (the dependent variable) on the change in unemployment benefits per unemployed person, at the annual level. The instrument is the change in the mean number of weeks of extensions. Standard errors clustered at the state level, and regressions weighted by total county population. The 1st Stage F-statistic is the Kleinbergen-Paap statistic, which accounts for clustered errors. In the bottom panel, each column is an OLS regression of the dependent variable (change or level of log property crime) on the TUR instrument. Standard errors clustered at the state level, and regressions weighted by total county population.

Chapter 3: The Role of Taxes and Trade Costs in the Greek Recovery

3.1 Overview

Despite an unprecedented reduction in labor costs, Greece has failed to engineer an export-led growth during the recent crisis. Exports did not rise, falling slightly in 2012, all the while other crisis hit countries in the euro periphery achieved large export gains with much more modest wage reductions. I study the role of taxes and trade costs in explaining the failure of Greece to boost its export sector. While the wage cuts were taking place, energy tax hikes sharply raised the cost of energy for industrial production. VAT, which disproportionately affects tradables, also rose, while there was no reduction in the substantial trade costs burdening Greek exports. After a detailed discussion of the theoretical and empirical literature on the effect of these factors on exports, I conduct a set of quantitative exercises in the context of a small-scale New Keynesian small open economy model, to gauge their relative importance in explaining poor export performance. I find that trade costs are crucial in explaining why exports did not rise; a 20% reduction in trade costs (to the level of Portugal) would mean 8% more exports within 10 quarters. Similarly, VAT and energy costs can partially explain why exports fell.

3.2   Introduction

The euro crisis started in 2010, shortly following the global financial crisis, and was predominantly characterized by severe adjustments on the part of the peripheral countries (Portugal, Ireland, Greece, Spain, and secondarily Italy), who attempted to reduce current account and budget deficits. Figure 3.1 plots the current account as a percentage of GDP for the PIIGS countries and Germany, and illustrates the imbalances that were generated with the introduction of the euro in the late 1990s, as well as the adjustment after 2010.

Figure 3.1: Current Account in the EU Periphery

Source: Ameco
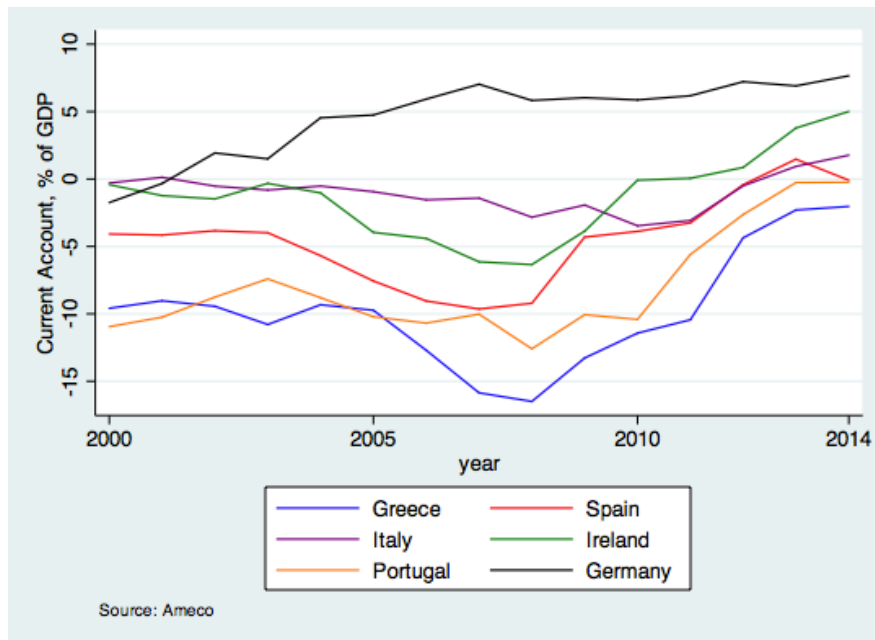
These current account adjustments are substantial, and even more remarkable when taking into account that all these countries are members of a currency union, where intra-union trade accounts for more than 80% of total trade. This meant that the current account adjustment could not take place through a currency devaluation,

and so the reduction in the current account deficit (including a whopping 15% of 2008 GDP from 2008 to 2014 for Greece) was achieved through a combination of falling imports and rising exports. To achieve this, these countries engaged in a so-called "internal" devaluation process, where wage reductions in the periphery compared to the core would correct the competitiveness problem of these countries, compared to the core, a result of easy credit and a consumption boom after the introduction of the euro in 1999.[1] Figure 3.2 shows the path of the nominal unit labor costs for the PIIGS versus Germany since the year 2000.

Figure 3.2: Nominal Labor Costs in the EU Periphery
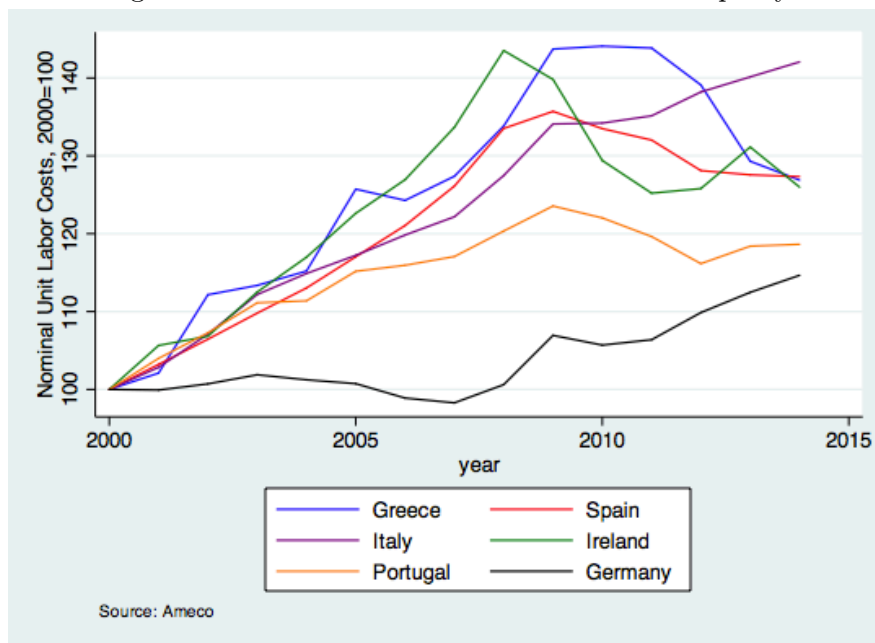
Starting in 2010, and with the exception of Italy, a large part of the cost gap with Germany was significantly reduced. Greece is again the country with the sharpest adjustment, showing a reduction of 12% from 2010 to 2014, a much larger fall than the other countries (though Ireland has been reducing labor costs since

---

[1]Greece entered the euro in 2001. Physical currency was introduced in 2002.

2007, as it was affected at a much earlier stage of the crisis than the rest of the PIIGS).

Combining then the substantial current account consolidation and the improvement in competitiveness would suggest that exports must have increased. This is indeed the case for Portugal, Spain, and Ireland, but not the case for Greece. In Figure 3.3, I update a graph from Arkolakis et al. (2014) to include 2014 data. The figure shows the cumulative contribution of exports and imports in the trade balance consolidation from 2007 to 2014, as a percentage of 2007 GDP. Falling imports have a positive contribution to the trade balance, and are recorded as positive. Ireland has had a remarkable export performance (allowing it in fact to also raise imports substantially), while Spain and Portugal both reduce imports and raise exports. Greece, on the other hand, has achieved its large adjustment exclusively through a steep fall in imports, accompanied by a very small fall in exports.

Figure 3.3: Contribution to the Trade Balance Consolidation
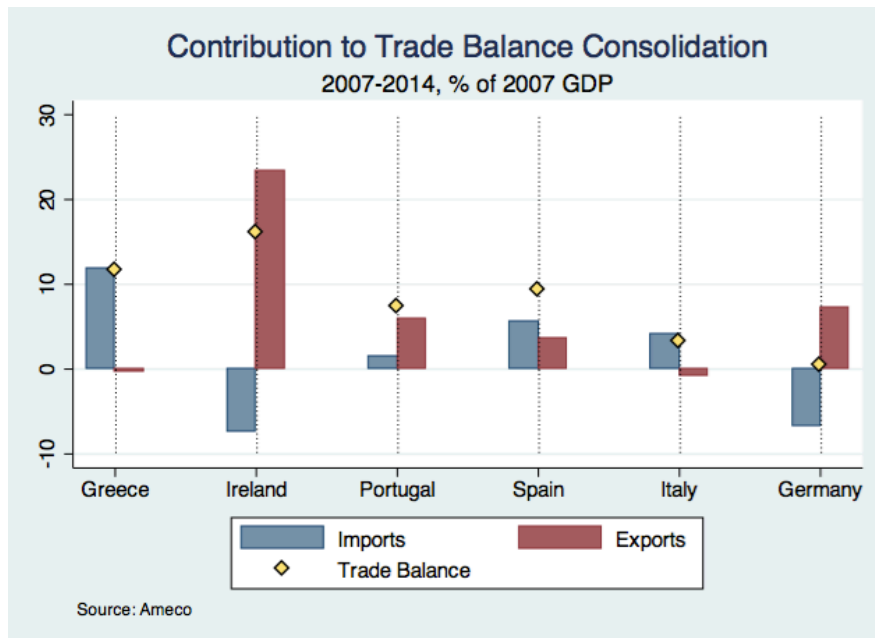
This picture poses a puzzle. In a classical model with fully flexible wages and prices, falling labor costs would imply higher exports. More realistic models, which allow for price rigidities and an imperfect passthrough of lower domestic export prices to lower foreign import prices (see Burstein & Gopinath (2014) for a recent survey), suggest that passthrough is imperfect and varies substantially across countries, but this fact would still not be able to explain why Ireland or Portugal have managed to grow their own exports. This suggests that there is something unique to Greece.

The purpose of this paper is twofold. First, to extend a recent literature that has considered possible reasons behind this adjustment failure, by providing a holistic analysis of the issue. Other studies have looked at energy (Mitsopoulos 2014) or price rigidities (Arkolakis et al. 2014) separately. I complement these studies by including a more extensive analysis of the role of taxes and logistics, which have been relatively unaddressed. Second, to provide a unified treatment of all these issues, by conducting a set of quantitative exercises in the context of a standard small-open economy model with a currency union, allowing for VAT imperfections, energy taxes, and logistics costs. This approach can provide an illustrative common analysis of these issues and potentially inform policy on the importance of each factor.

Taxes and distribution costs could in theory account for a significant fraction of the failure of export-led growth to pick-up in Greece. After the 2010 bail-out by the so-called "troika" comprised by the International Monetary Fund (IMF), the European Commission (EC), and the European Central Bank (ECB), strict

conditionality rules were imposed on Greece, which aimed at reducing a massive fiscal deficit, over 15% of GDP in 2009. In order to fulfill its fiscal goals, the Greek governments took a series of deficit fighting measures, with substantial tax hikes across the board, including, inter alia, a hike on VAT and the taxation of energy. Consequently, prices rose in Greece for the first few years of the adjustment, despite a sharp reduction in labor costs. In addition, an inefficient distribution sector prevented these gains in competitiveness from manifesting in a rise in exports. I focus on these two taxes and study their impact in the recovery.

The excise taxes on energy (electricity, oil, natural gas) rose substantially for both consumer and industrial use. At the same time, the state-controlled Public Electricity Corporation, which holds a virtual monopoly on the Greek electricity market, substantially raised the cost of electricity, as an indirect means of collecting taxes. All in all, this resulted in a rise of up to 60% in the cost of energy for industrial use in Greece since 2009 (Mitsopoulos 2014).

In turn, VAT also rose substantially and in a fragmented fashion since, as is typical for VAT, there are various exemptions and special rates. On the whole though, the VAT hike contributed significantly to the overall rise in the price level in the earlier years of the adjustment, up to a third according to Arkolakis et al. (2014). At the same time, as pointed out first by Feldstein & Krugman (1990), the VAT has a pernicious trade-adjustment property, namely the fact that it is higher on tradable than non-tradable goods. In a textbook model (Vegh 2013), a negative demand shock will reduce demand for both tradables and non-tradables goods, but since the latter can only be produced domestically, there will be a corresponding

labor shift to the tradable sector. However, if taxes on tradables are higher, this shift will not take place.

Finally, an inefficient logistics sector can be a significant barrier to trade. The land distribution sector is especially problematic, characterized a fragmented trucking industry, which is primarily based on a single truck ownership system. This in turn means that truck companies are too small to enjoy economies of scale and reduce trade costs. This is magnified by the virtual absence of a commercial rail system, implying that the substantial infrastructure improvements in the Port of Piraeus, the largest port in Greece, cannot be exploited by domestic exporters.

I find that trade costs have a very significant negative effect on exports; lowering trade costs by 20%, to the level of Portugal, within ten quarters, would have 8% higher exports, and 2.5% higher output. VAT and energy taxes are much less important in the steady state; VAT has a moderate effect on exports and a small effect on output, and opposite for energy taxes. However, together they can account for part of the decline in exports (as opposed to the lack of growth), since in the absence of these two tax hikes, exports would have been 3% higher within 5 quarters.

3.3 Background

3.3.1 Internal Devaluation

The basic idea of an internal devaluation that could boost competitiveness in the presence of fixed exchange rates goes back to Keynes, who was writing for the Gold Standard period, and argued that an export subsidy and an import tariff

could mimic the effect of a currency devaluation, by reducing the relative price of home tradables (de Mooij & Keen 2012). As such predatory tax policies are illegal in the EU, the more recent discussion of the same concept evolved along the lines of the so-called "fiscal devaluation", where a reduction of employers' social insurance contributions (SCR) and a rise in VAT would bring about a revenue-neutral improvement in competitiveness. The mechanics of such a fiscal devaluation would be as follows. Starting from an initial low employment, high sticky nominal wage position, a reduction in SCR would raise labor demand and thus employment, as the labor cost would fall even with fixed nominal wages. The rise in VAT would serve a double purpose. First, it would raise the price of domestic consumption (but not exports), and so make domestic tradables more attractive only to foreign consumers. Second, it would ensure the reform is revenue neutral (International Monetary Fund 2011). Note that flexible wages would negate this effect, as workers would demand higher wages, while a flexible exchange rate would appreciate after the increased demand for home goods. This is of course redundant, as with flexible nominal wages and prices the adjustment would take place through the market and there would be no need for a fiscal devaluation.

Farhi et al. (2014) provide theoretical support for this idea, by providing conditions under which a combination of taxes and subsidies could mimic the real effects of a nominal devaluation under fixed exchange rates, in the context of a standard New Keynesian small open economy model with sticky prices. They also show the equivalence of this new approach of fiscal devaluation with the older Keynesian idea of tariff-cum-subsidy. Interestingly, they show that, in the context of a currency

union, fiscal devaluations can be unilaterally engineered and be effective without accommodating monetary policies, as long as the size of the country is sufficiently small relative to the union. This simple tax policy is only really equivalent to a nominal devaluation under certain conditions, some restrictive (namely that the policy is unanticipated). Otherwise, under sticky prices, the VAT rise will raise the real exchange rate, the exact opposite of a nominal devaluation, and the combination of a consumption subsidy with a change in the income tax is needed to offset this effect, making the policy quite complicated. Furthermore, while a nominal depreciation would affect the export prices of all goods in an identical way, the effects of fiscal devaluation will be more pronounced for labor-intensive goods, and since non-tradables tend to be more labor intensive, the required reallocation will be hard to achieve International Monetary Fund (2011).

In any case, fiscal devaluation envisioned as such was never really implemented during the crisis, although it was tried to some extent in Denmark and Sweden in the late 1980s and early 1990s, but also in Germany in 2006, within the context of a currency union. If anything, the crisis countries in the eurozone periphery would be better placed to run successful fiscal devaluations of the Farhi et al. (2014) type, as they are quite small relative to the union, and so they would not require monetary accommodation. An explicit fiscal devaluation approach was part of the bail-out deal for Portugal but was eventually scrapped. France announced such a plan in 2012 but never implemented it.

3.3.2 VAT and trade

There is an old argument about the competitiveness gains of VAT. The argument rested on the mechanics of VAT as a "destination-based" taxed. It is imposed on goods consumed domestically, but not on those exposed, and so it can act as a tariff-cum-subsidy on its own. However, as Feldstein & Krugman (1990) showed, for the case of a uniform VAT, this argument misses the fact that domestic exporters will equalize the prices they demand both home and abroad. In a small open economy that is a price-taker in international markets, this means that the home price of exportable goods is simply equal to the international price plus the tax. The domestic importers will behave similarly, and thus the domestic price of imports will also equal to the international one plus the tax. Finally, the imposition of the VAT will raise the price of non-tradables in the same way, and so all absolute prices rise by the same rate, leaving relative prices unchanged. This makes VAT trade-neutral. Even when trade costs are taken into account, imposition of VAT does not affect the pre-tax equilibrium. The crucial element of VAT that makes it trade-neutral is also what has given rise to the competitiveness misconception, namely that it is rebated on exports but imposed on imports. However, all the export rebate does it to let the export enter the foreign market untaxed from home, so that it is only affected by foreign taxes. Similarly, the tax on imports merely ensures that the imported goods are subject to the same domestic tax policies as home goods.

This argument makes clear the fact that, in principle at least, the proposed fiscal devaluation approach needs a rise in VAT only for the policy to be budget-

neutral, as VAT in theory has no effect on trade. The trade-improvement part of fiscal devaluation strategies relies on the fact that VAT may substitute for other taxes that are not trade neutral, in this case the payroll tax, though this analogy is more common with regards to the income tax. Both income and payroll taxes are inherently trade reducing in the short-run, as they discourage savings, which, through the national account identities, mechanically result in a higher trade deficit. Such arguments are especially common in the United States, which does not have a VAT, but instead a sales tax, typically imposed at rates much lower than VAT in other OECD countries. Revenue-neutral tax reforms, where VAT replaces the income tax, are frequently proposed in the US. Feldstein & Krugman (1990) recognized this possibility for the case of an income tax, while Calmfors (1998) first made the argument about using the VAT and the payroll tax to engineer a fiscal devaluation in the EMU.[2]

It should be clear that VAT is trade-neutral in the sense that it does not distort relative prices of imports and exports, and hence it is not a beggar-thy-neighbor, expenditure-switching policy. However, there will in general be intertemporal effects from the imposition of VAT, just like with any other policy. For example, according to standard theory, an anticipated rise in VAT is expected to result in higher current consumption (and consequently higher current account deficits) now, as consumers front-load consumption, by engaging in intertemporal arbitrage. Similarly, a (temporary) unanticipated rise in VAT will bring about a reduction in total

---

[2]In the long-run, the present discounted value of the trade balance has to equal zero for every country, and so any trade-improving has to reverse.

consumption, and hence an improvement in the trade balance, along with a real exchange rate depreciation, which will be reversed when the VAT falls again. When taking the presence of non-tradables more explicitly into account, a uniform rise in the VAT reduces consumption for both tradables and non-tradables. If tradable goods prices are internationally set, relative prices of non-tradables will fall, shifting production to tradables (Vegh 2013). This intertemporal distortion generates an expenditure-changing effect.

There is in fact a lot of evidence that anticipated VAT changes do generate this theoretical effect. The main policy change studied is the VAT hike in Japan in 1997, where Cashin & Unayama (2011) find that an anticipated rise in VAT from 3% to 5% led to an significant increase in total expenditure consumption in the three months preceding the change (0.61, 3.24, and 8.85% relative to the counterfactual). The observe that over three quarters of this change is due to durables. As reported in de Mooij & Keen (2012), there is also similar evidence of responding to VAT changes for Germany and the UK, and the upshot is that this intertemporal effect may be substantial, but short-lived, and primarily affecting durables. To my knowledge, there has been no study explicitly looking at this intertemporal effects of VAT on the trade balance, but the connection is clear, especially if the response is mostly due to durables, as countries like Greece have little domestic production of durables.

The clear theoretical prediction about the trade-neutral nature of the VAT notwithstanding, reality is more complicated. Two main assumptions needed to hold for VAT to indeed be neutral.[3] First, the tax needs to be uniform, imposed

---

[3]In addition, if VAT is imposed on an origin-basis, flexible exchange rates are needed to ensure

at the same rate on all goods. Second, the rebate system needs to be properly functioning. I consider these issues in turn.

As Feldstein & Krugman (1990) made clear in their exposition, the trade-neutrality of the VAT is only true in what they call the *idealized* version of the tax, where it is imposed uniformly on all goods. If that was not the case, then the result would not hold. They made the specific distinction between tradables and non-tradables goods, as it is well known that non-tradables are typically taxed at a lower rate. Part of the reason why this is the case is administrative complexity in taxing certain non-traded activities, such as household production and, more importantly, the informal sector. This is especially crucial for Greece, which has the one of the largest informal sectors relative to GDP among OECD countries (Schneider & Buehn 2012). Indeed, to the extent that informal activity is predominantly non-traded, there exists a natural tax-advantage to non-tradable production. Furthermore, modern welfare states typically tax-discriminate in favor of certain goods and services, such as healthcare and education, for equity and fairness reasons. Such goods and services are also typically non-tradable, magnifying the relative tax distortion between the two types of production.

The effect of the unequal tax treatment of tradables and non-tradables is straightforward. Non-tradables become relatively cheaper, so consumption, and hence production, shifts to that sector. This is especially problematic for Greece in the current juncture as the recovery strategy hinged on exactly the opposite

---

neutrality. The policy-relevant case is that of destination-basis taxation, so I will not dwell on this special case.

goal, an export-led growth through a resource shift towards the tradable sector. The precise impact of this wedge is very hard to estimate, not least because of the large practical difficulties in precisely delineating tradables and non-tradables from the data. Nevertheless, relative elasticities with regards to the different types of goods are likely to matter regarding the effect of a VAT hike on trade. If, for instance, tradables consists of more inelastic commodities than non-tradables, the VAT hike may have no effect on trade at all, as consumers do not change their spending patterns. This is a very complicated question to answer, and an interesting topic for future research[4], intuition would suggest that for a country like Greece, with domestic production of most staple goods, non-tradables are less elastic than tradables, which, with the exception of energy, tend to be elastic goods, such as durables (citation needed). Furthermore, according to Feldstein & Krugman (1990), and standard models in general (Vegh 2013), the higher demand for non-tradables following the hike will be followed by a rise in both price and production of non-tradables (by a shift in production), as this demand shift cannot be accommodated by imports.

The need for a properly functioning of the rebate system in order to ensure the trade neutrality of VAT is clear. In the extreme case with no so-called border adjustments (no rebate on exports, no VAT on imports), the system becomes origin-based, and the neutrality is preserved, with the additional requirement of price and/or exchange rate flexibility. In the more empirically interesting case of no or partial rebate on exports and a full tax on imports, VAT becomes a protectionist

---

[4]There is no, to my knowledge, any empirical study examining this dimension.

policy. Recall that with a full rebate the domestic exporter is indifferent between the international price when exporting, and the tax inclusive price domestically, the domestic price must be equal to the international price plus the tax. By contrast, in a system with no rebate, the producer will pay the tax no matter where the good is sold, and so the domestic *consumer* price will equal the world price. This has the effect of reducing the relative price of exportables over imports domestically. As such, non-rebate of the VAT amounts essentially to an export tax, and, at least in the short run, lower profits for exporters are likely to lead to a *fall* in trade. Feldstein & Krugman (1990) pointed out that this much lamented feature of VAT by non-economist is precisely what makes VAT trade-neutral. In the more realistic case of a partial rebate, the domestic price of exportables will be somewhat higher than the world price, but still lower than in the case of a full rebate.

There is are further nuances regarding the property functioning of the VAT system that is specific to Greece. As is well-known, Greece has chronic tax-collecting problems, relating both to the size of the informal sector (particularly self-employment), as well as a culturally lax attitude towards tax collection. It was commonplace in the years before the crisis for retail firms and the self-employed to either not give receipts of service, or to charge a higher price to customers asking for a receipt. As VAT is collected on sales, this was a particularly effective strategy to avoid paying VAT, and one of the first initiatives of the Greek government after the troika programs started was the incentivize consumers to take receipts. However, as the export sector provides a de-factor formal sector and firms that trade internationally are typically larger and more cash-rich, the government routinely delayed, often for

128

a long time (citation needed) rebating VAT paid on intermediate inputs. As the domestic and intra-EU VAT rebating system is fully automated, the companies affected were those who imported intermediate inputs from outside of the EU and also exported their goods outside of the EU (citation needed, numbers too).

A similar VAT irregularity that was particularly unpopular for several years (but was recently stopped) was the demand by the government for businesses to pay a part of the VAT proceeds they were expecting to receive throughout some future period of time before actually collecting the tax. The rules surrounding this practice were highly erratic, but most damaging to businesses was the fact that the pre-payment was based on past sales, a requirement imposing very tight cash constraints on businesses who saw their sales falling by record levels for several years in a row. It is less obvious how this practice affected trade specifically.

A final theoretical issue of note is the difference between a sales tax (a tax on retail sales only) and a VAT. With a properly functioning VAT rebate system, a sales tax with a destination-based VAT are ex-ante identical. Two main differences may emerge. First, if exporters are not properly rebated, then VAT may have the unintended effects described above. A sales tax is by construction imposed only on retail consumers and does not need border adjustments. Second, VAT is more "evasion-proof" with regards to exports than sales taxes. Along the value-chain, the VAT may be rebated to buyers and sent to the tax authority by sellers several times. As such, several independent parties will need to engage in evasion for tax revenue to be completely lost to the state. By contrast, a sales tax is completely dependent on the retailer, and no tax at all is collected if the retailer fails to remit

their proceeds. This does not mean of course that evasion is not an issue regarding the VAT, quite the opposite, especially in cases where retail firms impose large mark-ups.[5] However, there is a sense in which revenue is more secure under VAT than sales taxes(de Mooij & Keen 2012).

Putting all this together, theoretical predictions imply that the effect of VAT on trade is not clear, and for a long time such debates were confined to theory, but the prevalence of rich data in the last few years have allowed researchers to put the theory to the data. Keen & Syed (2006) focus on OECD data, and consider the VAT together with corporate taxes. Their results are sensitive to the use of VAT variable, and to whether they account for corporate taxes. If the latter are excluded, VAT has a significant relative correlation with (net) exports, which goes to zero when account for the corporate tax. In a dynamic analysis, the VAT is irrelevant when using VAT revenue over consumption, or the standard VAT rate, but highly negative and significant if using VAT revenue over GDP. In the latter case, the effect is sharp but short-lived.

Desai & Hines (2005) use a very large cross-section of 136 countries for a single year (2000), and find that reliance on VAT is a significant trade hurdle: VAT reliance implies one-third fewer exports, while 10% higher VAT revenue imply two percent fewer exports. A similar, though lower, effect, is found when using a long but unbalanced of 168 countries, from 1950 to 2000. They conjecture that their results are primarily driven by the asymmetry with regards to non-tradables, and by

---

[5]One of the most visible reform initiatives in Greece was the attempts by authorities to curb tax evasion in retail, by inducing customers to demand for their receipts.

incomplete rebates to exporters. They confirm the cross-country data with evidence from affiliates of US multinational firms, who also export less when they operate in countries that rely on VAT. They also find that VAT is associated with a less open economy, though Keen & Lockwood (2007) point out that this may indicate reverse causality, where open economies are less likely to adopt VAT. Naturally, this does not apply to the EU, which is a customs union with VAT.

Nicholson (2013) looks more closely at the case of the United States competitiveness, and how it is affected by the fact that it is the only OECD country that does not use VAT. His panel is short, but has sectoral data, for 29 separate industries, which allows for a richer specification than Desai & Hines (2005), and he tests a gravity model of trade in relation to the US. He finds that VAT on other countries has a robust significant positive impact of US exports, and a negative (though less robust) impact on imports, which suggests that VAT negatively affects exports of adopted countries.

The upshot of those studies is that the evidence is decidedly mixed, and more work is needed for an issue that is fundamentally very hard to identify. However, even though the estimates provided are not causal, given that the null hypothesis assumes no effect whatsoever, the results should at the very least the possibility that the non-neutrality of VAT should be considered as a real possibility. It should also be noted that a complication of studying the VAT impact of trade in OECD countries is that there almost no cross-sectional variation (every country but the US, and Australia before 2000, has VAT), so all variation comes from relative rates in a pooled model, or from time series variation if data before and after VAT introduction

are available. In that case, as the introduction of VAT coincided with major trade agreements (most notably the EU, where VAT was widespread from the very early stages of the union, or adopted it upon entry), estimates of the effects of VAT on trade are unavoidably confounded.

As regards to the relative intensities of VAT across tradables and non-tradables in Greece, there are no, to my knowledge, specific estimates, owning both to the complexity of the VAT system, and to the fact that exact definitions of tradables and non-tradable sectors are equally complex. The statutory level rose from 18% to 23% in 2010, with two special categories rising to 13% (reduced) and 6.5% ("ultra-reduced"), up from 9% and 4.5%. The statutory rate is 30% on the three categories for all islands in the Aegean except for Crete. The ultra-reduced rate applies to certain cultural categories and hotels, while the reduced rate applies to foods and related categories, and most non-tradable services (dining a notable exception). Health and education are fully exempt from VAT. These, together with the fact that the large informal sector is by nature non-tradable and VAT exempt, imply that there exists a significant tax burden on the tradable sector.

Portugal and Ireland, by contrast, started from a higher VAT rate. Ireland had a maximum rate of 21%, only raising it to 23% in 2012. In Portugal, a rate of 20% rose to 23% in 2011. As such, the desired shift to tradables was mitigated to a larger extent in Greece than Ireland or Portugal, due to the larger rise in the statutory rate.[6] Even more important is the level of informality and the extent of

---

[6]It is the change, and not the level, that is under consideration here. Thus, it is the change in VAT, not its level, that is relevant.

non-compliance, both of which contribute to the divergence from trade-neutrality. A sufficient statistic to measure this divergence is the VAT Revenue Ratio (VRR), published by the OECD, which measures the deviation of actual VAT revenues from an ideal system with perfect compliance and a uniform comprehensive rate. In 2012, VRR in Greece was 0.37 (with 1 indicating an ideal system), compared to 0.47 for Portugal, and 0.45 for Portugal.[7] Finally, Greece has a much larger informal sector than Portugal or Ireland, which, as discussed above, is by construction not affected by VAT and exclusively non-tradable. Specifically, the size of the informal sector was estimated to 25.1% of GDP in 2010 in Greece, versus 22.2% in Portugal, and 16.5% in Ireland (Schneider & Buehn 2012).

3.3.3 Energy

Rising energy costs have been widely blamed as one of the reasons for the underperformance of exports in Greece. The particular geography of Greece is certainly a factor. The large network of islands makes the existence of a single unified grid difficult, if not outright impossible, while the highly mountainous inland makes maintenance of the continental grid costly as well.

The energy market is dominated by the state-controlled Public Power Corporation (DEI) in both the retail and wholesale sectors, though there has been a significant reduction in DEI's market share in the past few years, from 85.6% of the total energy market in 2009 to 66% in 2011 and 2012[8]. The fall in DEI's market

---

[7]Source: http://www.oecd.org/tax/consumption/consumption-tax-trends-19990979.htm

[8]DEI annual reports for several years, https://www.dei.gr/en/i-dei/enimerwsi-ependutwn/etisia-deltia.

power seems to have leveled off, as it was primarily due to the growth of the natural gas network (also primarily controlled by the recently privatized DESFA), whose development has stalled, and the emergence of a few small private operators, the largest of which have seized operations following an embezzlement scandal which cost the state revenue amounting to over 0.1% of GDP.[9] Around half of DEI's energy derives from lignite, a domestically extracted low-quality (and so both costly and highly polluting) type of coal.

Despite the gradual opening of the energy market, two developments during the crisis years have resulted in a large rise in energy costs, for both retail consumers and companies. First, as part of the initial debt consolidation efforts by the government, taxes on energy rose significantly. This rise was primarily due to a hike in excise taxes, which are non-refundable. As taxes vary by type and intensity of use, it is more instructive to look at tax revenues by energy taxes. Even though energy taxes in Greece were quite low compared to the rest of the EU before 2009, tax receipts rose by 60% from 2008 to 2011 (when they peaked, falling slightly in 2012), whereas they fell in Portugal and Spain, rose modestly in Ireland (by 13%) and more significantly in Italy, rising by 25%.

Second, the government started leveraging its control of the energy market, through the DEI bills, to use energy prices as an indirect source of revenue. One approach that caused much controversy was the imposition of several emergency "solidarity contributions" tied to DEI bills for retail consumers. An approach more

---

[9]http://www.kathimerini.gr/773645/article/epikairothta/ellada/se-dikh-oi-19-twn-energa-kai-hellas-power.

relevant to this paper was the rise in the price demand for energy by DEI. According to official Eurostat data, tax-inclusive electricity prices for industrial production in Greece, though certainly higher than the EU average, are not excessive in comparison. The price of a unit of electricity for energy intensive companies was 7 cents in 2013 for Greece, compared to 8.4 for Ireland, 7.8 for Spain, 8.3 for Italy, and 5.9 for Portugal. However, as pointed out by Pelagidis (2014), simply comparing energy prices charged for industrial use across countries is highly misleading. The reason is that it is standard practice for energy-intensive companies across Europe to negotiate individual agreements at well-below official market rates with energy provides, and these agreements are considered industrial secrets, and hence not included in averages quoted in Eurostat. In many countries, such agreements account for over 50% of total industrial energy use, which means that Eurostat-quoted prices substantially overestimate true energy costs. By contrast, Greece does not have such agreements, and hence the price recorded is the actual price paid. While precisely quantifying this disadvantage is not straightforward, an estimate is that energy-intensive Greek companies have to pay up to 80% more than similar firms in other EU countries (Pelagidis & Mitsopoulos 2014).

The combination of tax hikes and tax-like price hikes for energy use, combined with high borrowing costs, are, according to Pelagidis (2014), the chief contributor to the failure of export led growth to lead the Greek recovery, in spite of the fall in labor costs.

3.3.4 Logistics

Another important hurdle in spurring export growth in Greece is its fragmented and inefficient logistics sector, which refers both to the distribution sector, but also the various bureaucratic entities related to trade, such as customs officials. An indicator of how important this issue is perceived to be by officials is the fact that it led the World Bank to publish a special report on the logistics sector in Greece (World Bank 2013), a rare move for a developed country. This section explores the tax imposed on trade by the lack of an efficient logistics sector in some detail.

Although a catchall measure of trade costs is not straightforward, a standard such measure is provided by gravity models of international trade. Here I use a cost metric available from the World Bank Trade Cost Database, constructed according to the Inverse Gravity framework of Novy (2013). The gravity model predicts bilateral trade by the size of two countries and the distance between them, and is the workhorse model of the international trade literature. Novy (2013) derives a method to capture trade costs using only observable data, and comes up with a measure that combines transportation, tariffs, and other less tangible trade barriers, including red tape or poor law-enforcement. The cost measure is a tariff-equivalent, measuring the difference between actual trade flows and those predicted by a frictionless model.

Figures 3.4 and 3.5 provide graphical evidence of the burden the broader logistics sector imposes on Greek exports. The graphs show trading costs by trading partner for Greece and the other peripheral economies when trading with the three largest EU economies, Germand, France, and the UK, plus the United States. Figure

3.4 gives data for 2007 and 3.5 for 2012, the last year of data availability.[10]

Figure 3.4: Trade Costs by Trading Partner: Euro Periphery, 2007

Source: WB Trade Costs Database
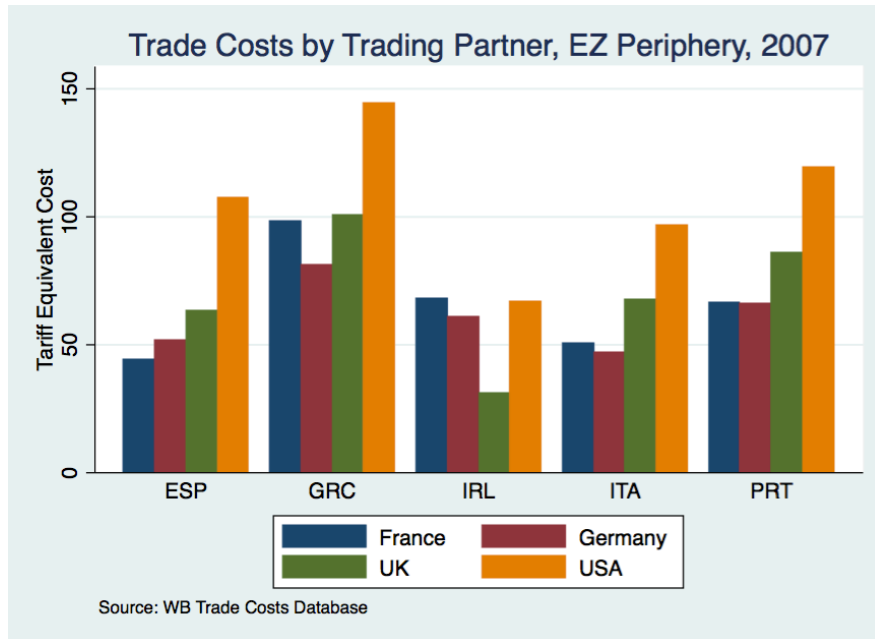
The figures clearly show that trade in Greece faces much larger trade costs compared to the other peripheral countries. In 2007, Greece faced 22% higher costs when trading with Germany compared to Portugal, 35% compared to Ireland, 55% compared to Spain, and 72% compared to Italy. The numbers when trading with France are 46%, 44%, 122%, and 96%, respectively. The situation is similar for trade with the UK and the US. Interestingly, even though there are no official trade barriers of any kind within the European Union, Greece has a higher cost of trading with the UK, an EU member, than Italy does in trading with the US, which is not an EU member (and thus does have some trade barriers when trading with the EU). Note that geographical distance is accounted for when constructed gravity models, so the geographical proximity of, say, Spain to France is controlled for. The

---

[10]There are no US data in 2012.

137

Figure 3.5: Trade Costs by Trading Partner: Euro Periphery, 2012
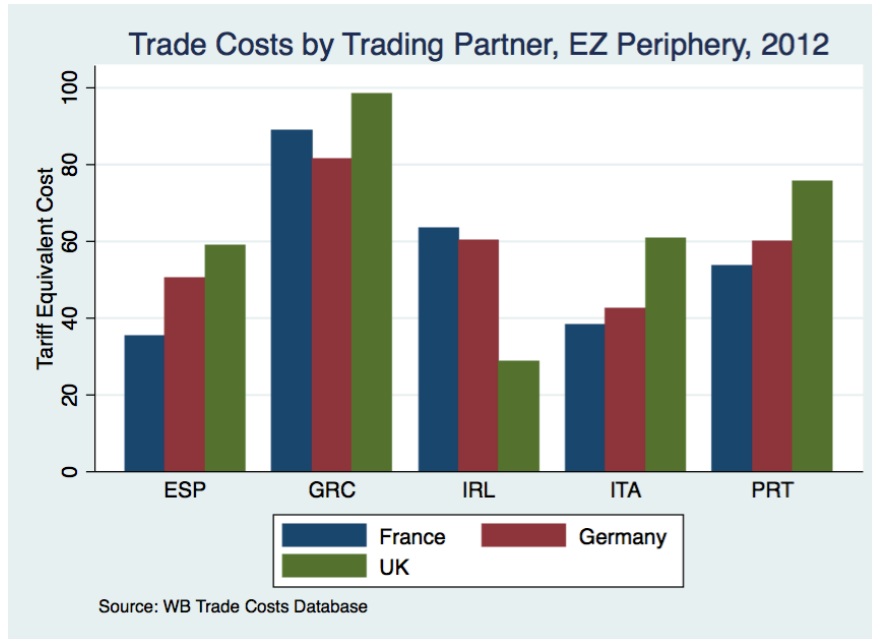
measure is likely contaminated by potential cultural factors. For instance, Catalan and Basque speakers along both sides of the Spain-France border arguably make trade easier. Such issues however cannot explain the lower costs Portugal faces compared to Greece when trading with Germany.

These issues had not improved by 2012. Although Greece's overall trade costs did fall somewhat, this improvement lagged the performance of the other peripheral countries. Greek costs vis-a-vis France fell by 9.5% from 2007 to 2012, compared with a fall of 24% for Italy, and around 19% for Portugal and Spain. Trade costs for Greece actually increased slightly, by 0.31%, when trading against Germany, compared to a fall of over 9% for Italy and Portugal and over 2% for Spain. The numbers are again similar against the UK. Ireland also lagged the rest, but still showed a marked trade-weighted improvement as the bulk of its bilateral trade is with the UK.

It is also interesting to consider how Greece fares against its non-Eurozone neighbors in Southeastern Europe. Figures 3.6 and 3.7 show trading costs for Greece, Bulgaria, Romania, and Turkey for 2007 and 2012, respectively.

Figure 3.6: Trade Costs by Trading Partner: Southeast Europe, 2007
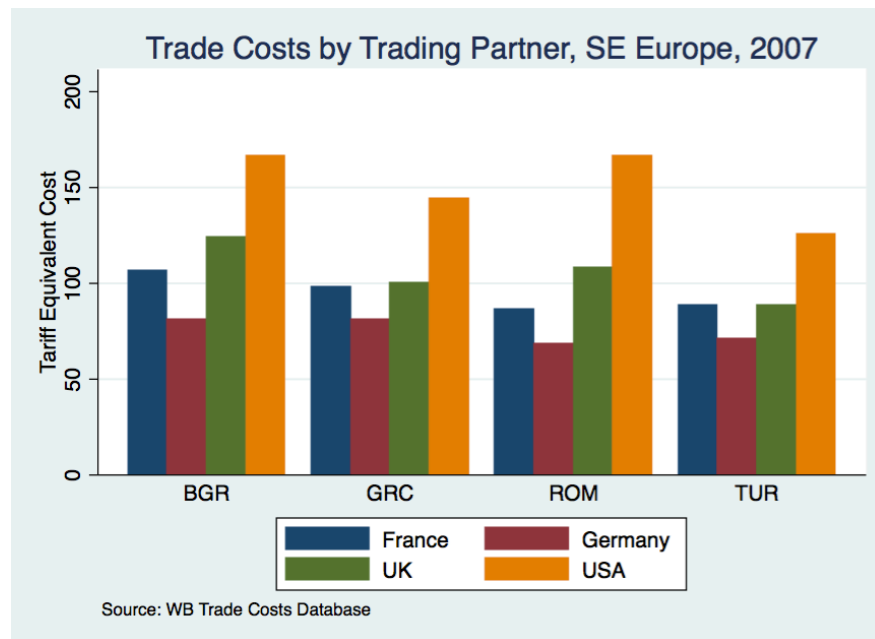
Here the differences are less pronounced compared to the Euro periphery, but it is important to consider the fact that Greece has a much higher level of GDP per capita than its neighbors. Greece has lagged against the Southeastern countries in reducing its costs. Bulgaria has made great strides, lowering by 32% costs of trading with France, 25% with Germany, and 27% for the UK. Turkey has been less successful than Bulgaria, but still performed better than Greece.

Another interesting metric quoted in the report is the Logistics Performance Index (LPI), an index of inefficiencies in the supply chain. It is intended to assess the logistics performance of each country by surveying of professionals in the major trading partners of the country. It ranks Greece 71st in the survey, while Spain,

Figure 3.7: Trade Costs by Trading Partner: Southeast Europe, 2012
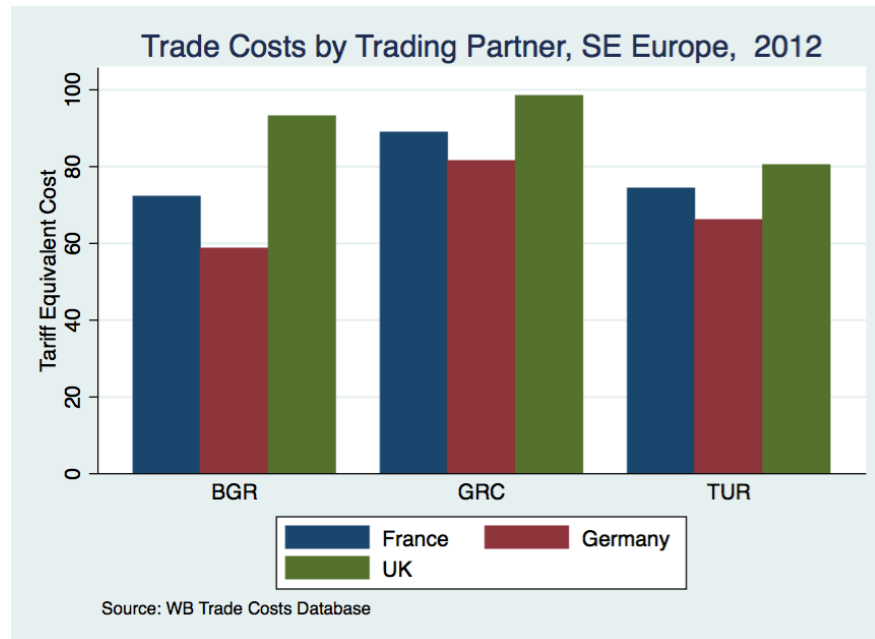
Italy, and Portugal rank 20, 27, and 28, respectively.

According to the World Bank report, even though the problems with the Greek distribution sector are varied and cannot be characterized by a single issue, an overarching hurdle to an improved value chain is the overwhelming reliance on road transportation through trucks, which accounts for 98% of land transport, versus 72% for the EU as a whole (World Bank 2013). The trucking sector is especially fragmented, with 90% of operators being characterized as "own-account", meaning that they transport their own goods. According to the Hellenic Statistical Authority (quoted in World Bank 2013), 1.27 million such companies operate, a stunning number for a country of 11 million inhabitants. The report documents that the commercial sector is small and fragmented, with little consolidation (two thirds of the companies have one truck), and old trucks. The authors list a number of possible factors, such as more stringent regulation for commercial licenses as opposed to own-

accounts, high fuel costs, VAT rates, road user charges, driver insurance costs and others. The total cost of per kilometer for Greek operators is almost double that of French ones.

The upshot is that land transportation imposes a significant cost on Greek trade, which is not due to mark-ups in the distribution value chain (though such do exist in upstream industries), but rather due to costly regulation, costly inputs, and the fragmentation of the trucking industry, which is inefficient as it cannot exploit scale economies.

The over-reliance of Greece on trucks for land transport is partly explained by geography. Greece has a network of over 300 inhabited islands[11], and the inland is highly mountainous. Nevertheless, equally important is the underdeveloped nature of the rail network. The report highlights issues both with the train company and the network of stations and lines. The main train company is state-owned and operated, suffering from the same inertia and inefficiencies as the rest of the Greek state bureaucracies. A particular problem regarding the train company is that the operator (TRAINOSE) and the infrastructure manager (OSE), even though they are both state-operated, do not coordinate regarding network development. Furthermore, the train system, including the stations, was not designed with commercial traffic in mind, but was meant to accommodate passengers instead, and the reports mentions that potential major clients lamented the poor design of stations in terms of transporting cargo, as well as the lack of punctuality regarding service.

---

[11]Greece has the second largest coastline to area ratio out of all countries in the world that are not islands or micro states. Equivalently, it has the 11th longest coastline even though it is the 97th largest in area.

A further significant hurdle regarding efficient rail transportation comes from the quality of the lines themselves, as to a large extent they are not electrified, which both raises the cost and limits the speed of transportation. As a consequence of all these issues, rail accounts currently for only two percent of exports. The low quality of the rail network has significant spillovers to other parts of the logistics network, however. The port of Piraeus, the largest port in Greece, is a global center of the maritime industry, in which Greece is a world leader, and has experienced significant investments in the last few years. However, the connection of the rail network with the port is heavily underdeveloped, meaning that goods have to travel to and from the port by trucks, suffering the inefficiencies mentioned above. Thus, the improvements in the Piraeus port mainly service transit demand, and have not contributed to export growth. As this investment activity is tied to privatization of the port operation, the lack of trickle down to the domestic economy threatens further developments in the port, which were planned to take place with further privatization.

Finally, trade is severely hampered by a corrupt and inefficient customs system. Customs brokers have an essential monopoly over the clearing of goods through customs, and even though the profession was recently liberalized de jure, it remains de facto closed according to the report. The most important protection the brokers enjoyed was the benefit of a credit line with the customs authority, whereas non-licensed actors had to pay fully in cash or check up-front. Anecdotally, the customs sector is considered one of the most corrupt parts of public administration. Though little was known about the extent of illicit activities regarding customs, a

recent report by a leading Greek daily newspaper uncovered a systemic culture of corruption between customs officers and brokers, with bribes involved in essentially every transaction between two parties.[12]

There are a number of further issues contributing to the low efficiency of the logistics sector, including the bad planning of logistics zones, restrictive licensing systems, and poor enforcement of trade-related laws. As the focus of this paper is on factor that hamper trade, I did not discuss the well-performing sectors of the Greek transportation sector, notably the shipping industry, which is the world leader in the field.

The upshot in the preceding discussion is that the state of the logistics sector, comprising primarily of transportation, infrastructure, and the quality of state bureaucracy, imposes a significant tax on trading activity, and could potentially account for a substantial part of the lackluster performance of the Greek exporting sector during the recovery. In addition, it is worthwhile to note that logistics inefficiencies lead to higher prices for all goods, albeit to varying lengths according to the transportation component of the final good, and hence lead to higher import, as well as export, prices, and higher CPI.

3.3.5 Other

As the focus of this paper is rather narrow, I will only briefly mention two other factors, which have been dealt with in detail elsewhere. The first is price

---

[12]Kathimerini newspaper, July 27, 2014.
http://www.kathimerini.gr/777866/article/epikairothta/ereynes/moy-evazan-lefta–kryfa-sta-xeria.

rigidities, a catchall term for various markups and inefficiencies in the product markets, studied in detail by Arkolakis et al. (2014). They argue against a common anecdote that Greece cannot produce tradable goods, and instead point to product market rigidities in explaining why prices kept rising several years into the recession, before starting to fall by 2013. Using a standard Eaton-Kortum trade model, they show that exports would have risen by 25% from 2007 to 2012, rather than falling by 5%. For the sake of completeness, I conduct a separate exercise in the context of my model, where I study how structural policies, which would reduce private sector markups, would have affected output and exports.

The second relates to credit tightening. The combination of heavy exposure of Greek banks to Greek government debt, whose value plummeted, and substantial deposit flight, squeezed bank liquidity and led to sharp rises in borrowing costs. Pelagidis & Mitsopoulos (2014) document that interest rates for loans to non-financial companies have been the highest in Europe (together with Cyprus) since 2008. See Pelagidis & Mitsopoulos (2014) and references therein for further details.

3.4   Model

The model is fairly standard, so I will only describe the structure here. The full model is presented in Appendix C, together with the full list of equilibrium conditions.

The context is a standard New Keynesian small open economy framework,

as in Gali & Monacelli (2005). A representative household consumes a composite consumption good, made up of tradable and non-tradable goods, and supplies labor. Preferences are logarithmic with respective to consumption, and of the constant relative risk variety for labor. The consumption preferences are also characterized by habit formation, a common feature of such models, in order to induce smooth adjustments to shocks.

I consider small open economy within a currency union, assuming, for simplicity, that the currency union is closed to the rest of the world, and focus analysis within the union. In contrast to Gali & Monacelli (2005), credits markets are incomplete. As is well known, small open economy models with incomplete markets feature a random walk in consumption, which precludes the use of approximation techniques, and stationarity needs to be induced. Here, I assume that the domestic economy borrows at a premium from abroad i.e. the interest rate is debt elastic (Schmitt-Grohe & Uribe 2003). This feature is also intended to capture the fact that the eurozone periphery faced skyrocketing borrowing costs, at large premium over the core, notably Germany, at the onset of the crisis.

The production structure is very similar to Rabanal & Tuesta (2012). There is a final goods sector comprised by a continuum of competitive firms that produces a final tradable and final non-tradable good, which are consumed directly by domestic households. The final tradable good is produced by combining home and foreign tradable intermediate goods, while the final non-tradable is produced using home non-tradable intermediates. Notice that international trade takes place only in tradable intermediates, not final goods. This is very common in such models,

145

where the final goods is typically thought of as a retail good. This approach is especially convenient in the present context relates to modeling VAT, which is rebated for both exports and intermediate goods. This setup then allows me to impose VAT on final consumption without complicating the model with rebates. The preferences of domestic consumers affect the production structure through the presence of bias in favor of home intermediates in the production of tradable goods.

Finally, intermediate goods are produced by monopolistically competitive firms, using labor and energy as inputs. Pricing is of the Calvo variety, with only a fraction of firms being able to adjust their prices in any given year. In addition, there is partial indexation to inflation, meaning that firms set their prices as a function of last year's prices. These are standard features in New Keynesian small open economy models.

3.4.1   Calibration

For the calibration of the parameters in the model, I rely both on the previous literature as well as on micro data. The output level of the foreign economy is taken as given, and the only effect of home on the foreign economy is through its effect on the foreign tradable price level, and hence on the relative size of the final and intermediate goods sectors, as well as on foreign imports and .

The habit formation parameter h is set to 0.6, as is standard for the euro area (Smets & Wouters 2003). I follow Papageorgiou (2014) and set the Calvo parameter θ_N and θ_T to 0.7059 for both tradable and non-tradable producers, and

to 0.697 for the foreign country, which imply that prices are reset on average every 3.4 and 3.3, respectively. These are parameters estimated from firm level evidence in European firms (Druant et al. 2009), and it should be noted that while these are very close to the posterior mean of Rabanal & Tuesta (2012) for the tradable sector, the latter obtain a very low posterior estimate for non-tradables. Similarly, I set the indexation parameters κ_T and κ_N to 0.259, and the foreign counterparts to 0.216. These correspond to the fraction of firms that consider past inflation when resetting prices, as reported in Druant et al. (2009).

I set the elasticity of substitution between core and energy consumption, as well as between labor and energy in production to 0.3, a value around the median of standard calibrations in the literature (e.g Alvarez et al. 2009, Jacquinot et al. 2006, Natal 2012), and estimates from Kilian & Murphy (2014). For the elasticity of substitution between core tradables and non-tradables I set ϵ to 0.6.

Regarding trade costs, the interesting counterfactual regarding exports is not the unrealistic case of zero trade costs, but rather the steady state gains in exports and output Greece would enjoy if it could reduce its trade costs with Germany (a proxy for the core Euro-area) to the level of Portugal, a country with equally little cultural ties to Germany. According to the World Bank Trade Costs Database, the tariff equivalent trade cost to Germany was 66% for Portugal and 81% for Greece in 2007, and that is my choice for the baseline tariff equivalent. This translates approximately into an iceberg cost of 44.75% in the context of the model.

I set the fraction of tradables in consumption to 0.4 for both countries (Rabanal & Tuesta 2012), and the fraction of home tradable intermediates in final tradables γ_x

to 0.6 (Farhi et al. 2014). Note that these are the only foreign parameters that I need to pin down, as they determine the relative size of the final goods sectors and foreign imports. The elasticity of substitution between home and foreign intermediates (ξ) is more challenging, because of the wide range of estimates reported in the literature. Rabanal & Tuesta (2012) estimate a posterior mean of 0.85 and Papageorgiou (2014) estimates 3.351 for the Greek economy using micro data, while Adolfson et al. (2007), in a prominent open-economy DSGE estimation exercise use a calibrated value of 5. I set ξ to 2, a value intended to match the export improvement of Portugal.

Another challenging set of parameters regards the energy shares in intermediate production $(1 - \gamma_h)$ and $(1 - \gamma_n)$. Bodenstein et al. (2008) and Natal (2012) set it to 0.02 for the US, citing evidence from sectoral value added data, as well imports of oil and gas, and Cuche-Curti et al. (2009) also use a value of 0.02 for Switzerland. Edelstein & Kilian (2007) report a value of 0.03 for the US using energy share in value added. To my knowledge, there is no paper directly looking at the energy inputs of different industries, let alone a separation into tradables and non-tradables. A more valuable source is the energy use of as a fraction of value added across tradables and non-tradable industries. Since such information is unavailable for Greece, I use US data from the BEA.[13] The classification of production as tradables or not is fraught with problems, particularly for countries like Greece, where the standard assumption that services are non-tradable is highly mis-leading, as services account for approximately half of exports, due to the size of the tourism and shipping sector. A more appropriate methodology (the modified indirect approach) has been recently

---

[13]GDP-by-Industry Data, http://www.bea.gov/industry/gdpbyind_data.htm.

pioneered by the OECD, and it is this approach I utilize, modified for Greece.[14] For

the US, and excluding government and utilities, the shares of energy are 0.026 for

non-tradables and 0.053 of tradables. For Greece, using the US energy input to value

added in GDP ratio for each industry, the values are 0.025 and 0.091, respectively,

owing overwhelmingly to the much larger shares amongst Greek tradables of trans-

portation and warehousing, and to a much lesser extent agriculture, the two most

energy intensive industry groups.[15] Removing the transportation sector reduces the

inputs to 0.022 and 0.039, respectively. For the purposes of the model, the latter

approach is more sensible, as transportation is really a cost on other trade activities.

This measure is indicative of the large burden the transportation sector imposes on

Greek trade.

3.5   Analysis

I will be conducting two sets of quantitative exercises to illustrate the relative

importance of VAT imperfections, energy costs, and trade costs on the failure of

Greece to boost its exports. In the first set of exercises, I will examine the quanti-

tative comparative statics of different policies on the steady state level of exports,

output, as well as the relative size of the tradable sector. In the second set of exer-

cises, I will compare the dynamics of the model when hit by an interest rate shock

---

[14]According to this approach, the following industries are classified as tradable: agriculture, mining, manufacturing, wholesale trade, transport, information, and professional services. I also assign accommodation to tradables, to account for the tourism industry, though the figure does not change much.

[15]The modified indirect approach classifies wholesale trade in the tradable sector and retail trade in the non-tradables. National Accounts for Greece do not report those separately, so assign the sum to non-tradables.

Table 3.1: Preference and Technology Parameters

Preference Parameters		
β	Discount factor	0.995
h	Habit formation parameter	0.600
χ	Debt elasticity of the Greek interest rate	0.001
ν	Labor elasticity	1.000
Elasticities of Substitution		
ϵ	Tradable and non-tradable core consumption goods	0.600
α_y	Labor and energy in intermediate production	0.300
ξ	Home and foreign intermediate inputs in final goods production	2.000
Production and Consumption Shares		
γ_c	Share oftradables in consumption, home	0.400
γ_x	Share of home intermediates in final good production	0.600
γ_h	Share of labor in home tradable intermediates	0.978
γ_n	Share of labor in home non-tradable intermediates	0.961
Calvo parameters		
θ_h	GR Tradable intermediates	0.706
θ_N	GR Non-tradable intermediates	0.706
Indexation parameters		
κ_h	GR tradable intermediates	0.259
κ_N	GR non-tradable intermediates	0.259

(intending to capture the large rise in the cost of borrowing during the crisis, as well as the fall of overall demand) under alternative scenarios regarding government policies.

To begin with, the top panel of figure 3.8 shows the steady state of exports, output, and the relative size of the tradable sector under a range of VAT imperfections. As I abstract from issues relating to incomplete rebates, as long as VAT is uniform across tradables and non-tradables, the level of exports is independent of the level of VAT. So in this exercise, starting from a uniform VAT of 18%, I sketch out the response of the steady of the model for a rise in tradables VAT, up to the statutory maximum of 23%. It is impossible to pin down the exact level of the tax on each sector, owning both to the number of different exemptions and the difficulty of precisely defining what is a tradable and a non-tradable good, so this exercise is only meant to be illustrative. We see that the steady state volume of

exports is lower by approximately 0.8% at the statutory maximum. GDP responds little, falling by approximately 0.3% lower, though it should be noted that as VAT proceeds are spent in the same way as private consumption, this is equivalent to a rebate, and so a small output response is expected. Finally, the share of labor used in the tradable sector responds more strongly, falling by over one percentage point, or 2.7%. Overall these response are not trivial and are consistent with the theory, and though they cannot account for the failure of Greek exports to pick-up, they may partially explain why exports did not pick-up.

The second panel in figure 3.8 conducts the same exercise against rising energy costs. As the interest is in relative price changes over the rest of the periphery, this exercise has a more straightforward interpretation, as we know that energy costs in Greece rose by over 60% since 2009. The response of output is significantly larger here, falling by 1.2%, but much smaller regarding exports, which is only about 0.2% lower in the steady state, as well as the size of the tradable sector. This is because the energy share of production is only somewhat larger in tradables, and hence the effect of energy costs falls almost uniformly on all production. This is true, of course, of the total tradable sector. As shown in Pelagidis & Mitsopoulos (2014), certain sectors are so energy intensive that the energy cost hike and the labor cost fall meant that the former was over 3 times higher than the lower. The point is that, on aggregate, little of Greek tradable production is particular energy intensive.

Finally, figure the bottom panel in figure 3.8 plots the results of the exercise where the tariff equivalent trade cost rises from 41% to the baseline of 81%. The response here is stark. Steady state exports are 12% higher at the low cost level, and

151

output 2.7% higher. It is clear that trade costs are much more important than either VAT non-uniformity or energy costs in explaining the trouble with Greek exports during the crisis. Note that the share of labor in the tradable sector actually rises with trade costs, due to import substitution.

Figure 3.8: Steady-State responses to different policies
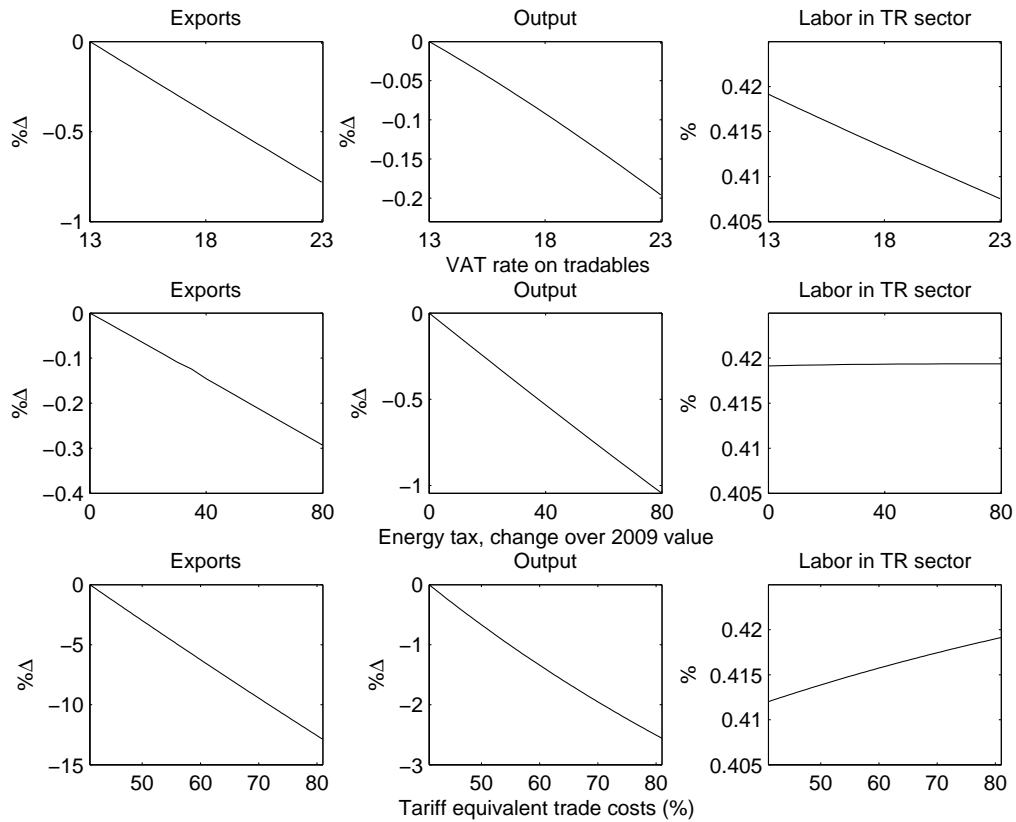
3.5.1 Dynamic Analysis with Perfect Foresight

To consider how the dynamics of the recovery where affected by the different taxes and costs, I introduce a borrowing shock to the economy that matches the 4% decline in output in 2010, and analyze the impulse responses under different policies, in a context where after the initial shock, there is perfect foresight regarding both

the future path of the interest rate and government policies.

First, it is interesting to consider whether policy to reduce trade costs would imply different adjustment dynamics. Starting at the baseline tariff equivalent cost of 81%, figure 3.9 shows the response of consumption and exports to the same shock under different reform scenarios, where the cost falls by 10%, 20%, and 30% within ten periods, and the results again are stark regarding exports. A reduction of 20% (roughly to the level of Portugal), would have raised exports enough to raise output by

Figure 3.9: Deterministic: Responses to interest rate shock - alternative paths for trade costs
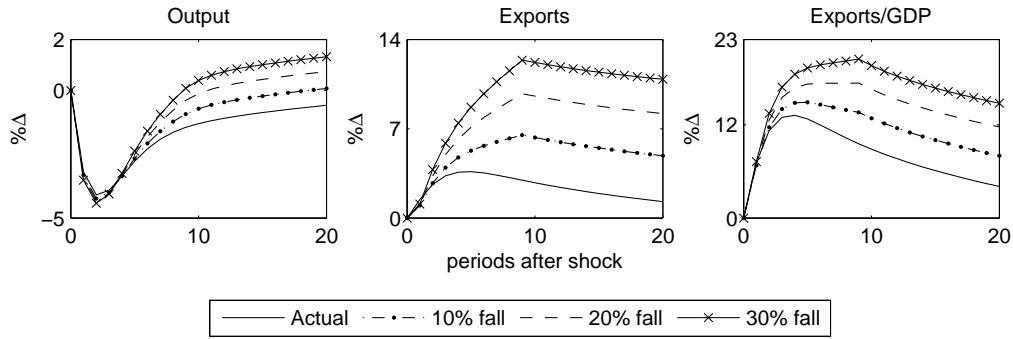
Next, consider VAT and energy costs. As argued above, VAT and energy costs may explain why exports fell, but not why they did not rise. Figure 3.10 shows the response of exports after the interest rate shock assuming no trade shocks, comparing the case of a 80% energy hike and VAT on non-tradables higher by 7 percentage points to 20%, with the counterfactual of non energy hike and uniform VAT at 13%. The VAT hike moderates the initial shock by raising labor through the wealth effect, though this effect evaporates within a quarter. The dynamic effect here is more pronounced than the static one for both VAT and energy. VAT has a larger

effect on exports and a small effect on output, while energy cost has a substantial effect on output, and a moderate effect on exports. Overall, exports would have been approximately 3% higher after 10 quarters in the absence of the policies changes.

As Arkolakis et al. (2014) point out, price rigidities in the form of uncompetitive product markets, are also important in explaining the lack of adjustment. Though the small-scale model used here is too simple to fully account for such issues, it is instructive to examine how the economy behaves under different assumptions about steady state markups. Figure 3.11 shows the impulse responses to the shock under structural policies that would reduce markups within ten periods, by 10%, 20%, and 30%. In the context of the model, policies that reduce markups have a sizable impact on both output and exports. Reducing markups has a balanced effect on both sectors, unlike trade cost reduction which only benefits the tradable sector, and so output and exports rise together markups fall, implying little change in exports as a fraction of GDP.

3.5.2 Rational expectations analysis using CDS data

The preceding dynamic analysis was based on a perfect foresight, deterministic model (after the initial unexpected shock), and thus necessarily abstracts from the case where the rational agent is uncertain regarding the future path of the interest rate. It is also abstracted from the actual path of borrowing costs after the initial shock.

In this section, I simulate the model in a stochastic context, where agents

Figure 3.10: Deterministic case: Responses to interest rate shock - VAT and Energy Costs
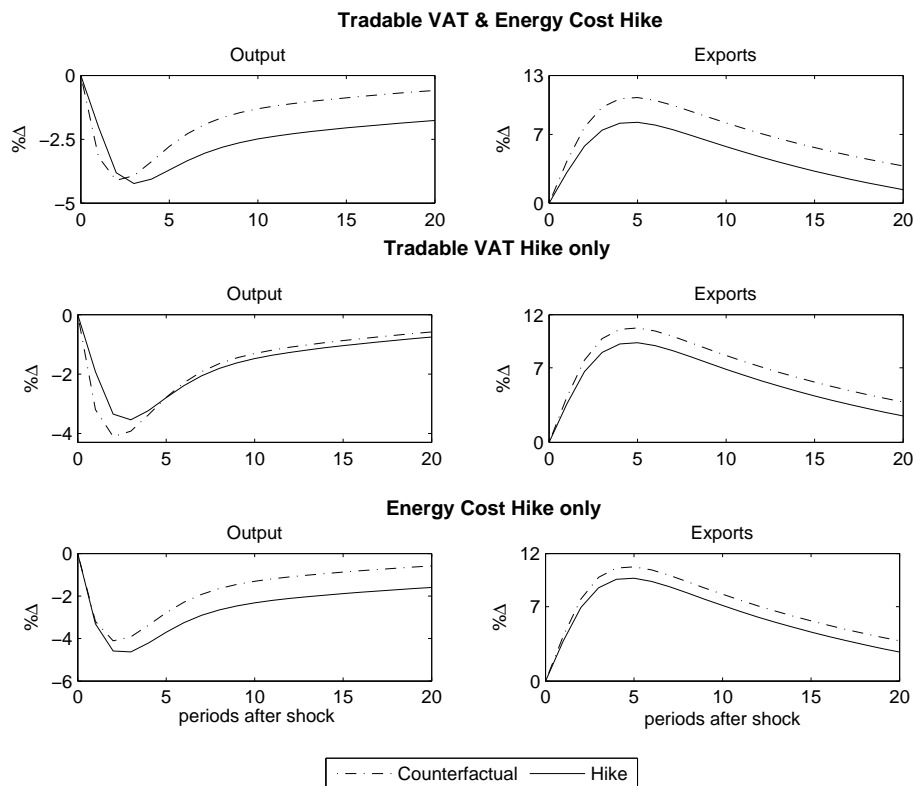

Tradable VAT hike: 20% from uniform 13%. Energy cost hike: 80% over initial steady state.

expected zero mean random shocks to their borrowing costs each period. In addition, I feed into the model the actual path of interest rates the Greek economy experienced from the first quarter of 2010, when the impending crisis became apparent, until the last quarter of 2014. At the same time, I maintain the assumption that the government commits to a policy after the initial shock, as the ultimate goal of this paper is to study the efficacy of government policies.[16] The choice of data is not a straightforward one. Even though the stock of outstanding credit in the Greek

---

[16]I use the simultxdet function of Dynare, which combines deterministic and stochastic shocks. The government policies are deterministic in that the agent knows the precise path from the initial period. The interest rate is a zero mean autoregressive stochastic process, but instead of having random shocks each period, I feed actual data into the model, in a sense "tricking" the agent. Though an adaptive expectations process may be more realistic, the model is not as naive as may seem, given the highly persistent nature of the shock (from the point of view of the agent.

Figure 3.11: Deterministic case: Responses to interest rate shock - alternative paths for markups
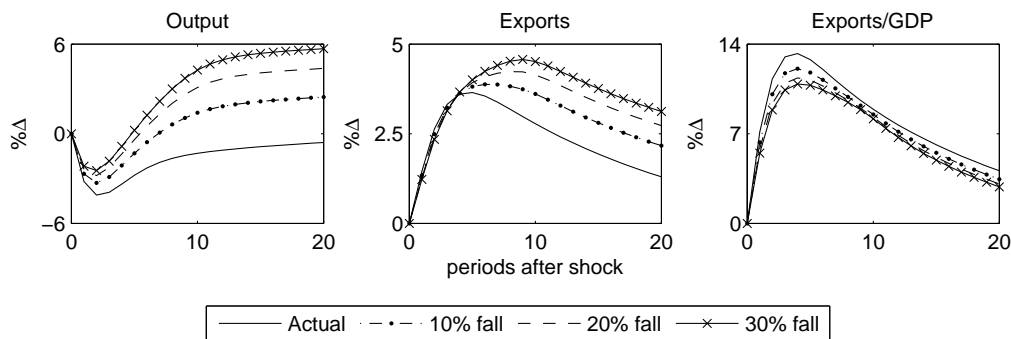
economy collapsed to a third of its precession level, the average interest rate never exceeded 8% [17], and so credit adjustment took place in the quantity margin, with credit being rationed a-la Stiglitz & Weiss (1981). Another approach is to use data on credit default swaps (CDS), an indirect metric of borrowing costs for the sovereign or the bank. Due to the massive private sector debt write-down (PSI) in 2012, sovereign CDS were triggered in 2012 and are thus not suitable as a metric of borrowing costs. I thus use data on 5 year CDS for the National Bank of Greece, by far the largest bank in Greece at the time in overall assets and private sector lending portfolio.[18]

Results from the stochastic simulations are shown in figures 3.12-3.14. Results follow broadly the same pattern as the deterministic exercise. The main difference is the much steeper fall in output, up to 17%, which is a lot closer to the actual fall of 20% by 2013. The recovery in the simulated data comes at an earlier time, however, and is quite fast. This is due to the sharp fall in the CDS after the PSI, and

---

[17]This refers to loans to non-financial corporations. Source: Bank of Greece.
[18]CDS rate on 1-2 year bonds were also extremely volatile during the PSI, reaching a value of several thousands basis points for a few months. This is a typical inverted yield curve situation.

Figure 3.12: Stochastic case: Responses to interest rate shock - alternative paths for trade costs
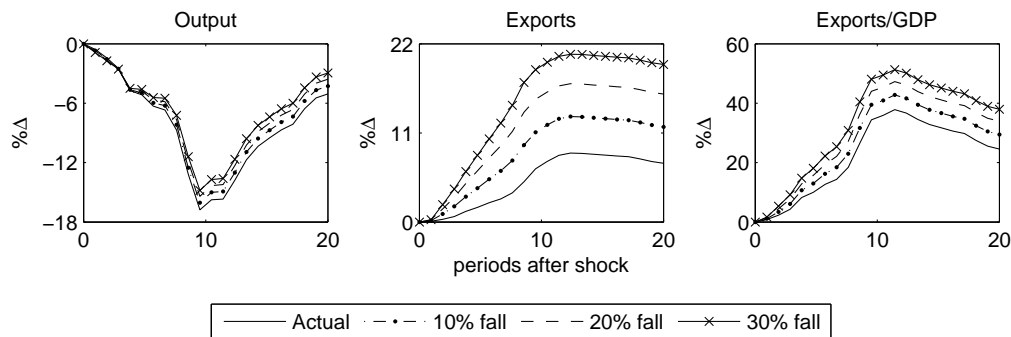

the absence of another demand channel in the model. In reality, fiscal tightening is largely blamed as having been another important drag on growth, and such a feature could easily be included in the model. Nevertheless, it is important that the model manages to get so close to the actual contraction, especially regarding the validity of the counterfactual results.

Results are very similar with the deterministic case, and the conclusion is the same. Trade costs are by far the most important reason why exports did not pick up. The counterfactual reduction of trade costs to the level of Portugal would have been over 10% higher after 10 periods. The model cannot account for the actual fall in exports observed in the data, but does show that they would have been higher in the absence of the VAT and energy hike.

3.6   Conclusion

This paper studies the Greek recovery during the recent euro crisis, predominantly focusing on the most salient feature of the adjustment, the failure of Greece to boost its export sector. This failure is more stark when one considers that other

Figure 3.13: Stochastic case: Responses to interest rate shock - VAT and Energy Costs
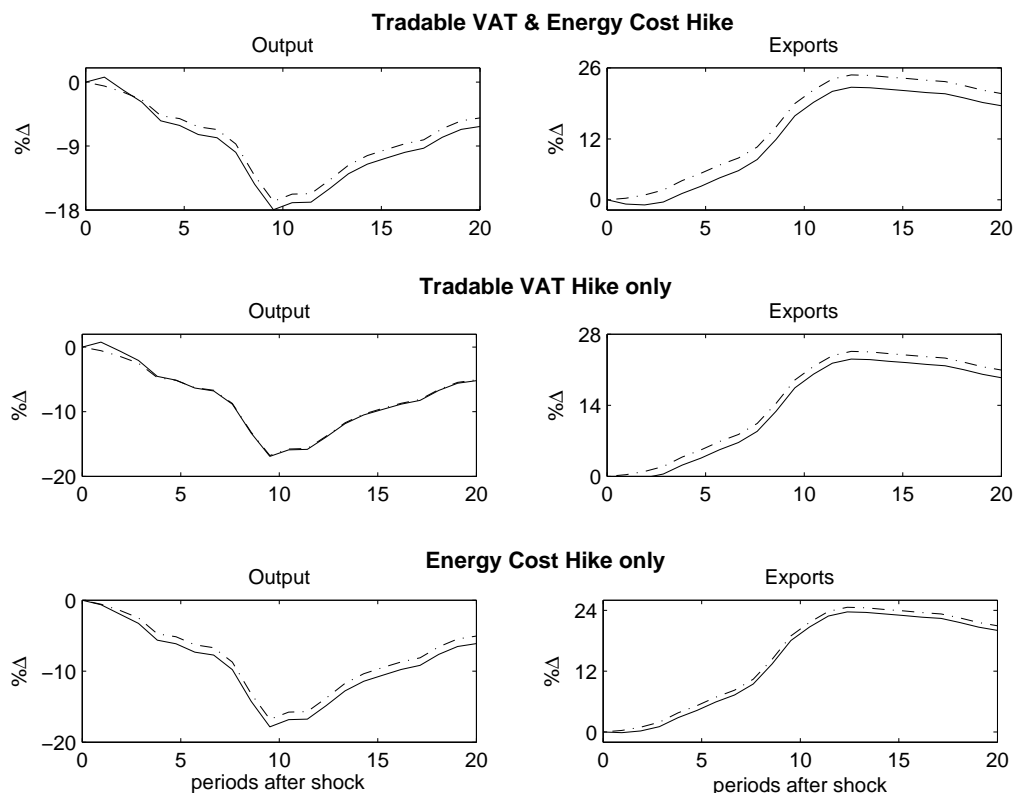
peripheral countries, such as Portugal, Ireland, and Spain, showed a marked improvement in their exports.

The focus of this paper mainly fell on three factors that could explain, on the one hand, why Greek exports failed to rise, and, on the other, why they actually fell, when standard trade models predicted a rise of 25% (Arkolakis et al. 2014). These are a) rise of the VAT rate, which falls predominantly on the tradable sector, hence impeding the desired adjustment, b) the energy tax hike, which placed a significant burden on production across sectors, and c), the presence of substantial trade costs, which are much higher for Greece than the rest of the euro periphery. After a detailed discussion of the theoretical and empirical background regarding these issues, I conducted a set of exercises in the context of a stylized, small-scale

Figure 3.14: Stochastic case: Responses to interest rate shock - alternative paths for
markups
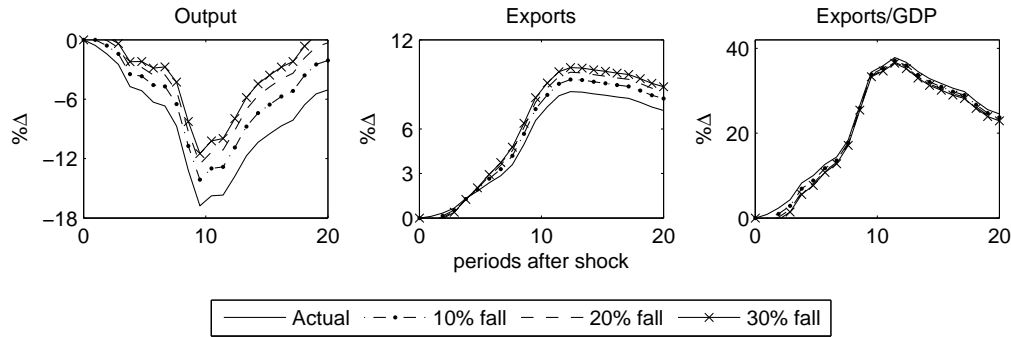
New-Keynesian small open economy model, to quantitatively assess the relative
importance of these factors for explaining the facts.

In the context of the model, trade costs are singularly important in explaining
why exports did not rise. The steady-state level of exports would be over 8% higher
if the tariff-equivalent trade cost of 81% could fall by 20% (the level enjoyed by
Portugal), and approximately 13% higher if it could fall by 30%. By contrast,
although the VAT and energy cost hike are an order of magnitude less important
for the steady-state level of exports, they can partially explain why exports fell. In
the absence of the VAT and energy hikes, exports would have been 3% within 5
quarters.

The policy implications of this paper are clear: for Greece to grow its export
sector, it has to invest in improving the efficiency of its logistics and transportation
sector. As argued in the main text, the over-reliance on a fragmented trucking in-
dustry and the absence of proper connection of the underdeveloped rail system with
the port has meant that significant investments in the major ports have not cre-
ated benefits to the domestic economy. Improving the logistics sector and reducing

159

domestic industry markups would help bring Greece out of its current depression.

Appendix A: Appendix for Chapter 1

A.1 Model

Recall that the utility function used for the illustrative example in Section 1.4 is given by

$$u(c, h, B) = \frac{c^{1+\eta}}{1+\eta} - \frac{h^{1+\gamma}}{1+\gamma} + \frac{B^{1+\alpha}}{1+\alpha}. \tag{A.1}$$

This is a standard utility function used for labor supply problems (Keane 2011), because of its convenience: γ is intimately connected with elasticities, and η with income effects. The choice of functional form is only meant to give closed form solutions for the elasticities, and is of no particular significance.

The full Langrangian of the program[1] to be maximized is then given by

$$L = \frac{c^{1+\eta}}{1+\eta} - \frac{h^{1+\gamma}}{1+\gamma} + \frac{B^{1+\alpha}}{1+\alpha} + \lambda[wh + y - pB - c]. \tag{A.2}$$

---

[1]In this exposition, I ignore, for simplicity, the possibility of corner solutions, which can be readily defined by constructing reservation wages (for labor) and a similar expression for bequests.

The first order conditions are

$$
\left.
\begin{array}{lll}
c\rfloor & U_c = \lambda \\[2mm]
h\rfloor & U_h + \lambda w = 0 \\[2mm]
B\rfloor & U_B = \lambda p
\end{array}
\right\}
\quad
\begin{array}{l}
C^\eta = \lambda \\[2mm]
h^\gamma = \lambda w \\[2mm]
B^\alpha = \lambda p
\end{array}
$$

These conditions, together with the budget constraint, define a system of four equations with three endogenous variables (c, h, B, and λ). The total derivative of the system (exploiting the additively separable structure of preference) can allow us to determine the effect of a small change in one variable on the complete system:

$$
\begin{array}{rl}
U_{cc}dc - d\lambda = & 0 \\[3mm]
U_{hh}dh + wd\lambda = & -\lambda dw \\[3mm]
U_{BB}dB - pd\lambda = & \lambda dp \\[3mm]
-dc + wdh - pdB = & -hdw - dy + Bdp
\end{array}
$$

In matrix notation, of the form $Ax = Zb$:

$$
\begin{bmatrix}
U_{cc} & 0 & 0 & -1 \\
0 & U_{hh} & 0 & w \\
0 & 0 & U_{BB} & -p \\
-1 & w & -p & 0
\end{bmatrix}
\begin{bmatrix}
dc \\ dh \\ dB \\ d\lambda
\end{bmatrix}
=
\begin{bmatrix}
0 & 0 & 0 \\
-\lambda & 0 & 0 \\
0 & 0 & \lambda \\
-h & -1 & B
\end{bmatrix}
\begin{bmatrix}
dw \\ dy \\ dp
\end{bmatrix}
$$

To estimate the effect of a small change in the exogenous variables on the endogenous variables and derive the Slutsky equation, it is convenient to use Cramer's rule.

First, the determinant of A is given by:

$$\Delta = U_{cc} \begin{vmatrix} U_{hh} & 0 & w \\ 0 & U_{BB} & -p \\ w & -p & 0 \end{vmatrix} + \begin{vmatrix} 0 & U_{hh} & 0 \\ 0 & 0 & U_{BB} \\ -1 & w & -p \end{vmatrix}$$

$$= U_{cc} \times \left\{ U_{hh} \begin{vmatrix} U_{BB} & -p \\ -p & 0 \end{vmatrix} + w \begin{vmatrix} 0 & U_{BB} \\ w & -p \end{vmatrix} \right\} - U_{hh} \begin{vmatrix} 0 & U_{BB} \\ -1 & -p \end{vmatrix}$$

$$= - \left\{ U_{cc} U_{hh} p^2 + U_{cc} U_{bb} w^2 + U_{hh} U_{BB} \right\}$$

This is positive due to the second order conditions of the utility function - utility is concave with respect to consumption and bequests, and (negative) convex with respect to labor.[2]

Now we use Cramer's rule:

$$\frac{\partial h}{\partial w} = \frac{1}{\Delta} \begin{vmatrix} U_{cc} & 0 & 0 & -1 \\ 0 & -\lambda & 0 & w \\ 0 & 0 & U_{BB} & -p \\ -1 & -h & -p & 0 \end{vmatrix} = \frac{U_{cc}}{\Delta} \begin{vmatrix} -\lambda & 0 & w \\ 0 & U_{BB} & -p \\ -h & -p & 0 \end{vmatrix} + \frac{1}{\Delta} \begin{vmatrix} 0 & -\lambda & 0 \\ 0 & 0 & U_{BB} \\ -1 & -h & -p \end{vmatrix}$$

---

[2]In the more general case where the cross-partial derivatives are non-zero, the positivity of the determinant is guaranteed by the fact that the second order effect of a budget neutral change has to be negative.

$$
= \frac{1}{\Delta} U_{cc} \left\{ -\lambda \begin{bmatrix} U_{BB} & -p \\ -p & 0 \end{bmatrix} + w \begin{bmatrix} 0 & U_{BB} \\ -h & -p \end{bmatrix} \right\} + \frac{1}{\Delta} \lambda \begin{bmatrix} 0 & U_{BB} \\ -1 & -p \end{bmatrix}
$$

$$
= \frac{1}{\Delta} U_{cc} \left\{ \lambda p^2 + wh U_{BB} \right\} + \frac{1}{\Delta} \lambda U_{BB}
$$

Substituting for Δ, we finally have that

$$
\frac{dh}{dw} = \frac{U_{cc} U_c p^2 + wh U_{BB} U_{cc} + U_c U_{BB}}{- (U_{cc} U_{hh} p^2 + U_{cc} U_{bb} w^2 + U_{hh} U_{BB})}. \tag{A.3}
$$

Before proceeding to functional forms, first let us define the following parameters for convenience. Let $S_j = \frac{j}{wh+y-pB}$, where $j \in J = why, y, pB$. Also, let $S_{i,j} = \frac{i}{j}$, where $i \in J$. For instance $S_y = \frac{y}{wh+y-pB}$ and $S_{w,y} = \frac{S_w}{S_y}$.

Using now the functional form of the utility function, and after a few steps of tedious algebra, we have the Marshallian elasticity of labor supply with respect to wage:

$$
\epsilon_{h,w} = \frac{\partial h}{\partial w} \frac{w}{h} = \frac{\eta(1 - S_B) + \eta \alpha S_w - (\eta - \alpha)}{\alpha \gamma - \eta \alpha S_w - \eta \gamma S_B}. \tag{A.4}
$$

Using similar logic, the formulas for the response of hours with respect to p and y are

$$\frac{dh}{dp} = \frac{wU_{cc}\left(-U_c p - BU_{BB}\right)}{-\left(U_{cc}U_{hh}p^2 + U_{cc}U_{bb}w^2 + U_{hh}U_{BB}\right)} \tag{A.5}$$

$$\text{and} \quad \frac{dh}{dy} = \frac{wU_{cc}U_{BB}}{-\left(U_{cc}U_{hh}p^2 + U_{cc}U_{bb}w^2 + U_{hh}U_{BB}\right)}, \tag{A.6}$$

respectively.

Using the functional forms, we arrive at the following closed form solutions for the elasticities with respect to the price of bequests and non-labor income:

$$\epsilon_{h,p} = \frac{\partial h}{\partial p}\frac{p}{h} = \frac{(1+\alpha)\eta S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B}, \tag{A.7}$$

$$\text{and} \quad \epsilon^I = \frac{dh}{dy}\frac{y}{h} = \frac{\eta\alpha S_y}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B}, \tag{A.8}$$

respectively. Note that $S_B \leq 0, S_w \geq 0, S_y \geq 0$, while the denominator $\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B$ is negative. It follows then that $\epsilon^I < 0$, as expected. The sign of $\epsilon_{h,p}$ depends on the magnitude of α - this precisely reflects the interplay between income and substitution effects.

Also note that, if $B = 0$, $\epsilon_{h,w}$ and ϵ^I reduce to standard Marshallian and income elasticities of the type seen in Keane (2011).

The decomposition of $\frac{\partial h}{\partial p}$ into an income and substitution effect uses duality

theory. Let $h(p, w, y)$ be the solution to (1.2) and let $h^c(p, w, u)$ be the solution to

$$e(p, w, u) = \min_{c,h,B} pB + c - wh \qquad \text{(A.9)}$$

$$\text{s.t. } u(c, h, B) \geq u.$$

Intuitively, the expenditure function is the amount an individual needs in addition to labor earnings to achieve utility u, given prices.[3]

From duality, $h^c(p, w, u) = h(p, w, e(p, w, u))$, and,

$$\frac{\partial h^c}{\partial p} = \frac{\partial h}{\partial p} + \frac{\partial h}{\partial y} \frac{\partial e(p, w, u)}{\partial p}. \qquad \text{(A.10)}$$

From Sheppard's Lemma,

$$\frac{\partial e(p, w, u)}{\partial p} = B, \qquad \text{(A.11)}$$

and so

$$\frac{\partial h^c}{\partial p} = \frac{\partial h}{\partial p} + \frac{\partial h}{\partial y} B. \qquad \text{(A.12)}$$

---

[3]This insight is due to David Card.

167

Multiplying through to get elasticities and using the notation from above:

$$\frac{\partial h^c}{\partial p}\frac{p}{h} = \frac{\partial h}{\partial p}\frac{p}{h} + \frac{\partial h}{\partial y}\frac{y}{h}\frac{pB}{y}. \tag{A.13}$$

$$\Rightarrow \epsilon^c_{h,p} = \epsilon_{h,p} - \epsilon^I \frac{S_B}{S_y}$$

$$= \frac{\eta S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B} < 0. \tag{A.14}$$

This is the substitution effect of the rise in the estate tax. The bequest is a consumption good, so a higher tax on consumption reduces the price of leisure and reduces labor supply.

Similarly, we can get the Slutsky equation for labor supply

$$\frac{\partial h^c}{\partial w} = \frac{\partial h}{\partial w} - \frac{\partial h}{\partial y}h. \tag{A.15}$$

Using (A.15), we can get elasticities:

$$\frac{\partial h^c}{\partial w}\frac{w}{h} = \frac{\partial h}{\partial w}\frac{w}{h} - \frac{\partial h}{\partial y}\frac{wh}{h} \tag{A.16}$$

$$\Rightarrow \epsilon^c_{h,w} = \epsilon_{h,w} - \epsilon^I \frac{S_w}{S_y}$$

$$= \frac{\alpha - \eta S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B} > 0. \tag{A.17}$$

Plugging (A.14) and (A.8) into (1.6), we have an expression for the response of labor

supply to taxation only in terms of model parameters:

$$\epsilon_{h,t} = \left[\epsilon_{h,p}^{c} + \epsilon^{I} S_{B,y}\right] \frac{t}{1-t}$$

$$= \left[\frac{\eta S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B} + \frac{\eta\alpha S_y}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B} \times S_{B,y}\right] \frac{t}{1-t} \qquad (A.18)$$

$$= (1+\alpha)\frac{\eta S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B} \frac{t}{1-t}.$$

$$= (1+\alpha)\frac{\eta S_{B,y}}{\alpha\gamma/S_y - \eta\alpha S_{w,y} - \eta\gamma S_{B,y}} \frac{t}{1-t}.$$

where the last equality is obtained by multiplying top and bottom by C/N, in order to express the S quantities in terms of non-labor income.

Finally, it is also interesting to consider the direct effect of the tax on bequests. Again using Cramer's rule,

$$\frac{\partial B}{\partial p} = \frac{1}{\Delta} \begin{vmatrix} U_{cc} & 0 & 0 & -1 \\ 0 & U_{hh} & 0 & w \\ 0 & 0 & \lambda & -p \\ -1 & w & B & 0 \end{vmatrix} = \frac{1}{\Delta} U_{cc} \begin{vmatrix} U_{hh} & 0 & w \\ 0 & \lambda & -p \\ w & B & 0 \end{vmatrix} + \frac{1}{\Delta} \begin{vmatrix} 0 & U_{hh} & 0 \\ 0 & 0 & \lambda \\ -1 & w & B \end{vmatrix}$$

$$= \frac{1}{\Delta} U_{cc} \left\{ U_{hh} \begin{bmatrix} \lambda & -p \\ B & 0 \end{bmatrix} + w \begin{bmatrix} 0 & \lambda \\ w & B \end{bmatrix} \right\} - \frac{1}{\Delta} U_{hh} \begin{bmatrix} 0 & \lambda \\ -1 & B \end{bmatrix}$$

$$= \frac{1}{\Delta} U_{cc} \left\{ pBU_{hh} - w^2\lambda \right\} - \frac{1}{\Delta} \lambda U_{hh}.$$

Substituting in the formula for Δ we have

$$\frac{\partial B}{\partial p} = \frac{-U_{cc}U_c w^2 + pBU_{cc}U_{hh} - U_c U_{hh}}{-\left(U_{cc}U_{hh}p^2 + U_{cc}U_{bb}w^2 + U_{hh}U_{BB}\right)}. \tag{A.19}$$

Using the functional form we finally have

$$\frac{\partial B}{\partial p} = \frac{\gamma - \eta S_w + \eta\gamma S_B}{\alpha\gamma - \eta\alpha S_w - \eta\gamma S_B}. \tag{A.20}$$

This is unambiguously negative in the context of this model; bequests cannot be a "Giffen" good. Different functional forms could in principle accommodate such behavior. What would be needed is strong positive complementarity between bequests and leisure: when the price of bequests rise so that bequests tend to fall, leisure also falls, actually resulting in higher bequests. As Hines (2013) notes, this would require quite dramatic lifetime consumption responses for those who leave bequests, and so it is almost certain that $\frac{\partial B}{\partial p}$ is in fact negative.

A.2 A change in the exemption

The previous section focused for simplicity on a linear tax schedule, in order to sketch the response for the MW group. But the response for the HW group, who only face a rise in the exemption, is very straightforward to include, as it is essentially a shock to non-labor income. To see this, consider the budget constraint of an individual in the HW group, who gives a bequest B above exemption level E. It is given by

$$p(B - E) + E + c = wh + y$$

$$\Rightarrow pB + c = wh + y + E(p - 1). \tag{A.21}$$

Since $p \geq 1$, a rise in E is exactly equivalent to a rise in y. It is then straightforward to rewrite the system as

$$
\begin{bmatrix}
U_{cc} & 0 & 0 & -1 \\
0 & U_{hh} & 0 & w \\
0 & 0 & U_{BB} & -p \\
-1 & w & -p & 0
\end{bmatrix}
\begin{bmatrix}
dc \\
dh \\
dB \\
d\lambda
\end{bmatrix}
=
\begin{bmatrix}
0 & 0 & 0 & 0 \\
-\lambda & 0 & 0 & 0 \\
0 & 0 & \lambda & 0 \\
-h & -1 & B - E & -(p - 1)
\end{bmatrix}
\begin{bmatrix}
dw \\
dy \\
dp \\
dE
\end{bmatrix}.
$$

Then we have

$$\frac{dh}{dy} = \frac{wU_{cc}U_{BB}(p - 1)}{-\left(U_{cc}U_{hh}p^2 + U_{cc}U_{bb}w^2 + U_{hh}U_{BB}\right)}. \tag{A.22}$$

171

Proceeding as before, this expression can be written in terms of elasticities as

$$\epsilon^E = \frac{\eta \alpha S_E}{\alpha \gamma - \eta \alpha S_w - \eta \gamma S_B}, \tag{A.23}$$

where $S_E = \frac{E(p-1)}{C}$. This representation allows us to confirm the intuitive notion that the income effect should be stronger for the MW group. To see this, it is more straightforward to multiply top and bottom of (A.23) and (A.8) with $\frac{C}{wh+y}$, to obtain

$$\epsilon^E = \frac{\eta \alpha S_{E,N}}{\alpha \gamma S_{c,N} - \eta \alpha S_{w,N} - \eta \gamma S_{B,N}}.$$

$$\text{and} \quad \epsilon^I = \frac{\eta \alpha S_{y,N}}{\alpha \gamma S_{c,N} - \eta \alpha S_{w,N} - \eta \gamma S_{B,N}}. \tag{A.24}$$

Consider an individual at moderate levels of the HW group, for example around \$5 million, $S_{E,N} = \frac{E(p-1)}{N} \approx \frac{E}{N}$, since $t = 0.49$. Then by definition, $S_{E,N}$ cannot be more than 0.2. Conversely, for an average individual in the MW group, $wh+y=\$1.6$ million. As long as this individual receives an inheritance or enjoys capital gains, so that his non-labor wealth is at least \$320,000, then the income effect will be larger.

Obviously such a calculation is very rough and only meant to be indicative. All other parameters have to be approximately equal, which is not implausible for these two individuals. Clearly, this will not hold if we compare an HW individual with someone at the very top of the wealth distribution, but since $S_{E,N}$ tends to 0 as N tends to infinity, the wealth effect for the very wealthy will be almost zero, as

intuitively expected.

Appendix B: Appendix for Chapter 2

B.1   Crime and UI before the Great Recession

In this section, I look at the relationship between crime, unemployment, and unemployment compensation before the crisis. Exploring how crime and UI covary in "normal" times, which is an interesting exercise in its own right, will highlight the problems of inferring causal estimates in the absence of a natural experiment.

Consider the following model, under the aforementioned implicit assumptions regarding aggregation:

$$crime_{ist} = \alpha_i + \beta_1 U_{ist} + \beta_2 UI_{ist} + \gamma_i + \delta_s$$

(B.1)

$$+ \lambda_t + X'_{ist}\theta + \epsilon_{ist}.$$

The unemployment rate is given by U, while UI gives unemployment compensation per unemployed person. In the rest of the paper, use of the term UI will refer to this measure. The vector of covariates X includes relevant controls, while γ, δ and λ are county, state, and time dummies respectively. Use of covariates will be irrelevant to the main discussion and they shall only be used here for illustration.

The natural experiment I exploit is in fact a considerable improvement over previous papers, in that it is notoriously difficult to agree on the proper set of covariates in crime regressions (Horowitz 2004). The subscripts i, s, and t denote county, state, and time respectively. I explicitly subscript for state to underline the fact that unemployment benefits are a state issue, and variation of UI per recipient across counties within a state at a given point in time is only a result of differences across counties in take-up rates, spell lengths, average wages etc, and is thus unlikely to provide sufficient variation.[1] The dependent variable is the log property crime rate, and I will be only referring to property crime henceforth, unless otherwise stated.

The hypothesis to be tested in this exercise is that counties with higher unemployment tend to have higher crime rates, but controlling for unemployment, higher UI is associated with less crime. Unemployment insurance serves as a deterrent for crime for the unemployed, by raising income in the no-crime state, and thus reducing the relative gain from crime. As already mentioned, crime and legal work are far from mutually exclusive. Freeman (1999) argues that "the border between illegal and legal work is porous, not sharp", and presents this as the main reason why unemployment, though robustly positively related to crime, fails to be an overwhelming factor in explaining crime trends.

Table B.1 shows the results from the baseline model in (B.1) under various specifications, for all counties in the US with population over 50,000, from 1994 to 2007.[2] Assuming first a pooled model with no covariates, we see that the longitudinal

---

[1] Of course, with full individual data, benefits received for some period of time for a given recipient are fully explained by wages and spell length.

[2] I restrict the sample to all counties with data for the full time span.

relationship between crime, unemployment, and UI (deflated) is as expected. Crime is positively related to unemployment and negatively related to UI.

With year dummies, to control for aggregate trends, the coefficient are little affected, as the natural source of variation operates at the state level. Adding state dummies instead isolates the action to within-state variation, allowing for national trends. The UI sign is still negative, as there is still enough variation across counties or time to exploit. The former is likely be the result of differences in take-up rates or average wages (assuming these are persistent) and the latter due to unemployment persistence. Demographic differences could affect both. Unfortunately, there are no, to my knowledge, data on take-up rates at the county level, and this would be an interesting question for future research. Restricting the time-span of the sample in either direction reverses the sign, so it seems likely that variation is across time (not shown). It follows that with both state and year dummies, the situation is the same. In this case, the identification is only off within-state, deviations-from-national-mean changes in benefits. As these can only be driven by worsening local labor markets, the sign reverses.[3] Similar results are obtained with county fixed effects.

Table B.2 gives the results of the same model, only this time I add population controls.[4] We see that now addition of any regional dummies reverses the UI sign, in

---

[3]The same would be true if state-specific year effects were added, in which case variation would come from yearly within state variation across counties.

[4]I include population age fractions (under 10, 10 to 14, 15 to 19, 20 to 24, 25 to 44, 45 to 64 and over 54, fraction male and fraction non white. Using finer race controls is tricky because the intercensal estimates have more detailed information starting from 2000, but in a way that data before 2000 are non-nested in that after 1999. I also run the model with fraction non-hispanic white, non-hispanic black and hispanic and nothing changes.

the same manner as before (significant for state fixed effects, insignificant and almost zero for county fixed effects). This tells us that once we account for demographics, the remaining time-series variation is too small to provide decent identification. Furthermore, we can infer that demographics alone create enough longitudinal variation, which implies that ignoring them is likely to cause problems in identification. Put differently, unobserved heterogeneity is likely to have an important time-varying component, and location fixed effects are not sufficient.[5]. Finally, it is clear that some source of external shock, such as the one employed in this paper, is necessary for identification, and the absence of such a shock in the past can explain why the potential effect of UI on crime has not been addressed before. Indeed, most states had not activated EB since the early 1980s recession, long before data quality (especially in terms of more local units of analysis than states) was sufficient and modern estimation techniques for such issues became widespread.

---

[5]As a corollary, once we account for demographics and local fixed effects, the main determinants of UI, average wages and replacement rates, are irrelevant and estimates are unaffected (results not shown).

Table B.1: Log property crime rate, 1994-2007

	(1)	(2)	(3)	(4)	(5)	(6)
Unemp	0.0473***	0.0384***	0.0560***	0.0486***	0.0466***	0.0262***
	(0.0105)	(0.0112)	(0.0148)	(0.0155)	(0.0135)	(0.0089)
UI	-0.0833***	-0.0835***	-0.0189**	0.0378**	-0.0409***	0.0066
	(0.0156)	(0.0190)	(0.0081)	(0.0158)	(0.0065)	(0.0129)
Constant	8.2709***	8.4710***	8.0034***	8.0417***	8.1280***	8.2752***
	(0.0930)	(0.1030)	(0.0689)	(0.1034)	(0.0657)	(0.0700)
R^2	0.098	0.124	0.284	0.323	0.798	0.832
State FE	No	No	Yes	Yes	No	No
Year FE	No	Yes	No	Yes	No	Yes
County FE	No	No	No	No	Yes	Yes

N=10,695, Groups=794, balanced panel. Standard errors clustered at the state level.
Regressions weighted by total county population.

B.2 Further details on crime data

Measurement error is a known issue in the NACJD data. Ehrlich (1996) suggested that log transformation mitigates this issue, by compressing the within group variation. Such a transformation also helps with potential measurement error due to underreporting, as the underreported rates are likely to be proportional to the true crime rate. A well known result in econometrics is the fact that measurement error in the dependent variable does not affect consistency, only reduces efficiency, assuming the measurement error is classical, meaning additive and independent of the true value of the dependent variable. Abrevaya & Hausman (2004) show that, as opposed to a linear model, where such error only results in reduced efficiency, a transformed non-linear model (including logarithmic transformations) can result in inconsistent estimates. The reason is that a term comprised of the measurement

Table B.2: Log property crime rate, more controls, 1994-2007

	(1)	(2)	(3)	(4)	(5)	(6)
Unemp	0.0170	0.0062	0.0278**	0.0197	0.0370***	0.0147**
	(0.0147)	(0.0171)	(0.0133)	(0.0158)	(0.0090)	(0.0060)
UI	-0.0511***	-0.0518***	0.0326**	0.0678***	0.0034	0.0114
	(0.0145)	(0.0172)	(0.0131)	(0.0214)	(0.0054)	(0.0113)
Constant	12.5262***	11.7895***	14.9637***	13.2653***	8.4786***	4.7760***
	(1.5333)	(1.6025)	(1.1527)	(1.1154)	(1.4961)	(1.7046)
R^2	0.265	0.274	0.506	0.520	0.829	0.836
State FE	No	No	Yes	Yes	No	No
Year FE	No	Yes	No	Yes	No	Yes
County FE	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

N=10,695, Groups=794, balanced panel. Standard errors clustered at the state level.
Regressions weighted by total county population.

error and a function of the dependent variable is passed on the error of the right hand side of the model. Right hand side variables, by virtue of being correlated with the dependent variable, will be correlated with the error term, under relatively weak assumptions. This can be dealt with by instrumenting the causal variables of interest, which is exactly the strategy adopted and explained in detail in the empirical strategy section.[6] Furthermore, using logs also helps with the interpretation of coefficients, as level changes would be hard to interpret given the vast variation of crime by county. In any case, it has been shown that a logarithmic specification is indeed optimal, in the sense that crime has an approximate log-normal distribution (Ehrlich 1996), and is the standard used in the literature.

Crime data are by nature heavily underreported. For 2008, the National Crime

---

[6]A good instrument will also give consistent estimates if the error is additive but non-classic i.e. it is correlated with the dependent variable.

Victimization Survey estimates that approximately 57% of all crimes committed across the county were not reported. This is more prominent in violent crimes, except murder, where there is essentially zero underreporting (Gould et al. 2002), and in attempted, but not completed, crimes. For instance attempted rape has only 30% reporting rate, compared to 65% for rape. Property crimes, especially successful one, has much higher reporting rates. Successful burglary with forcible entry has 74% reporting, completed auto theft 91% reporting, and completed theft of $250 or more is 54%. Theft is distinct from robbery in the sense that there is no contact with the perpetrator, so the crime is discovered after being committed. Robbery, which is classified as a personal crime, has a 67% reporting rate if successful (73% if injury is involved).

The literature has identified two main issues with underreporting (summarized in Gould et al. 2002). The first is the fact that underreporting results in measurement error, discussed above. A second issue is the fact that reporting methods, and presumably reporting propensity, varies by state and locality. To the extent that these differences are linear and time-invariant, the first differencing approach employed eliminates this issue.

B.3 Unemployment Insurance System

B.3.1 EUC triggers

EB triggers are described in detail in the main text. Here I describe the ECU triggers.

The initial version of EUC08 became active on July 6, 2008. In order to calculate the extensions that we common across all states, I have to calculate the mean numbers of weeks each program lasted for a given year. So for 2008, the minimum 13 week extension lasted for 19 weeks (July 6 to November 22), and the 20 week extension for another 6. This resulted in a minimum average extension of $(19\times 13 + 6\times 20)/52 = 7.06 \approx 7$ weeks in 2008. In 2009, there were 20 minimum weeks of extensions for the entire year, meaning that each state had at least 13 weeks of extensions without any requirements. In reality the smaller rise in ΔMEW was fifteen weeks, as all states activated some program at one time or another.

B.3.2 Average Weeks of Extensions for 2009, by State

Alaska	51.69	Montana	38.35
Alabama	41.15	North Carolina	53.12
Arkansas	40.29	North Dakota	22.15
Arizona	46.94	Nebraska	22.15
California	50.23	New Hampshire	36.81
Colorado	39.50	New Jersey	49.94
Connecticut	47.81	New Mexico	33.48
District Of Columbia	49.85	Nevada	51.33
Delaware	42.17	New York	44.81
Florida	46.77	Ohio	49.46
Georgia	47.92	Oklahoma	29.23
Hawaii	31.31	Oregon	53.92
Iowa	29.46	Pennsylvania	49.10
Idaho	48.81	Rhode Island	55.23
Illinois	48.48	South Carolina	46.69
Indiana	50.04	South Dakota	22.15
Kansas	36.65	Tennessee	45.62
Kentucky	49.46	Texas	38.83
Louisiana	29.23	Utah	25.08
Massachusetts	47.46	Virginia	40.06
Maryland	32.00	Vermont	44.10
Maine	46.60	Washington	50.94
Michigan	51.92	Wisconsin	49.58
Minnesota	47.69	West Virginia	39.54
Missouri	46.38	Wyoming	26.23
Mississippi	35.23		

Appendix C: Appendix for Chapter 3

This first section of this appendix describes the model, and the second set gives the full set of equation characterizing the equilibrium in the model.

C.1 Model

C.1.1 Consumers

The representative household in country G maximizes

$$E_0 \sum_{t=0}^{\infty} \beta^t U(C_t, L_t), \tag{C.1}$$

where L_t denotes hours of labor, and C_t is a consumption aggregator, given by

$$C_t = \left[\gamma_c^{\frac{1}{\epsilon}} C_{T,t}^{1-\frac{1}{\epsilon}} + (1 - \gamma_c)^{\frac{1}{\epsilon}} C_{N,t}^{1-\frac{1}{\epsilon}} \right]^{\frac{\epsilon}{\epsilon-1}}, \tag{C.2}$$

where $C_{T,t}$ is the tradable consumption index, and $C_{N,t}$ is the non-tradables good, and the definition of shares and exponents follow from above.

Similar conditions hold for the foreign economy, which I omit for brevity in

this and the following section. Throughout the text, starred variables will denote foreign variables.

Aggregate demands Optimal allocations of tradable and non-tradable goods over total consumption are given by

$$C_{T,t} = \gamma_c \left(\frac{(1 + \tau_{T,t}) P_{T,t}}{P_t} \right)^{-\epsilon} C_t; \quad C_{N,t} = (1 - \gamma_c) \left(\frac{(1 + \tau_{N,t}) P_{N,t}}{P_t} \right)^{-\epsilon} C_t \quad \text{(C.3)}$$

where $P_{T,t}$ and $P_{N,t}$ are the price indices for tradable and non-tradable goods, respectively. The price index

$$P_t = \left[\gamma_c (1 + \tau_{T,t})^{1-\epsilon} P_{T,t}^{1-\epsilon} + (1 - \gamma_c)(1 + \tau_{N,t})^{1-\epsilon} P_{N,t}^{1-\epsilon} \right]^{\frac{1}{1-\epsilon}} \quad \text{(C.4)}$$

gives the total consumption CPI (consumer price index). I define τ_T and τ_N to be the VAT rates for tradables and non-tradables respectively. Note that P_t and $P_{C,t}$ are tax-inclusive price indices, while the other indices are tax-exclusive. As VAT is ideally fully rebated, it is equivalent to a consumption tax. The more realistic case of explicitly accounting for partial rebates is more interesting, but analytically cumbersome and outside the scope of this paper. As such, the VAT gap intends to capture the idea that tradables are taxed more heavily due to both exemptions and the fact that imperfect rebates disproportionately affect tradables. Writing price indices in this way also simplifies exposition.

Preferences   Let preferences be given by

$$U(C_t, L_t) = \log(C_t - H_t) - \frac{L_t^{1+\nu}}{1+\nu}, \qquad (C.5)$$

where H is the habit stock, a function of consumption last period, $H_t = hC_{t-1}$, $h \in [0, 1]$. The constant ν is strictly positive, and gives the inverse elasticity of labor supply, respectively. The flow budget constraint is given by

$$P_t C_t + B_t \le R_{t-1} B_{t-1} + W_t L_t + T_t, \qquad (C.6)$$

where B_t are one period uncontingent bonds, W_t is the nominal wage, and T_t firm profits.

Market Incompleteness   The simplest way to capture the fact that Greek households are borrowing in their own currency but at a premium over other EMU members is by introducing a risk premium. Here, r_t, the gross home interest rate, is given by

$$R_t = R_t^* + \Phi(b_t), \qquad (C.7)$$

where $\Phi = \chi \left[\exp \left(\frac{B}{PY} - \frac{B_t}{P_t Y_t} \right) - 1 \right] + z_{r,t}$, where χ is a known constant, and $z_{r,t}$ is a shock to borrowing costs.[1] It is assumed that the representative consumer does

---

[1]It is well known that small open economy models with incomplete markets require such a modeling assumption to induce stationarity, otherwise the equilibrium features a random walk and the steady state features initial conditions. See Schmitt-Grohe & Uribe (2003) for a discussion.

not internalize the effect of her borrowing on the risk premium i.e. she takes $\Phi(.)$ as given.

Consumer Maximization   The first order conditions are:

$$U_{C,t} = \lambda_t P_t \qquad\qquad (C.8)$$

$$U_{L,t} = \lambda_t W_t \qquad\qquad (C.9)$$

$$\lambda_t = \beta R_t E_t \lambda_{t+1}. \qquad\qquad (C.10)$$

The intratemporal condition is given by

$$(C_t - H_t) L_t^{\nu} = \frac{W_t}{P_t}, \qquad\qquad (C.11)$$

and the stochastic Euler Equation by

$$\frac{1}{R_t} = \beta E_t \left\{ \left(\frac{C_{t+1} - H_{t+1}}{C_t - H_t} \right)^{-1} \frac{P_t}{P_{t+1}} \right\}. \qquad\qquad (C.12)$$

Here, $\frac{1}{R_t} = \Lambda_t = E_t\{\Lambda_{t,t+1}\}$ gives the price of the one period no-coupon bond paying $1/P_t$ units of aggregate consumption, and $\Lambda_{t,t+1}$ is the stochastic discount factor between periods t and $t+1$.

C.1.2   Producers

Production is organized as follows. There is a final tradable good consumed by domestic consumers, whose production requires an intermediate tradable input

(either home or foreign). There is also a final non-tradable good, produced by domestic non-tradable intermediates. Energy is not used in final goods production. The final sector can be thought of as the retail sector. The tradables intermediate goods can be sold at home or abroad, and the non-tradable intermediates can also be sold at home. Both types of intermediates are produced with labor and energy, allowing for different factor intensities.

Final goods production is characterized by flexible prices and perfect competition, but intermediate production is monopolistically competitive, with Calvo sticky prices. This is a common approach in New Keynesian models to the model tractable. The setting is very close to Rabanal & Tuesta (2012). Here I will only give the main conditions, see the Appendix for full a derivation.

Distribution costs   Consider first distribution costs. As argued previously, the costs imposed on trade by the logistics sector are not a result of excessive market power in the distribution industry, and hence large mark-ups, but rather several inefficiencies in the truck industry, the rail network, and corruption. As these are much more important for international trade, and since the measure available (from the World Bank trade costs database) is a tariff equivalent, I use the standard "iceberg costs" device common in the trade literature, where a fraction ζ of goods shipped abroad "melts" in transit. This is consistent with a tariff equivalent measure denoted by τ_d, where $(1 - \zeta) = \frac{1}{1+\tau_d}$ (Novy 2007), but more intuitively appealing, as tariffs also produce revenues.

Final Goods The good is produced by a continuum of identical firms, using intermediate goods, with production technology given by

$$Y_{T,t} = \left[\gamma_x^{\frac{1}{\xi}} X_{h,t}^{1-\frac{1}{\xi}} + (1-\gamma_x)^{\frac{1}{\xi}} [(1-\zeta) X_{f,t}]^{1-\frac{1}{\xi}} \right]^{\frac{\xi}{\xi-1}}. \tag{C.13}$$

The share γ_x give the relative intensities of the inputs, and ξ the elasticity of substitution between the amounts of home and foreign traded intermediates, $X_{h,t}$ and $X_{f,t}$ respectively, used in production. These are standard Dixit-Stiglitz aggregators of varieties of home and foreign traded intermediates, with elasticity of substitution σ:

$$X_{h,t} = \left[\int_0^1 X_{h,t}(h)^{1-\frac{1}{\sigma}} dh \right]^{\frac{\sigma}{\sigma-1}} \quad \text{and} \quad X_{f,t} = \left[\int_0^1 X_{f,t}(f)^{1-\frac{1}{\sigma}} df \right]^{\frac{\sigma}{\sigma-1}} \tag{C.14}$$

The optimal demands for intermediate varieties from the final goods producers are given by

$$X_{h,t}(h) = \gamma_x \left(\frac{P_{h,t}(h)}{P_{h,t}} \right)^{-\sigma} \left(\frac{P_{h,t}}{P_{T,t}} \right)^{-\xi} Y_{T,t}, \quad \text{and} \tag{C.15}$$

$$X_{f,t}(f) = (1-\gamma_x) \left(\frac{P_{f,t}(f)}{P_{f,t}} \right)^{-\sigma} (1-\zeta)^{\xi-1} \left(\frac{P_{f,t}}{P_{T,t}} \right)^{-\xi} Y_{T,t}.$$

where

$$P_{h,t} = \left[\int_0^1 P_{h,t}(h)^{1-\sigma} dh \right]^{\frac{1}{1-\sigma}} \quad \text{and} \quad P_{f,t} = \left[\int_0^1 P_{f,t}(f)^{1-\sigma} df \right]^{\frac{1}{1-\sigma}}, \tag{C.16}$$

and the final tradable price level given by $P_{T,t} = \left[\gamma_x P_{h,t}^{1-\xi} + (1-\gamma_x) \bar{P}_{f,t}^{1-\xi} \right]^{\frac{1}{1-\xi}}$, where

$\bar{P}_{f,t} = P_{f,t}/(1-\zeta)$. The producer price is then $P_{f,t}$ but consumers pay $\bar{P}_{f,t}$.

The final non-tradable production is a simple Dixit-Stiglitz aggregator of intermediates, given by

$$Y_{N,t} = \left[\int_0^1 X_{N,t}(n)^{1-\frac{1}{\sigma}} dn \right]^{\frac{\sigma}{\sigma-1}}, \qquad (C.17)$$

with the corresponding price index

$$P_{N,t} = \left[\int_0^1 P_{N,t}(n)^{1-\sigma} dn \right]^{\frac{1}{1-\sigma}}. \qquad (C.18)$$

Intermediate Goods The standard approach in the literature is to consider linear production functions for intermediate goods. Instead, in order to account for energy, I follow Cuche-Curti et al. (2009) and consider a CES production function in energy and labor.

The production technologies are given by the following expressions, for tradable and non-tradable intermediates respectively

$$Y_{h,t}(h) = \left[\gamma_h^{\frac{1}{\alpha_y}} L_{h,t}(h)^{\frac{\alpha_y-1}{\alpha_y}} + (1-\gamma_h)^{\frac{1}{\alpha_y}} E_{h,t}(h)^{\frac{\alpha_y-1}{\alpha_y}} \right]^{\frac{\alpha_y}{\alpha_y-1}} \qquad (C.19)$$

$$Y_{N,t}(n) = \left[\gamma_n^{\frac{1}{\alpha_y}} L_{N,t}(n)^{\frac{\alpha_y-1}{\alpha_y}} + (1-\gamma_n)^{\frac{1}{\alpha_y}} E_{N,t}(n)^{\frac{\alpha_y-1}{\alpha_y}} \right]^{\frac{\alpha_y}{\alpha_y-1}},$$

for $n, h \in [0,1]$.

I introduce price rigidity in the standard Calvo approach, with partial indexation, similar to Rudolf & Zurlinde (2014). In every period, a fraction θ_h of tradables

intermediate producers cannot optimally set prices, and they instead set their price as a function of recent home intermediate tradables inflation:

$$P_{N,t}(n) = P_{N,t-1}(n) \left(\frac{P_{N,t-1}}{P_{N,t-2}} \right)^{\kappa_N}, \tag{C.20}$$

where $\kappa_N \in [0,1]$ is the indexation parameter.

The price-setting non-tradable intermediate goods firms choose their price by maximizing the following profits function:

$$\max_{P_{N,t}(n)} = \sum_{k=0}^{\infty} \theta_N^k \Lambda_{t,t+k} \left\{ \left[\frac{P_{N,t}(n) \left(\frac{P_{N,t+k-1}}{P_{N,t-1}} \right)^{\kappa_N}}{P_{t+k}} - MC_{N,t+k} \right] Y_{N,t+k}^d \right\}, \tag{C.21}$$

subject to the demand for the non-tradables intermediate goods, given by

$$Y_{N,t+k}^d = \left[\frac{P_{N,t}(n)}{P_{N,t+k}} \left(\frac{P_{N,t+k-1}}{P_{N,t-1}} \right)^{\kappa_N} \right]^{-\sigma} Y_{N,t+k}, \tag{C.22}$$

where $Y_{N,t+k}$ is total demand for final non-tradables, as defined above. $MC_{N,t}$ is the marginal cost of production, given by

$$MC_{N,t} = \frac{\left[\gamma_n W_t^{1-\alpha_y} + (1-\gamma_n) P_{EN,t}^{1-\alpha_y} \right]^{\frac{1}{1-\alpha_y}}}{P_t}. \tag{C.23}$$

The price level for non-tradables evolves according to the following expression:

$$P_{N,t} = \left\{ \theta_N \left[P_{N,t-1} \left(\Pi_{N,t-1} \right)^{\kappa_N} \right]^{1-\sigma} + (1-\theta_N) \hat{p_{N,t}}^{1-\sigma} \right\}^{\frac{1}{1-\sigma}}, \tag{C.24}$$

where $\Pi_{N,t-1} = \frac{P_{N,t-1}}{P_{N,t-2}}$ is non-tradable inflation, and $\hat{p}_{N,t}$ is the optimal price by the price-setting firms.

The profit maximization problem for the tradables intermediates is analogous, except that now the demand is given by $Y_{h,t}^d = X_{h,t}(h) + X_{h,t}^*(h)$, where $X_{h,t}^*(h)$ is the foreign demand for home intermediate variety h.

Closing the model Market clearing requires that production equals consumption in both sectors, and the aggregate labor supply equals labor demand. The following conditions hold:

$$Y_{N,t} = C_{N,t} + G_{N,t} \tag{C.25}$$

$$Y_{T,t} = C_{T,t} + G_{T,t}. \tag{C.26}$$

$G_{h,t}$ and $G_{N,t}$ are exogenous government spending shocks, which provide a convenient way to introduce demand shocks to the model. These are financed by lump-sum taxes. Market clearing in the debt market implies

$$B_t + B_t^* = 0, \tag{C.27}$$

while the evolution of net foreign assets is equal to net exports

$$B_t = R_{t-1}B_{t-1} + NX_t. \tag{C.28}$$

Concerning monetary policy, the path of borrowing costs for the government

and the private sector in Greece diverged so much from the path of ECB stance during the crisis that it is not worthwhile to complicate the model by adding explicit monetary rules. I simply set R_t to equal $1/\beta - 1$.

C.2 Equlibrium

C.2.1 Households

The Euler equation is given by

$$\lambda_t = \beta R_t E_t \left\{ \lambda_{t+1} \right\}, \tag{C.29}$$

where λ_t is the marginal utility of consumption

$$\lambda_t = \frac{1}{(C_t - bC_{t-1})P_t}. \tag{C.30}$$

The labor supply decision satisfies

$$L_t^\nu = \lambda_t W_t, \tag{C.31}$$

where

$$L_t = L_{N,t} + L_{h,t}. \tag{C.32}$$

Household demands for final goods are given by

$$C_{T,t} = \gamma_c \left(\frac{(1 + \tau_{T,t}) P_{T,t}}{P_{C_t}} \right)^{-\epsilon} C_t; \quad C_{N,t} = (1 - \gamma_c) \left(\frac{(1 + \tau_{N,t}) P_{N,t}}{P_t} \right)^{-\epsilon} C_t, \quad \text{(C.33)}$$

The CPI is given by

$$P_t = \left[\gamma_c (1 + \tau_{T,t})^{1-\epsilon} P_{T,t}^{1-\epsilon} + (1 - \gamma_c)(1 + \tau_{N,t})^{1-\epsilon} P_{N,t}^{1-\epsilon} \right]^{\frac{1}{1-\epsilon}}. \quad \text{(C.34)}$$

C.2.2 Final Goods Producers

Final tradables are produced according to the following technology

$$Y_t = \left[\gamma_x^{\frac{1}{\xi}} X_{h,t}^{1-\frac{1}{\xi}} + (1 - \gamma_x)^{\frac{1}{\xi}} \left[(1 - \zeta) X_{f,t} \right]^{1-\frac{1}{\xi}} \right]^{\frac{\xi}{\xi-1}}. \quad \text{(C.35)}$$

Demands for intermediate tradable goods from the final goods producers are given by

$$X_{h,t} = \gamma_x \left(\frac{P_{h,t}}{P_{T,t}} \right)^{-\xi} Y_{T,t}, \quad \text{and} \quad X_{f,t} = (1 - \gamma_x)(1 - \zeta)^{\xi-1} \left(\frac{P_{f,t}}{P_{T,t}} \right)^{-\xi} Y_{T,t}.$$

The price indices for final tradables is given by

$$P_{T,t} = \left[\gamma_x P_{h,t}^{1-\xi} + (1 - \gamma_x) \bar{P}_{f,t}^{1-\xi} \right]^{\frac{1}{1-\xi}}, \quad \text{(C.36)}$$

where $\bar{P}_{f,t} = P_{f,t}/(1 - \zeta)$. The trade cost ζ is the iceberg cost of trade, linked to the tariff equivalent by the formula $\tau_d = \frac{\zeta}{1-\zeta}$. The law of one price will hold for

tradable goods (excluding distribution costs), and $P_{h,t} = P_{h,t}^*$, and $P_{f,t} = P_{f,t}^*$.

C.2.3   Intermediate Non-tradable Good Producers

The profit maximization problem of the price-setting firms yields to following solutions:

$$
\frac{\hat{p}_{N,t}(n)}{P_{N,t}} = \frac{\sigma}{\sigma - 1} \left\{ \frac{E_t \sum_{k=0}^{\infty} \theta_N^k \Lambda_{t,t+k} \left(\prod_{s=1}^{k} \frac{(\Pi_{N,t+s-1})^{\kappa_N}}{\Pi_{N,t+s}} \right)^{-\sigma} MC_{N,t+k} Y_{N,t+k}}{E_t \sum_{k=0}^{\infty} \theta_N^k \Lambda_{t,t+k} \left(\prod_{s=1}^{k} \frac{(\Pi_{N,t+s-1})^{\kappa_N}}{\Pi_{N,t+s}} \right)^{1-\sigma} \frac{P_{N,t+k}}{P_{t+k}} Y_{N,t+k}} \right\},
$$

$$(C.37)$$

with

$$
MC_{N,t} = \frac{\left[\gamma_n W_t^{1-\alpha_y} + (1 - \gamma_n) P_{EN,t}^{1-\alpha_y} \right]^{\frac{1}{1-\alpha_y}}}{P_t}.
$$

$$(C.38)$$

The price level for final non-tradables evolves from the following:

$$
P_{N,t} = \left\{ \theta_N \left[P_{N,t-1} \left(\Pi_{N,t-1} \right)^{\kappa_N} \right]^{1-\sigma} + (1 - \theta_N) \hat{p}_{N,t}^{1-\sigma} \right\}^{\frac{1}{1-\sigma}},
$$

$$(C.39)$$

and the production function is given by

$$
Y_{N,t}(n) = \left[\gamma_n^{\frac{1}{\alpha_y}} L_{N,t}(n)^{\frac{\alpha_y - 1}{\alpha_y}} + (1 - \gamma_n)^{\frac{1}{\alpha_y}} E_{N,t}(n)^{\frac{\alpha_y - 1}{\alpha_y}} \right]^{\frac{\alpha_y}{\alpha_y - 1}}.
$$

$$(C.40)$$

C.2.4 Intermediate Tradable Good Producers

The profit maximization problem of the price-setting firms yields to following solutions:

$$\frac{\hat{p}_{h,t}(h)}{P_{h,t}} = \frac{\sigma}{\sigma - 1} \left\{ \frac{E_t \sum_{k=0}^{\infty} \theta_h^k \Lambda_{t,t+k} \left(\prod_{s=1}^{k} \frac{(\Pi_{h,t+s-1})^{\kappa_h}}{\Pi_{h,t+s}} \right)^{-\sigma} MC_{h,t+k} Y_{h,t+k}}{E_t \sum_{k=0}^{\infty} \theta_h^k \Lambda_{t,t+k} \left(\prod_{s=1}^{k} \frac{(\Pi_{h,t+s-1})^{\kappa_h}}{\Pi_{h,t+s}} \right)^{1-\sigma} \frac{P_{h,t+k}}{P_{t+k}} Y_{h,t+k}} \right\}, \quad \text{(C.41)}$$

with

$$MC_{h,t} = \frac{\left[\gamma_h W_t^{1-\alpha_y} + (1-\gamma_h) P_{Eh,t}^{1-\alpha_y} \right]^{\frac{1}{1-\alpha_y}}}{P_t}. \quad \text{(C.42)}$$

The price level for final tradables evolves according to the following expression:

$$P_{h,t} = \left\{ \theta_N \left[P_{h,t-1} \left(\Pi_{h,t-1} \right)^{\kappa_h} \right]^{1-\sigma} + (1-\theta_h) \hat{p}_{h,t}^{1-\sigma} \right\}^{\frac{1}{1-\sigma}}, \quad \text{(C.43)}$$

and the production function is given by the following:

$$Y_{h,t}(h) = \left[\gamma_h^{\frac{1}{\alpha_y}} L_{h,t}(h)^{\frac{\alpha_y-1}{\alpha_y}} + (1-\gamma_h)^{\frac{1}{\alpha_y}} E_{h,t}(h)^{\frac{\alpha_y-1}{\alpha_y}} \right]^{\frac{\alpha_y}{\alpha_y-1}}. \quad \text{(C.44)}$$

C.2.5 Market Clearing

Following Rabanal & Tuesta (2012), I assume that government consumption only falls on the final sector. The market clearing conditions for tradable and non-

tradable final goods is given by

$$Y_{N,t} = C_{N,t} + G_{N,t} \tag{C.45}$$

$$Y_{T,t} = C_{T,t} + G_{T,t}. \tag{C.46}$$

The government runs a balanced budget, so spending is equal to revenue:

$$G_t = \tau_{T,t} P_{T,t} C_{T,t} + \tau_{T,t} P_{T,t} C_{T,t}. \tag{C.47}$$

The condition for market clearing in the intermediate tradable sector is:

$$Y_{h,t}(h) = X_{h,t}(h) + \zeta X_{h,t}^*(h) + (1 - \zeta) X_{h,t}^*(h), \forall h \in [0,1]. \tag{C.48}$$

Writing the condition this way makes clear that a fraction ζ of exports is lost to trade costs, and the rest reaches the foreign economy. Similarly, the condition in the intermediate non-tradable sector is:

$$Y_{N,t}(n) = X_{N,t}(n), \forall n \in [0,1]. \tag{C.49}$$

Concerning the energy sector, the simplest approach is to abstract from issues relating to the processing of crude oil imports, and the domestic production of electricity, and assume all energy is costlessly produced domestically, where one unit of energy requires one unit of output in the respective sector it is used.

The economy imports intermediate tradables, and exports intermediate trad-

ables, and so the trade balance (NX_t) is given by[2]

$$NX_t = \frac{P_{h,t}X^*_{h,t}}{P_t} - \frac{P_{f,t}X_{f,t}}{P_t}. \tag{C.50}$$

Aggregate real GDP is the sum of tradable and non-tradable final goods consumption for the public and private sector:

$$Y_t = \frac{(1+\tau_{T,t})P_{T,t}(C_{T,t}+G_{T,t})}{P_t} + \frac{(1+\tau_{N,t})P_{N,t}(C_{N,t}+G_{N,t})}{P_t}. \tag{C.51}$$

Finally, by definition of national accounting, the change in the net foreign asset position (NFA_t) has to be equal to the trade balance plus net investment income, or, in the context of this model, net returns on bonds held in the previous period. So we have that

$$NX_t = B_t - R_{t-1}B_{t-1}. \tag{C.52}$$

---

[2]Recall that the distribution cost is not part of export revenue. It is however part of import cost.

Bibliography

Abrevaya, J. & Hausman, J. (2004), 'Response Error in a Transformation Model with an Application to Earnings-Equation Estimation', *Econometrics Journal* **7**, 366–388.

Adolfson, M., Laseen, S., Linde, J. & Villani, M. (2007), 'Bayesian Estimation of an Open Economy DSGE Model with Incomplete Pass-Through', *Journal of International Economics* **72(2)**, 481511.

Aizer, A. & Doyle, J. (2013), Juvenile Incarceration, Human Capital and Future Crime: Evidence from Randomly-Assigned Judges. NBER Working Paper 19102.

Allen, R. & Stone, J. (1999), 'Market and Public Policy Mechanisms in Poverty Reduction: The Differential Effects on Property Crime', *Review of Social Economy* **52(2)**, 156–173.

Alvarez, L., Hurtado, S., Sanchez, I. & Thomas, C. (2009), 'The impact of oil price changes on Spanish and Euro Area Consumer Price Inflation', *Bank of Spain Working Paper* .

Angrist, J. (2001), 'Estimation of Limited Dependent Variable Models With Dummy Endogenous Regressors', *Journal of Business and Economics Statistics* **19(1)**, 2–28.

Angrist, J. D. & Krueger, A. B. (1999), Empirical Strategies in Labor Economics, *in* 'Handbook of Labor Economics', Amsterdam: North-Holland.

Angrist, J. & Imbens, G. (1995), 'Two-State Least Squares with Variable Treatment Intensity', *Journal of the American Statistical Association* **90**, 430–442.

Angrist, J. & Pischke, J.-S. (2010), *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press: Princeton NJ.

Arkolakis, C., Doxiadis, A. & Galenianos, M. (2014), The Challenge of Trade Adjustment in Greece, *in* 'Reforming the Greek Economy', MIT Press.

Atkinson, A. & Stiglitz, J. (1976), 'The Design of Tax Structure: Direct versus Indirect Taxation', *Journal of Public Economics* **6(1-2)**, 55–75.

Attanasio, O. & Hoynes, H. (2000), 'Differential Mortality and Wealth Accumulation', *Journal of Human Resources* **35(1)**, 1–29.

Beck, R. A., Lenihan, K. J. & Rossi, P. H. (1980), 'Crime and Poverty: Some Experimental Evidence From Ex-Offenders', *American Sociological Review* **45(5)**, 766–786.

Becker, G. (1968), 'Crime and Punishment: An Economic Approach', *Journal of Political Economy* **76(2)**, 169–217.

Bernheim, B. D., Lemke, R. J. & Scholz, J. K. (2004), 'Do Estate and Gift Taxes Affect the Timing of Private Transfers?', *Journal of Public Economics* **88(12)**, 2617–2634.

Bitler, M. & Hoynes, H. (2013), The More Things Change, the More They Stay the Same: The Safety Net, Living Arrangements, and Poverty in the Great Recession. NBER Working paper 19449.

Blumenthal, D. (2006), 'Employer-Sponsored Health Insurance in the United States - Origins and Implications', *New England Journal of Medicine* **355(1)**, 82–88.

Blundell, R. & MaCurdy, T. (1999), Labor Supply: A Review of Alternative Approaches, *in* 'Handbook of Labor Economics', Amsterdam: North-Holland.

Blundell, R., Meghir, C. & Smith, S. (2004), Pension Incentives and the Pattern of Retirement in the United Kingdom, *in* 'Social Security Programs and Retirement around the World: Micro-Estimation', University of Chicago Press, pp. 643–690.

Bodenstein, M., Erceg, C. & Guerrieri, L. (2008), 'Optimal Monetary Policy with Distinct Core and Headline Inflation Rates', *Journal of Monetary Economics* **55**, 18–33.

Brown, J., Coile, C. & Weisbenner, S. (2010), 'The Effect of Inheritance Receipt on Retirement', *The Review of Economics and Statistics* **92(2)**, 425–434.

Brown, J. & Weisbenner, S. (2004), Intergenerational Transfers and Savings Behavior, *in* 'Perspectives on the Economics of Aging', NBER Chapters, National Bureau of Economic Research, pp. 181–204.

Burstein, A. & Gopinath, G. (2014), International Prices and Exchange Rates, *in* 'Handbook of International Economics, Volume 4', Elsevier.

Caliendo, M., Tatsiramos, K. & Uhlendorff, A. (2009), 'Benefit Duration, Unemployment Duration and Job Match Quality: A Regression-Discontinuity Approach', *Journal of Applied Econometrics* **28(4)**, 604–627.

Calmfors, L. (1998), 'Macroeconomic Policy, Wage Setting, and Employment - What Difference Does the EMU Make?', *Oxford Review of Economic Policy* **14(3)**, 125–151.

Cashin, D. & Unayama, T. (2011), 'The Intertemporal Substitution and Income Effects of a VAT Rate Increase: Evidence from Japan', *RIETI Discussion Paper 11-E-045* .

Chamley, C. (1986), 'Optimal Taxation of Capital Income in General Equilibrium with Infinite Lives', *Econometrica* **54(3)**, 607–22.

Chetty, R. (2008), 'Moral Hazard versus Liquidity and Optimal Unemployment Insurance', *Journal of Political Economy* **116,(2)**, 173234.

Coile, C. (2004), Health Shocks and Couples' Labor Supply Decisions. NBER Working Paper 10810.

Coile, C. & Gruber, J. (2004), The Effect of Social Security on Retirement in the United States, *in* 'Social Security Programs and Retirement around the World: Micro-Estimation', University of Chicago Press, pp. 691–730.

Cragg, J. (1971), 'Some Statistical Models for Limited Dependent Variables With Application to the Demand for Durable Goods', *Econometrica* **39**, 829–844.

Cuche-Curti, N., Dellas, H. & Natal, J.-M. (2009), 'DSGE-CH: A Dynamic Stochastic General Equilibrium Model for Switzerland', *SNB Economic Studies* .

Cullen, J. & Levitt, S. (1999), 'Crime, Urban Flight, and the Consequences for Cities', *Review of Economics and Statistics* **81(2)**, 159–169.

de Mooij, R. & Keen, M. (2012), 'Fiscal Devaluation" and Fiscal Consolidation: The VAT in Troubled Times', *IMF Working Paper* .

Defronzo, J. (1996), 'Welfare and Burglary', *Crime and Delinquency* **42**, 223–229.

Defronzo, J. (1997), 'Welfare and Homicide', *Journal of Research in Crime and Delinquency* **34**, 395–406.

Desai, M. & Hines, J. R. (2005), 'Value-added Taxes and International Trade: The Evidence'. University of Michigan Working Paper.

Diamond, P. & Saez, E. (2011), 'The Case for a Progressive Tax: From Basic Research to Policy Recommendations', *Journal of Economic Perspectives* **25(4)**, 165–190.

Druant, M., Fabiani, S., Kezdi, G., Lamo, A., Martins, F. & Sabbatin, R. (2009), 'How are Firms Wages and Prices Linked. Survey Evidence in Europe', *European Central Bank Working Paper* .

Durlauf, S. N., Navarro, S. & Rivers, D. A. (2010), 'Understanding Aggregate Crime Regressions', *Journal of Econometrics* **158**, 306–317.

Edelstein, P. & Kilian, L. (2007), 'The Response of Business Fixed Investment to Changes in Energy Prices: A Test of Some Hypotheses About the Transmission of Energy Price Shocks', *The B.E. Journal of Macroeconomics* **7(1)**.

Ehrlich, I. (1973), 'Participation in Illegitimate Activities: A Theoretical and Empirical Investigation', *Journal of Political Economy* **81(3)**, 521–565.

Ehrlich, I. (1996), 'Crime, Punishment, and the Market for Offenses', *Journal of Economic Perspectives* **10(1)**, 43–67.

Engelhardt, Bryan, G. R. & Rupert, P. (2008), 'Crime and the Labor Market: A Search Model with Optimal Contracts', *Journal of Public Economics* **92(10-11)**, 1876–1891.

Fagan, J. & Freeman, R. (1999), 'Crime and Work', *Crime and Justice* **25**, 225–290.

Farber, H. & Valletta, R. (2013), Do Extended Unemployment Benefits Lengthen Unemployment Spells? Evidence from Recent Cycles in the U.S. Labor Market. NBER Working Paper 19048.

Farhi, E., Gopinath, G. & Itskhoki, O. (2014), 'Fiscal Devaluation', *Review of Economic Studies* **81**, 725–760.

Farhi, E. & Werning, I. (2010), 'Progressive Estate Taxation', *Quarterly Journal of Economics* **125(2)**, 635–73.

Farhi, E. & Werning, I. (2013), 'Estate Taxation with Altruism Heterogeneity', *American Economic Review* **103(3)**. Forthcoming.

Feldstein, M. & Krugman, P. (1990), International Trade Effects of Value-Added Taxation, *in* 'Taxation in the Global Economy', University of Chicago Press.

Fishback, P. & Kantor, R. J. S. (2010), 'Striking at the Roots of Crime: The Impact of Social Welfare Spending on Crime During the Great Depression', *Journal of Law and Economics* **53(4)**, 715–740.

Foley, F. (2011), 'Welfare Payments and Crime', *Review of Economics and Statistics* **93(1)**, 97–112.

Fougere, D., Kramarz, F. & Pouget, J. (2009), 'Youth Unemployment and Crime in France', *Journal of the European Economic Association* **7(5)**, 909–938.

Freeman, R. B. (1995), The Labor Market, *in* 'Crime', Institute for Contemporary Studies Press: San Francisco.

Freeman, R. B. (1999), The Economics of Crime, *in* 'Handbook of Labor Economics, Vol. 3C', Elsevier: New York.

Friedberg, L. (2000), 'The Labor Supply Effects of the Social Security Earnings Test', *Review of Economics and Statistics* **82(1)**, 48–63.

Gale, W. G. & Scholz, J. K. (1994), 'Intergenerational Transfers and the Accumulation of Wealth', *Journal of Economic Perspectives* **8(4)**, 145–160.

Gali, J. & Monacelli, T. (2005), 'Monetary Policy and Exchange Rate Volatility in a Small Open Economy', *Review of Economic Studies* p. 707734.

Ganong, P. & Liebman, J. (2013), The Decline, Rebound, and Further Rise in SNAP Enrollment: Disentangling Business Cycle Fluctuations and Policy Changes. NBER Working Paper 19363.

Glaeser, E., Sacerdote, B. & Scheinkman, J. (1995), 'Crime and Social Interactions', *Quarterly Journal of Economics* **111(2)**, 507–548.

Gould, E. D., Weinberg, B. A. & Mustard, D. B. (2002), 'Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997', *Review of Economics and Statistics* **84(1)**, 45–61.

Grogger, J. (1998), 'Market Wages and Youth Crime', *Journal of Labor and Economics* **16(4)**, 756–791.

Gruber, J. (1997), 'The Consumption Smoothing Benefits of Unemployment Insurance', *American Economic Review* **87(1)**, 192–205.

Gruber, J. & Hungerman, D. M. (2007), 'Faith-based charity and crowd-out during the Great Depression', *Journal of Public Economics* **91(5-6)**, 1043–1069.

Gruber, J. & Madrian, B. C. (1995), 'Health-Insurance Availability and the Retirement Decision', *American Economic Review* **85(4)**, 938–948.

Hannon, L. & Defronzo, J. (1998*a*), 'The Truly Disadvantaged, Public Assistance, and Crime', *Social Problems* **45**, 432–445.

Hannon, L. & Defronzo, J. (1998*b*), 'Welfare and Property Crime', *Justice Quarterly* **15**, 273–287.

Heckman, J. J. & Vytlacil, E. J. (2007), Understanding Instrumental Variables in Models with Essential Heterogeneity, *in* 'Handbook of Econometrics, Vol. 6B', Elsevier: New York.

Heckman, James J., S. U. & Vytlacil, E. J. (2006), 'Understanding Instrumental Variables in Models with Essential Heterogeneity', *Review of Economics and Statistics* **88(3)**, 389–432.

Hines, J. (2013), 'Income and Substitution Effects of Estate Taxation', *American Economic Review* **103(3)**.

Holtz-Eakin, D., Joulfaian, D. & Rosen, H. (1993), 'The Carnegie Conjecture: Some Empirical Evidence', *The Quarterly Journal of Economics* **108(2)**, 413–35.

Holtz-Eakin, D. & Marples, D. (2001), Distortion Costs of Taxing Wealth Accumulation: Income Versus Estate Taxes. NBER Working Paper 8261.

Horowitz, J. (2004), Statistical Issues in the Evaluation of Right-to-Carry Laws, *in* 'Firearms and Violence: A Critical Review', National Academies Press: Washington, DC.

Imbens, G. & Angrist, J. D. (1994), 'Identification and Estimation of Local Average Treatment Effect', *Econometrica* **62(2)**, 467–475.

Imbens, G., Rubin, D. B. & Sacerdote, B. I. (2001), 'Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players', *American Economic Review* **91(4)**, 778–794.

International Monetary Fund, F. A. D. (2011), *Fiscal Monitor: Addressing Fiscal Challenges to Reduce Economic Risks*, International Monetary Fund.

Jacquinot, P., Mestre, R. & Spitzer, M. (2006), 'An Open-Economy DSGE Model of the Euro Area', *European Central Bank Working Paper* .

Joulfaian, D. (2005), 'Choosing between gifts and bequests: How taxes affect the timing of wealth transfers', *Journal of Public Economics* **89(11-12)**, 2069–2091.

Joulfaian, D. (2006), 'The Behavioral Response of Wealth Accumulation to Estate Taxation: Time Series Evidence', *National Tax Journal* **59(2)**, 253–268.

Joulfaian, D. & Wilhelm, M. (1994), 'Inheritance and Labor Supply', *Journal of Human Resources* **29(4)**, 1205–1234.

Judd, K. (1985), 'Redistributive Taxation in a Simple Perfect Foresight Model', *Journal of Public Economics* **28(1)**, 59–83.

Keane, M. (2011), 'Labor Supply and Taxes: A Survey', *Journal of Economic Literature* **49(4)**, 961–1075.

Kearney, M. S., Harris, B. H., Jacome, E. & Parke, L. (2014), Ten Economic Facts about Crime and Incarceration in the United States, Technical report, The Hamilton Project, Brookings Institution.

Keen, M. & Lockwood, B. (2007), 'The Value-Added Tax: Its Causes and Consequences', *IMF Working Paper* .

Keen, M. & Syed, M. (2006), 'Domestic Taxes and International Trade: Some Evidence', *IMF Working Paper* .

Kennickel, A. (1998), Multiple Imputation in the Survey of Consumer Finances, Technical report, Board of Governors, Federal Reserve System. http://www.federalreserve.gov/econresdata/scf/files/impute98.pdf.

Kilian, L. & Murphy, D. (2014), 'The Role of Inventories and Speculative Trading in the Global Market for Crude Oil', *Journal of Applied Econometrics* **29**.

Kopczuk, W. (2007), 'Bequest and Tax Planning: Evidence from Estate Tax Returns', *The Quarterly Journal of Economics* **122(4)**, 1801–1854.

Kopczuk, W. (2013*a*), 'Incentive Effects of Inheritances and Optimal Estate Taxation', *American Economic Review* **103(3)**.

Kopczuk, W. (2013b), Taxation of Intergenerational Transfers and Wealth, *in* 'Handbook of Public Economics, Vol. 5', Amsterdam: North-Holland.

Kopczuk, W. & Lupton, J. (2007), 'To Leave or Not To Leave: The Distribution of Bequest Motives', *Review of Economic Studies* **74(1)**, 207–235.

Kopczuk, W. & Slemrod, J. (2001), The Impact of the Estate Tax on the Wealth Accumulation and Avoidance Behavior of Donors, *in* 'Rethinking Estate and Gift Taxation', Brookings Institution Press.

Kotlikoff, L. & Summers, L. (1981), 'The Role of Intergenerational Transfers in Aggregate Capital Accumulation', *Journal of Political Economy* **89**, 706–732.

Kyckelhahn, T. & Martin, T. (2013), Justice Expenditures and Employment Extracts, 2010, Technical report, Bureau of Justice Statistics, Office of Justice Programs, U.S. Department of Justice.

Lin, M.-J. (2008), 'Does Unemployment Increase Crime? Evidence from the U.S. Data 1974-2000', *Journal of Human Resources* **43(2)**, 413–436.

Lumsdaine, R. & Mitchell, O. (1999), New Developments in the Economic Analysis of Retirement, *in* 'Handbook of Labor Economics', Amsterdam: North-Holland.

Lumsdaine, R., Stock, J. H. & Wise, D. (1990), 'Efficient Windows and Labor Force Reduction', *Journal of Public Economics* **4(2)**, 131–159.

Lumsdaine, R., Stock, J. H. & Wise, D. (1994), 'Pension Plan Provision and Retirement: Men and Women, Medicare and Models', pp. 183–220.

Machin, S. & Marie, O. (2006), 'Crime and Benefit Sanctions', *Portuguese Economic Journal* **5**, 149–165.

Machin, S. & Meghir, C. (2004), 'Crime and Economic Incentives', *Journal of Human Resources* **39(4)**, 958–979.

Macurdy, T. (1981), 'An Empirical Model of Labor Supply In a Life-Cycle Setting', *Journal of Political Economy* **89(6)**, 1059–85.

McGarry, K. (2000), 'Inter Vivos Transfers or Bequests? Estate Taxes and the Timing of Parental Giving', *Tax Policy and the Economy* **14**, 93–121.

Mitsopoulos, M. (2014), Manufacturing, Competition and Business Environment. Removal of Obstacles - Opening to International Competition, *in* 'Competitiveness for Growth: Policy Proposals', Athens: Hellenic Bank Association.

Modigliani, F. (1988), 'The Role of Intergenerational Transfers and Life Cycle Saving in the Accumulation of Wealth', *Journal of Economic Perspectives* **2**, 15–40.

Monte, L. & Lewis, D. (2011), 'Desperate or Deviant? Causes of Criminal Behavior among TANF Recipients', *Poverty & Public Policy* **3(3)**, Article 6.

Mueller, A. I., Rothstein, J. & von Wachter, T. (2013), Unemployment Insurance and Disability Insurance in the Great Recession. NBER Working Paper 1967.

Mustard, D. (2010), Labor Markets and Crime: New Evidence on an Old Puzzle, *in* 'Handbook on the Economics of Crime', Northampton MA: Edward Elgar.

Natal, J.-M. (2012), 'Monetary Policy Response to Oil Price Shocks', *Journal of Money, Credit and Banking* **44(1)**, 53–101.

Nicholson, M. W. (2013), 'The Impact of Tax Regimes on International Trade Patterns', *Contemporary Economic Policy* **31(4)**, 746–761.

Novy, D. (2007), 'Is the Iceberg Melting Less Quickly? International Trade Costs after World War II', *University of Warwick Working Paper* .

Novy, D. (2013), 'Gravity Redux: Measuring International Trade Costs With Panel Data', *Economic Inquiry* **51(1)**, 101–121.

Nyce, S., Schieber, S., Shoven, J., Sita, S. S. & Wise, D. (2013), 'Does Retiree Health Insurance Encourage Early Retirement?', *Journal of Public Economics* **104**, 40–51.

Papageorgiou, D. (2014), 'BoGGEM: A Dynamic stochastic General Equilibrium Model for Policy Simulations', *Bank of Greece Working Paper* .

Pelagidis, T. (2014), 'Why Internal Devaluation is Not Leading to Export-Led Growth in Greece'. Brookings Institution.
URL: *http://www.brookings.edu/blogs/up-front/posts/2014/09/12-internal-devaluation-export-growth-greece-pelagidis*

Pelagidis, T. & Mitsopoulos, M. (2014), *Greece. From Exit to Recovery?*, Brookings Institution Press.

Piketty, T. (2010), On the Long-Run Evolution of Inheritance: France 1820-2050. PSE Working Papers.

Piketty, T. (2011), 'On the Long-Run Evolution of Inheritance: France 1820-2050', *Quarterly Journal of Economics* **126(3)**, 1071–1131.

Piketty, T. (2014), *Capital in the 21^{s}t Century*, Boston: Harvard University Press.

Piketty, T. & Saez, E. (2003), 'Income Inequality in the United States, 1913-1998', *Quarterly Journal of Economics* **118(1)**, 1–39.

Piketty, T. & Saez, E. (2012), 'A Theory of Capital Income Taxation', NBER. Working Paper 17989.

Piketty, T. & Saez, E. (2013), 'A Theory of Optimal Inheritance Taxation', *Econometrica* **81(5)**, 1851–1886.

Poterba, J. (2001), 'Estate and Gift Taxes and Incentives for Inter Vivos Giving in the US', *Journal of Public Economics* **79(1)**, 237–64.

Rabanal, P. & Tuesta, V. (2012), 'Nontradable Goods and the Real Exchange Rate', *Open Economies Review* .

Raphael, S. & Winter-Ebmer, R. (2001), 'Identifying the Effect of Unemployment on Crime', *Journal of Law and Economics* **44(1)**, 25984.

Roback, J. (1982), 'Wages, Rents, and the Quality of Life', *Journal of Political Economy* **90(6)**, 1257–1278.

Rothstein, J. (2011), 'Unemployment insurance and job search in the Great Recession', *Brookings Papers on Economic Activity* **Fall**, 132–210.

Rudolf, B. & Zurlinde, M. (2014), 'A Compact Open Economy DSGE Model for Switzerland', *SNB Economic Studies* .

Saez, E., Slemrod, J. & Giertz, S. H. (2012), 'The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review', *Journal of Economic Literature* **50(1)**, 3–50.

Santos Silva, J. & Tenreyro, S. (2007), 'The Log of Gravity', *Review of Economics and Statistics* **88(4)**, 641–658.

Schmitt-Grohe, S. & Uribe, M. (2003), 'Closing Small Open Economy Models', *Journal of International Economics* **61**, 163185.

Schneider, F. & Buehn, A. (2012), 'Shadow Economies in Highly Developed OECD Countries: What Are the Driving Forces?'. IZA Discussion Paper No. 6891.

Smets, F. & Wouters, R. (2003), 'An Estimated Dynamic Stochastic General Equilibrium Model of the Euro Area', *Journal of the European Economic Association* **1(5)**, 1123–1175.

Stiglitz, J. (1978), 'Notes on Estate Taxes, Redistribution, and the Concept of Balanced Growth Path Incidence', *Journal of Political Economy* **86(2)**, S137–S150.

Stiglitz, J. & Weiss, A. (1981), 'Credit Rationing in Markets with Imperfect Information', *American Economic Review* **71(3)**, 393–410.

Stock, J. & Wise, D. (1990), 'Pensions Plan Provision and Retirement: Men and Women', *Econometrica* **58(5)**, 1151–1180.

Stock, J. & Yogo, M. (2005), Testing for Weak Instruments in Linear IV Regression, *in* 'Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg', Cambridge: Cambridge University Press, pp. 80–108.

Summers, L. (2012), 'How to fix costly and unjust US tax system', *Financial Times* . December 16.

206

Vegh, C. (2013), *Open Economy Macroeconomics for Developing Countries*, MIT Press.

Wilson, J. Q. (2011), 'Crime and the Great Recession', *City Journal* **21(3)**.

World Bank (2013), 'Greek Logistics: Unlocking Growth Potential through Regulatory Reform and Complementary Measures'.