Essays in Public Economics

by

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DEDICATION

This doctoral dissertation is dedicated to Bríd and Eamonn Hargaden.

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ABSTRACT

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This dissertation comprises three research papers, each of which addresses a question in public economics.

Chapter 1 investigates the response of taxpayers to tax incentives in Ireland before and after the Great Recession. Pre-2009 there is clear evidence of bunching in the income distribution just below thresholds that trigger large tax liabilities. This evidence disappears from 2009 onwards. This suggests that the taxpayer response is weaker during a recession. Much of the difference reflects reduced employment in sectors such as construction that exhibit above-average ability to report tax-advantaged incomes. However, even for people who remained with the same firm over the period the likelihood of reporting a tax-advantaged income fell during the recession.

Chapter 2 examines the link between political fragmentation and tax policy. A model of government is presented where an n-member coalition chooses revenue

and expenditure policies. I derive the response of tax policy to a change in the number of coalition partners. The model predicts that more fragmentation leads to *(i)* lower taxes; *(ii)* lower expenditure; and *(iii)* lower social security transfers. These results run counter to the conventional wisdom that countries with more fragmented governments have larger public sectors. I test the model on a large panel of developed countries, and all three of the model's predictions are supported. I estimate that moving from a two- to three-party legislature lowers tax revenue by 6.7%, expenditure by 9.5%, and transfers by 5.4%.

Chapter 3 tests the relationship between crime and the labour market in Ireland during the boom and bust period 2003–2014. Based on detailed county-level panel data on crime and unemployment register figures, higher unemployment is associated with higher crime rates: I estimate a property crime elasticity of about 0.5. This implies that a 10% rise in numbers on the unemployment register increases thefts and burglaries by 5%. To estimate causal effects, I also test the relationship using a Bartik-style instrumental variable. The IV results confirm that a reduction in employment leads to more crime.

CHAPTER I

Taxpayer Responses over the Cycle: Evidence from Irish Notches

1.1 Introduction

This paper investigates whether employees respond to tax incentives differently during recessions, exploiting a series of natural experiments to identify the response of taxpayers' incomes to changes in tax rates over the business cycle in Ireland, 2005–2013. I find that employees are less responsive during recessions. The paper analyzes the distribution of taxable income in the neighborhood of 'notches', which are thresholds in the income schedule after which after-tax incomes discontinuously drop. Notches provide extremely strong incentives to manipulate reported income. For example, by earning €1 over €26,000 in 2010 an individual's tax liability increased by €1,040. Such unambiguous incentives provide a clean test of people's ability to adjust taxable income. The Irish tax system included notches before, during, and in the immediate aftermath of the Great Recession. During the time period, two new notches were introduced, two old notches were abolished, and the location of the thresholds changed on an almost-annual basis.

There is clear evidence of income manipulation to just below the notches, with excess mass ("bunching") in the earnings distribution at levels that avoid the tax liability. The characteristics of both the employer and the employee are determinants of reporting a tax-advantageous income. For example, people who work for sole proprietors and people who work in the construction sector are more likely to report tax-advantaged incomes, whereas workers in public administration are the least likely to report incomes that avoid the tax liability.

However, the evidence of income manipulation is muted beginning in 2009, and bunching about notches is not statistically significant thereafter. Interpreting the introduction of a notch as a treatment affecting the number of people avoiding an income threshold, the treatment effects from before 2008 are approximately three times as large as those after 2008. Unemployment rose from 4.9% in January 2008 to 13% by October 2009. There is little-to-no evidence of behavioral adjustment after the economic environment deteriorated: the labor force's response to tax incentives during recessions appears muted.

The data source and identifying variation in this paper provide several advantages over other recent papers in the literature on taxpayer responses. Firstly, the data are administrative records provided by the Irish Revenue Commissioners.[1] Employee tax returns in Ireland are filed to the tax authorities directly by their employer, and employees generally make no additional deductions to their taxable income. As wages are tax-deductible for firms and filing incorrect returns is a serious criminal offence, it is unlikely that the results are driven solely by misreporting. Secondly, notches induce sharp changes in the total tax liability of workers (Slemrod, 2013). Unlike estimates based on changes in the marginal rate (Saez, 2010; Bastani and Selin, 2014), estimates come from thresholds where net income falls discontinuously. As the

[1]The data come from a tax agency in a developed country responsible for the collection of a large share of national product: income taxes in 2013 amounted to €17.2bn of a total GNP of €158.3bn. Sources: Department of Finance Databank, CSO Quarterly National Accounts.

changes in tax liability are on average worth about a week's wages, these are large and likely salient magnitudes to workers. Furthermore, the annual budget is announced the month prior to taking force, thus providing people approximately 13 months to adjust reported income. Thirdly, the estimation will exploit the fact that the location of the notches changed almost-annually. I will argue that this additional variation facilitates a more compelling identification strategy than conventional approaches. Fourthly, the notches are local to people earning the average industrial wage. Many previous studies on the effect of tax rates on taxable income (Auten and Carroll, 1999; Feldstein, 1995; Gruber and Saez, 2002) have focused primarily on the effects on higher earners. Lastly, the time period saw particularly volatile movements in output. The data contain both the booming Celtic Tiger economy where GNP growth averaged 5% per annum, and also the Great Recession that immediately followed. The drop in quarterly output during from peak to trough was four times larger in Ireland than in the United States.

The paper includes a variety of methods to investigate the behavioral response. Primarily, the fact that the notch thresholds change facilitates a new difference-bunching estimator. This tests whether the income distribution about a notch changes significantly from the prior year. The conditions for parameter identification using this method are less restrictive than the level-bunching estimates used by e.g. Saez (2010), Chetty et al. (2011), and Kleven and Waseem (2013). The difference-bunching evidence is clear in 2006–2008, but disappears from 2009.

To ensure the results are not caused by shifts in the income distribution, the paper also includes details on more direct tests for between-years differences in the distribution with the Kolmogorov-Smirnov testing procedure. This test is particularly useful as it requires no distributional assumptions and holds even under certain forms of non-stationarity. This method provides more statistical evidence of a muted response from 2009.

Borrowing terminology from the RCT literature, I also take a more parametric approach and estimate a fixed-effect Poisson regression to test if the 'treatment' of introducing a notch results in an excess number of people reporting tax-advantaged incomes just below the notch threshold. The incidence rate ratios are three times larger in 2006–2008 than in 2009–2013. Through placebo tests, this approach can also confirm that the results are robust to omitting years where notch thresholds do not change.

With evidence of cyclicality in mind, I analyze the determinants of reporting tax-advantaged or tax-disadvantaged incomes. The characteristics of both the employee (e.g. age, nationality, full-time status) and the firm (e.g. industry, size, form of incorporation) — and thus the employer-employee pairing — are determinants of reporting behavior. The industries most likely to report tax advantaged incomes include construction and agriculture, while employees in state-owned utilities and public administration do not exhibit bunching in tax-advantaged regions.

As the composition of the labor force changed substantially over the period, I construct counterfactual wage distributions for different years using the method of DiNardo, Fortin and Lemieux (1996). The counterfactual decomposes the excess bunching into a component explained by changes in employer-employee covariates and an unexplained residual component. The counterfactuals confirm that the changing composition of the labor force alters the wage distribution during a recession, and that these changes in characteristics can explain approximately 70% of the temporal variation in reporting behavior. However, the large (30%) residual component shows that composition alone cannot fully explain the cyclical patterns in taxpayer responsiveness.

Finally, using inverse probability weights to control for changes in the characteristics of workers, I confirm that the cyclical variation is not driven purely by the labor force composition effect: the determinants of bunching themselves change over the cycle. For example, self-employed people are always more likely to report tax-advantaged incomes; however, I find that the mean marginal effect (probability of reporting a tax advantaged income) of self-employment income changes from 13% to 24% between 2006 and 2010. Even holding the labor force fixed, there are additional constraints on behavior during the recession.

As people reduce their labor supply, avail of more deductions, and put more effort into hiding activities from tax authorities, changes to income tax rates typically cause changes in the income tax base. One metric that can summarize all margins of adjustment is the change in taxable income itself. The elasticity of taxable income (ETI), a measure of how the total tax base responds to a change in the (net of) tax rate, has risen in prominence to become a statistic of central importance in public finance. There are several reasons for this. Principally, under plausible conditions the ETI is a sufficient statistic for the welfare costs of taxation.[2] The basic intuition is that as the ETI captures all margins of adjustment (evasion, avoidance, decreased effort levels, etc.) it summarizes all costs of taxation. Statistics with such immediate application to welfare analysis are rare in economics, and thus substantial research effort has been expended on the ETI. Furthermore, it is of immediate policy relevance as an estimate of the effects of rate changes on government revenue. Finally, the ETI is a key determinant of the efficient level of redistribution (Slemrod and Kopczuk, 2002). Given any tax system and social preferences for redistribution, the less responsive taxpayers are to tax rates (i.e. the lower the ETI) the higher the optimal amount of redistributive transfers. The ETI is proportional to the extent of bunching at kinks and notches in the income threshold (Saez, 2010; Kleven and

[2]See Chetty (2009) and Doerrenberg et al. (2015) for critical discussion on these conditions.

Waseem, 2013).

This paper adds to the literature on taxpayer responses in three important ways. Firstly, using the newly developed difference-bunching estimator, I identify the response of taxpayers to new notches in the tax code. Secondly, I provide further support for the existing evidence (Devereux, 2004; Feldman and Slemrod, 2007) that the characteristics of both the employer and employee are determinants of the response, e.g. that employees of smaller firms are more likely to report tax-advantageous incomes. Finally, I provide clear evidence that the extent of taxpayer response declines during the recession. I propose that taxpayer responsiveness has a cyclical component.[3]

Cyclicality in the behavioral response of taxpayers carries substantial policy implications. Slemrod and Kopczuk (2002) notes that the ETI depends on both preferences (which are arguably exogenous) and the tax system (which is certainly not exogenous), and therefore that the value of the ETI can be seen as a policy choice. If the ETI also varies with the phase of the business cycle, this insight needs to be generalized. For example, holding tax systems and the concavity of social welfare function constant, a lower ETI during a recession would suggest less resources need be directed for optimal enforcement.

1.2 Understanding Taxpayer Responses

A popular graduate public finance textbook (Salanié, 2011) opens with "Any tax measure will prompt agents to change their behavior so as to pay less taxes." The response of taxpayers to notches in the Irish income tax system, and whether it has a cyclical component, is the research question of this paper. There are several channels

[3]A distinct literature has debated whether the fiscal multiplier has a cyclical component, cf. Auerbach and Gorodnichenko (2013, 2012); Bachmann and Sims (2012).

through which taxpayer responses can change: avoidance, timing, intensity of labor effort, etc. (Saez, Slemrod and Giertz, 2012).

By far the most common measure of the taxpayer response is the elasticity of taxable income (ETI), the percent change in taxable income caused by a percent change in the net of tax rate. Heterogeneous responses are to be expected. Devereux (2004) finds larger labor supply elasticities for women than for men, and Kydland (1984) noted that the hours of less-skilled groups fluctuated relatively more than high-skilled groups over the cycle. Indeed, to the extent that these groups are spread across the economy, it is likely that ETI varies by sector.

Any further constraints imposed on behavior during recessions (e.g. to choose hours) may also affect the ability to change taxable income. There is some suggestive evidence of cyclicality in tax-claiming from the EITC literature: in the data from Chetty, Friedman and Saez (2013) the (median) degree of bunching at EITC kink points peaks in 2007, declines in 2008 and recovers slightly in 2009 for both employees and the self-employed.

In addition to constraints, the composition of the labor force likely affects its responsiveness to taxation. If business cycles create non-random attrition from the labor market, and/or the nature of employer-employee pairings changes during a recession, then the aggregate ETI will vary. Solon, Barsky and Parker (1994) note that changes in the composition of the labor market over the cycle are crucial for measuring real wage cyclically. Similarly, if a recession disproportionately affects 25 year-old male construction workers, and if the ETI for 25 year-old male construction workers does not perfectly match that of the population, then this attrition will influence estimates of ETI. Indeed, a simple sufficient condition for cyclical ETI is non-random attrition from the labor force.

The Great Recession in Ireland saw enormous changes in the labor market, in particular in terms of job losses. In some sectors the margin of adjustment was primarily extensive. Exploiting administrative tax records as a data source[4], let us use the construction sector as an example. While the median pay for construction workers fell by 15% between 2006 and 2012, the numbers employed in the sector fell by 67%. More than half (51%) of the people employed in construction in 2006-2007 were no longer in the data in 2012. The composition of the labor force evidently changed substantially. Perhaps more importantly, the composition of employer-employee pairings changed too. To continue the construction example, of those that remained employed in Ireland in 2012, a large fraction (42%) no longer worked in construction. These people, most of whom were Irish (77%) and male (86%), often found work in considerably different settings. While some got work in quite comparable industries like agriculture/mining (4%) or manufacturing (16%), many found themselves in wholesale/retail (16%), admin and support (13%), or health/social work (10%). The flow of workers both to different jobs and to outside of the labor force was substantial. The importance of these flows will become apparent in the empirical analysis, when we find that the employee-employer pairing is a determinant of reporting behavior.

1.2.1 Empirical Estimates of ETI

One of the earliest estimates of ETI was provided by Feldstein (1995), who used differences in group means to infer an elasticity in excess of 2 for higher-earners.[5] This remains a very high estimate, and the diff-in-diff strategies that generated consensus estimates are typically less than half this size. For example, Auten and Carroll (1999) estimate a tax elasticity of 0.6 from changes created by the Tax Reform Act of 1986, Similarly, Gruber and Saez (2002) estimated the elasticity to be about

[4] The data are outlined in Section 4.1 below.

[5] It should be noted that Feldstein was researching prior to the broad availability of administrative data, and that this estimate was based on a sample size of 57 tax returns.

0.4, with a value about half that for average earners and approximately 0.6 for higher earners, and Singleton (2011) found an elasticity of about 0.25. Subsequent research highlighted the importance of timing as source of variation (Goolsbee, 2000; Sammartino and Weiner, 1997) in taxpayer responses. Timing concerns are unlikely to play a role in this paper as taxes are remitted regularly throughout the year.

Many empirical estimates of the ETI, some of which are mentioned, use stimulus packages as an identification strategy. Stimulus packages are, almost by definition, conducted during recessions. If the responsiveness of the labor force changes over the cycle, then it is likely that estimates would differ if conducted at a peak of a business cycle rather than a trough. Consequently, the profession's reliance on stimulus-based identification strategies may have systematically biased estimates of the ETI downward.

1.2.1.1 Bunching estimates

More recently the influential contribution of Saez (2010) shows that the extent of bunching at a kink or notch can identify ETI, and that ETI will be proportional to the extent of the bunching in the distribution of earners at the kink/notch threshold. Due to the relatively clean identification provided by kinks and notches, the approach has been applied to a large number of settings: for example, in relation to Medicaid (Yelowitz, 1995), retirement savings (Ramnath, 2013), VATs (Onji, 2009; Dharmapala, Slemrod and Wilson, 2011), labor market activation programs (Kline and Tartari, 2015), automobile characteristics (Sallee and Slemrod, 2012), property taxes (Best and Kleven, 2015; Kopczuk and Munroe, 2015; Slemrod et al., 2015), and Spanish corporate tax regulations (Almunia and Lopez-Rodriguez, 2014). Most of the literature has found bunching where theory would predict it, though sometimes the results are of negligible magnitude (Hsieh and Olken, 2014). An emerging literature is beginning to address the dynamics of bunching (Marx, 2012; Mortenson and Whitten, 2015). This paper investigates the null hypothesis that the rate of bunching is consistent over the business cycle.

The most relevant paper is Kleven and Waseem (2013) who investigate bunching about a set of notches in Pakistan, and develop a clever method to decompose bunching into structural elasticities and optimization frictions. However, Kleven and Waseem were restricted by the fact that the notch thresholds in Pakistan were constant over time. This required the authors to make relatively strong assumptions about the extent of bunching at the thresholds. The estimation procedure in this paper exploits the fact the notch thresholds varied, and thus I estimate the ability of taxpayers to respond to *changes* in taxes. The findings are, therefore, of immediate relevance to policymakers considering a change in existing policy and/or the introduction of a new policy.

1.3 Institutional Background

1.3.1 Tax Collection in Ireland

As they are the individuals required to remit employees' tax payments to the Office of the Revenue Commissioners ("Revenue"), the administrative burden of income taxation in Ireland is largely borne by employers. Employers must register each employee with Revenue when they begin working, and employers are also required to calculate and deduct income tax each time payments are made to an employee.[6] Taxpayers may elect to file individually or jointly with a spouse. Via a 'tax credit certificate'[7] issued by Revenue when the employee is registered, the employer has a

[6] Revenue exempt firms from this requirement if the employee earns less than €8 per week.

[7] Tax credits are not easily manipulated. As of 2015 a single individual receives a tax credit of €1,650, and this is doubled for married couples. The primary sources of additional credits are those

record of the correct rate of tax to deduct from an employee's pay/wages. As the majority of tax administration is conducted between Revenue and employers directly, few employees make subsequent changes to taxable income. For example, pension contributions are deducted from gross income before it becomes liable for tax. The mortgage interest deduction is paid at source, by the mortgage provider, typically in the form of a reduced monthly payment.[8] Any remaining changes to taxable income will be a source of measurement error in my data. At the end of the year, employers are required to file a P35L form to Revenue detailing the total taxable income and amount of taxes deducted for each employee.

The principal income tax in Ireland is the Pay As You Earn (PAYE) tax. In 2015 the standard rate of PAYE is 20%, with a higher marginal rate of 40% payable on income in excess of €33,800.[9] Although PAYE is the main source of income tax revenue, there are additional social security-related taxes such as Pay-Related Social Insurance (PRSI). The focus on this paper is on three other taxes that produced notches in take-home pay: the Health Levy, the Income Levy, and the Universal Social Charge (USC).

1.3.2 Health Levy

Officially but less frequently called the Health "Contribution", the Health Levy was instituted by Section 4 of the Health Contributions Act, 1979. An unusual feature of the Health Levy is that its eligibility threshold created a notch in tax-home pay. Income below the threshold was exempt from the Levy, but an individual whose annual earnings exceeded the threshold was liable to pay the Levy on the

available for widows, carers of incapacitated children or relatives, and blind people.

[8] The cost to the Exchequer of the mortgage interest deduction is approximately €350m (Oireachtas Debates, 17 July 2014.)

[9] The €33,800 limit is for a single person. For a married couple the limit is €42,800.

entirety of their income. Earmarked for health expenditure, the eligibility threshold has a long history of change.[10] Unfortunately, the data on the behavioral response to the Health Levy prior to the mid-2000s are not available. From the beginning of the dataset through mid-2009, passing the Health Levy threshold incurred a liability of 2% of total income. From May 2009 until the end of 2010, that liability increased to 4%.

1.3.3 Income Levy

With the deterioration in the public finances caused by the decline in the construction sector and the recapitalization of the banking sector, the Irish government announced the introduction of the Income Levy in 2009. The Income Levy operated in much the same way as the Health Levy, but was activated at a lower income level. With a rate of 2% applicable for two-thirds of the year (1 May–31 December) on incomes of at least €15,028, this implies a liability of €200. The subsequent year it was applicable for the full year, implying a liability of just over €300.

1.3.4 Universal Social Charge

The Universal Social Charge (USC) was introduced in 2011 to replace the Health and Income Levies. The threshold was set at €4,004 in 2011 and increased to €10,036 between 2012 and 2014. In 2015 the threshold was raised to €12,012. A 2% liability in the relevant income range was constant over the period. For comparability to the previous Levies I will not include analysis of the €4,004 threshold in the main body

[10] Announcing changes to the tax system in December 1999, Minister for Finance Charlie McCreevy T.D. stated "As in previous years, the threshold for the payment of the health levy will be increased by £500 from £11,250 to £11,750 per annum." (Oireachtas Debates, Wednesday 1 December 1999)

of the paper.[11]

Table 1.1: Summary of Notch Thresholds and Liabilities

Year	Health Levy	Income Levy	USC	Liability	
2005	20,800			(416)	
2006	22,880			(458)	
2007	24,960			(499)	
2008	26,000			(520)	
2009	26,000	15,028		(867)	(200)
2010	26,000	15,028		(1,040)	(301)
2011			4,004	(80)	
2012			10,036	(201)	
2013			10,036	(201)	
2014			10,036	(201)	

All units are denominated in nominal euros. The right-hand column indicates the additional tax liability incurred from exceeding thresholds on the left. For example, earning €1 in excess of €26,000 in 2008 increased tax liability by €520. Data for 2014 are due for release before the end of 2015.

Table 1.1 summarizes the notch thresholds and associated liabilities. Notches are quite an unusual feature in developed countries' income tax systems. It is unprecedented to have such rich variation in notch thresholds in a developed country. The focus of the empirical estimation in Section 1.4 will be to compare taxpayer responses to these changes conditional on the employer-employee pairing.

1.4 Empirical Estimates

1.4.1 Data

The data for this paper come from a variety of administrative sources provided to the Central Statistics Office (CSO). The primary data source is the annual P35L

[11] The primary result for the €4,004 threshold is that no excess bunching is observed. This is consistent with a reduced ability to manipulate income during a recession, but I am less convinced by this result because the €4,004 threshold is further away from the other thresholds. Analysis is included in the appendices for completeness.

file that Revenue provides to the CSO. This file includes the Personal Public Service Number (PPSN)[12] of the employee, the business identifier of the employer, the number of weeks employed, and the total taxable income of the employee. These data are complete for 2006–2013. The second data source is the Central Business Register (CBR), which includes firms' form of incorporation and NACE sector. The third data source is the Job Churn statistical product, which contains the total reckonable pay of the firm, the total number of employees, the number of hires, and the number of separations. These data are complete for 2006–2013. Although it is a non-random sample for that year, the Job Churn data also include the 2005 taxable income for 'job-stayers', i.e. the 79% of workers who remained with the same employer in 2005 and 2006. The fourth data source is Client Record System (CRS), provided by the Department of Social Protection, which contains the month and year of birth, sex, and nationality of all employees. All sources are merged using PPSNs, creating a panel of all registered employees and employers in the state. A sample of 10% of employees was provided to me by CSO, where the selection was based on random digit sampling, e.g. if the last digit in the employee's PPSN equals 7. It remains a representative sample of the universe of workers from 2006–2013.

There is a noteworthy limitation of the data, namely a lack of information on the marital status of individuals. Specifically, the data do not indicate if an individual is married or, if so, to whom. Consequently, there is a class of people for whom the notches are not relevant: married joint-filers with both spouses working. Married individuals with only one working spouse, or married individuals who are filing separately, are all still affected by the notches. However, married joint-filers with both spouses working will only superficially appear affected by the notches.

I believe this limitation is relatively minor. The inapplicability of the tax thresh-

[12]The PPSN is comparable to a Social Security Number.

old to married joint-filers with two incomes will only tend to bias my results towards zero. Unlike much of the existing literature, I find significant behavioral responses at thresholds despite this attenuation bias. Furthermore, I am primarily interested in comparing responses of taxpayers across the business cycle. The set of people not affected by the notch are (i) married; (ii) jointly filing; and (iii) earning income from both spouses. The number of weddings in Ireland declined by about 5% during the recession[13], and the divorce rate in Ireland is about two-thirds lower than the European average.[14] The decrease in employment over the period likely dominates either of the preceding two conditions, and thus the set of people who are *not* affected by the tax very likely decreases over time. I find that the behavioral response decreases over time, despite this change.

Minimal cleaning of the data was needed. I removed people earning precisely the amount provided by Community Employment Schemes[15], the Carer's Allowance[16], or the spousal earning thresholds.[17] The Community Employment Schemes and Carer's Allowance support thousands of people, creating additional spikes near the notch points. These are government-sponsored non-market programs with binding floors, and so I exclude them to avoid contamination with more traditional forms of income. Similarly, the data always show spikes at the spousal earning thresholds. The notches are not binding on joint-filing dual income spouses. I remove these

[13]Source: CSO *Marriages Registered by Age Group, Bride and Groom, Form of Ceremony and Year*, Table VSA51

[14]Source: Eurostat *Crude divorce rate*, Table tps00013

[15]The Community Employment (CE) schemes have been in effect since the mid-1990s. They are subsidized forms of employment for the long-term unemployed. Approximately 22,000 people were employed in CE Schemes (Oireachtas Debates, 8 February 2012). In 2015, the minimum (and standard) rate of pay was €208 per week.

[16]The Department of Social Protection provides a payment to people who care for others (typically other family members) who for medical reasons require full-time assistance. Approximately 57,000 people are in receipt of a carer's allowance (Oireachtas Debates, 17 June 2014.) In 2015, the rate of payment was €204 per week.

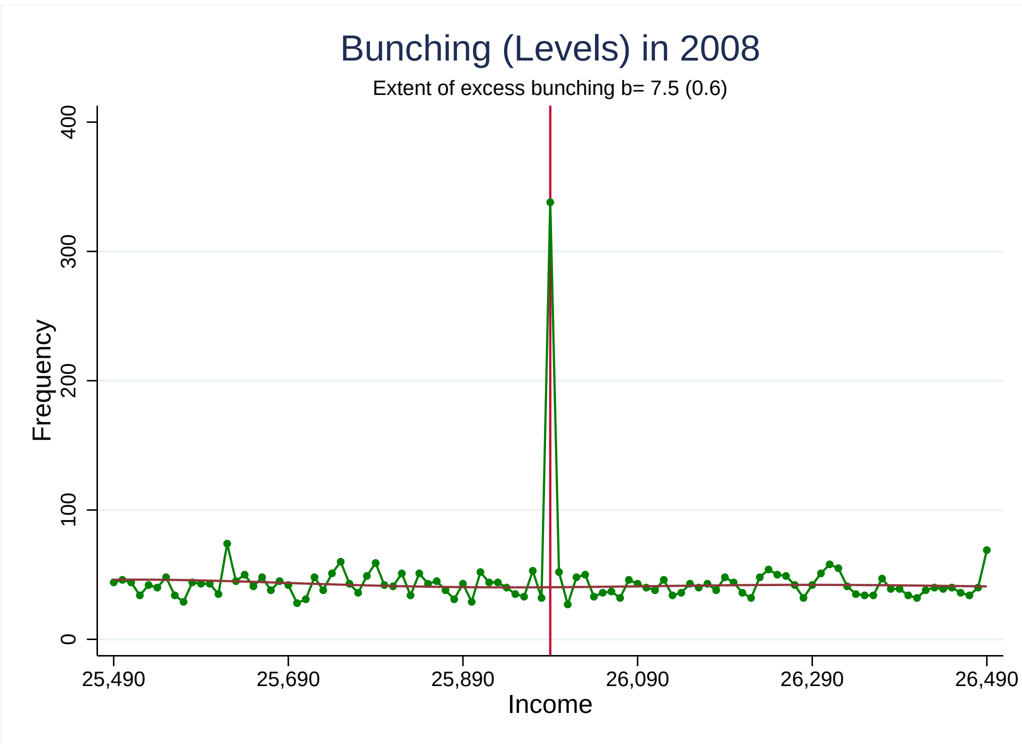
[17]For married couples, there is a kink in the marginal tax rate at the lower-earning spouse threshold. In 2015, the lower-earning spousal threshold was €24,800.

observations from the sample as they are not liable for the notches.

1.4.2 Bunching (Levels)

The first evidence of taxpayer responses can be seen from histograms of the income distribution about a notch. Figure 1.1 shows the clear bunching of people earning between €25,991 and €26,000 in 2008.[18] The green dots indicate the empirical distribution of earnings about a notch at €26,000, represented by the vertical red line. Earnings are grouped into €10 bins.

Figure 1.1: Bunching at the 2008 notch point



The vertical axis measures the total number of people in the 2008 data reporting incomes in the €10 bins defined by the horizontal axis. Note the large spike in the number of people earning an income that just avoids the tax liability triggered by passing the notch threshold.

[18] All notches relate to income exceeding the threshold. Therefore incomes in 2008 of €25,999 or €26,000 were essentially equivalent for tax purposes, but substantially different from an income of €26,001.

Figure 1.1 also includes a high-order polynomial of best fit around the empirical distribution excluding a small window about the notch.[19] This method is original to Chetty, Friedman, Olsen and Pistaferri (2011).[20] By comparing the true size of the bins with the size predicted by the polynomial, we can compute the excess mass ("bunching") at the notch. In particular, the polynomial is constructed by choosing coefficients in

$$B_j = \sum_{i=0}^{q} \beta_i^0 \cdot (\boldsymbol{Z}_j)^i + \sum_{i=-R}^{R} \gamma_i^0 \cdot \mathbb{1}[\boldsymbol{Z}_j = i] + \epsilon_j^0 \tag{1.1}$$

where B_j is the number of individuals in income bin j, $\boldsymbol{Z}_j$ is income relative to the notch thresholds in €10 intervals, q is the order of the polynomial, and R denotes the width of the excluded region around the notch. As with Chetty et al. (2011), I find that the results are not particularly sensitive to the precise specification of the counterfactual, i.e. to small changes in q or R.

We can compare the ratio of the excess number of people to the left of the notch to the number of people predicted by the polynomial. This provides our estimate of the rate of excess bunching b. Bootstrapped standard errors for this estimate are included in parentheses. We can see clear evidence of excess bunching that is statistically significant at all conventional levels. Level-bunching graphs for all notches are included in Appendix A.

1.4.3 Bunching (Differences)

Although the excess bunching is clear in the previous section, it is not apparent whether the bunching is caused by the notch. The requirement needed for parameter identification in level-bunching estimation is that the earnings distribution would be

[19]The provision for estimation excluding a small window permits non-precise bunching.

[20]I am indebted to Tore Olsen for generously sharing the code for this portion of the paper.

flat absent the notch. This assumption is routinely made in the literature (Saez, 2010; Kleven and Waseem, 2013).

However, this assumption is likely violated by several real-world features, the most prominent of which is round-number bunching. Round-number bunching occurs when there is excess mass in the income distribution at numbers such as €25,000. Casual inspection of the earnings distribution in Appendix D shows very strong evidence for round-number bunching. Furthermore, the extent of round-number bunching is not uniform across different round-numbers (for example, there is considerably more bunching at €26,000 than €27,000) and therefore dummy variables capturing round-numbering are unlikely to adequately control for this phenomenon.

The nature of the quasi-experimental variation in this paper, namely that the location of notch thresholds change over time, naturally leads towards an analysis not in levels but in differences. This permits a more compelling identification strategy than the levels-bunching estimation previously employed in the literature. In particular, the identification assumption for this estimator is that the change in the income distribution would have been flat absent the introduction of the notch. The difference-bunching approach eliminates concerns about round-number bunching under the assumption that the level of round-number bunching is not changing from one year to the next.

More formally, suppose we can represent the number of people reporting an income in bin i at time t as

$$B_{it} = a_i + \beta \boldsymbol{X}_{it} + \epsilon_{it}$$

where a_i represents a fixed effect for the income bin i that allows *inter alia* that bin i includes a round-number, X is an indicator variable for whether the bin contains a tax notch, and ϵ is unexplained error. Without changes in X, it is not possible to separately identify a from X. Therefore, identification of β requires that $\mathbb{E}\left[X'\epsilon\right] = 0$ implying that there is no excess bunching inherent to that particular bin. This will be violated if round number bunching is prevalent, e.g. if the notch occurs at incomes exceeding €26,000. However, consider the equivalent estimation through first differences:

$$\Delta B_{it} = \underbrace{\Delta a_i}_{=0} + \beta \Delta \boldsymbol{X}_{it} + \Delta \epsilon_{it}$$

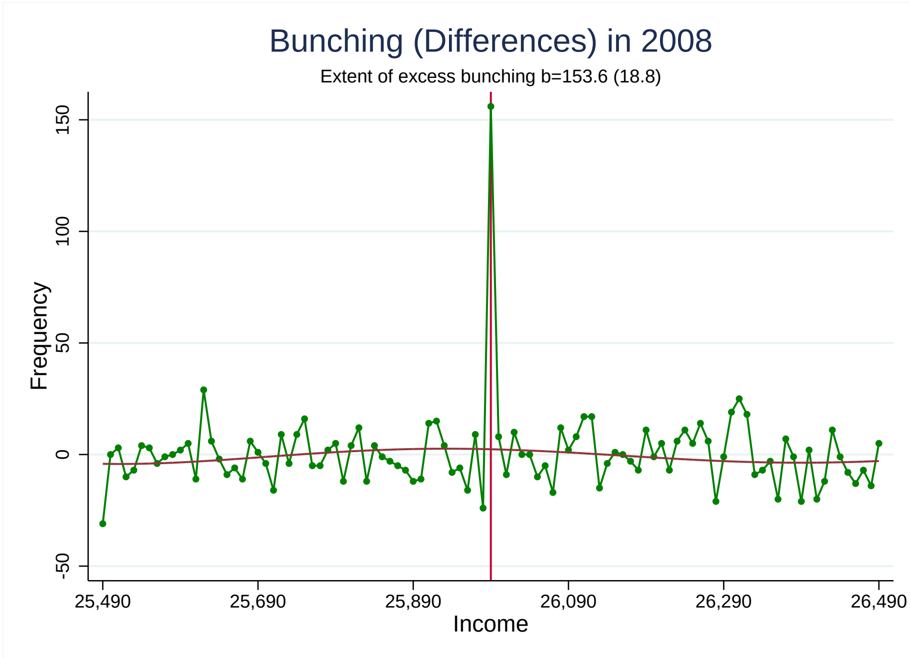
As a_i is a fixed effect, first differences removes any "inherent" excess bunching. By moving from levels to changes, the exclusion restriction is eased substantially. Parameter identification now requires that, relative to the prior year, the excess mass at any particular bin is zero in expectation. Figure 1.2 shows the effect of introducing the notch remains clear in differences.

Figure 1.2 portrays the change in the number of people reporting income in a particular bin between 2007 and 2008. The polynomial of best fit confirms that the expected change in any particular bin is approximately zero. This is equivalent to satisfying a stationarity assumption. There is a clear spike for the €26,000 bin which is consistent with taxpayers reporting incomes to avoid exceeding notch thresholds.

The counter-factual estimates are still generated as per Equation (1.1). Under stationarity the expected change in the number of people in a bin is zero. As the counterfactual has mean zero, it is inappropriate to calculate excess bunching as the ratio of actual bunching to expected bunching. Consequently my measure of excess bunching b is the difference between b_i and $\mathbb{E}\left[b_i\right]$. Inference in the difference-bunching estimator is conducted against a null hypothesis of $H_0 : b = 0$ and the calculated bootstrapped standard errors are included in parentheses. There is clear, obvious, and significant bunching in the early years.

It is at this point of the paper that we begin to analyze taxpayer responses over

Figure 1.2: Difference-bunching at the 2008 notch point



The vertical axis measures the change in the number of people reporting incomes in the €10 bins defined by the horizontal axis between 2007 and 2008. Note the large spike in the number of people earning an income that just avoids the tax liability triggered by passing the notch threshold.

the cycle. We have seen clear evidence of tax-advantageous reporting through 2008. September 2008 saw Lehman Brothers declaring bankruptcy, one of the iconic triggers precipitating the Great Recession. In June 2008, the unemployment rate in Ireland was 6%. Within a year of the collapse of Lehman, unemployment had reached 12.9%. It would continue to rise through 2010-2011 until peaking at 15.1% in early 2012, finally dipping below 10% only in 2015. The 2009 tax year also saw two notches in Ireland: the retention of the notch[21] at €26,000 and the introduction of the Income Levy notch at €15,028. Figures 1.3 and 1.4 plot the difference-bunching estimates for these notches.

In stark contrast to the prior years, the evidence of manipulation disappears

[21]Although the threshold remained the same, the tax penalty for exceeding the €26,000 notch increased from €520 to €867.

in 2009. Note that the vertical axes in Figures 1.3 and 1.4 are the same scale as Figure 1.2. For the introduction of the new notch at €15,028. There is some excess bunching near the threshold, but it is not statistically significant and it is an order of magnitude smaller than the comparable 'new notch' estimates from the prior year. Further evidence is provided for the extant notch at €26,000. One might expect an additional year of 'learning' to increase the number of people bunching at this threshold. In fact, the differential-bunching estimate is actually negative: despite an increase of the liability from €520 to €867 and the additional year for 'learning', fewer people report incomes in the relevant €10 bin than the previous year.

The pattern continues in subsequent years. After 2008, there is no evidence of manipulation of reported incomes about the notches. The full set of figures is included in Appendix B.

Figure 1.3: Difference-bunching at the (pre-existing) 2009 notch point

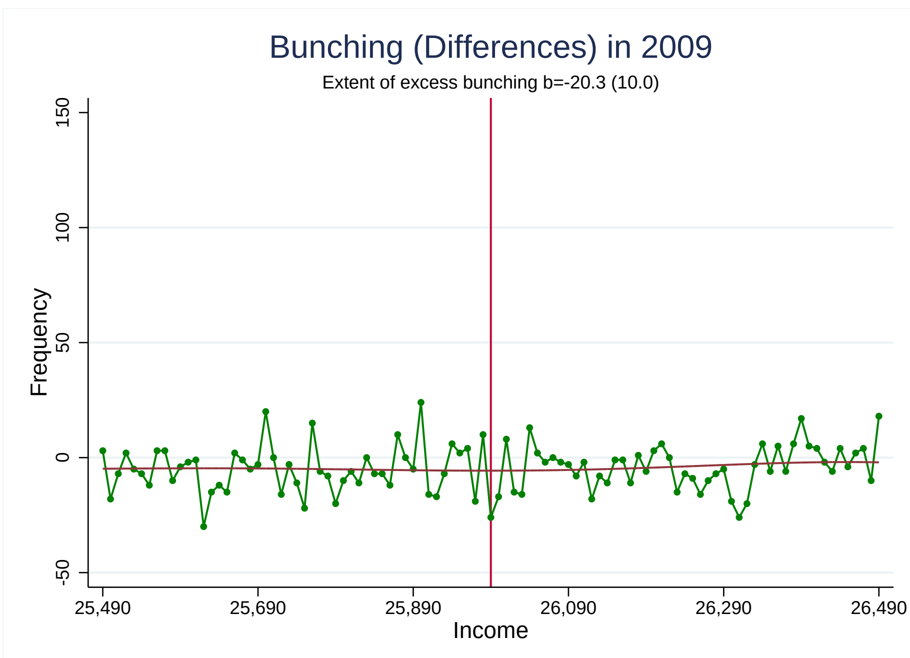
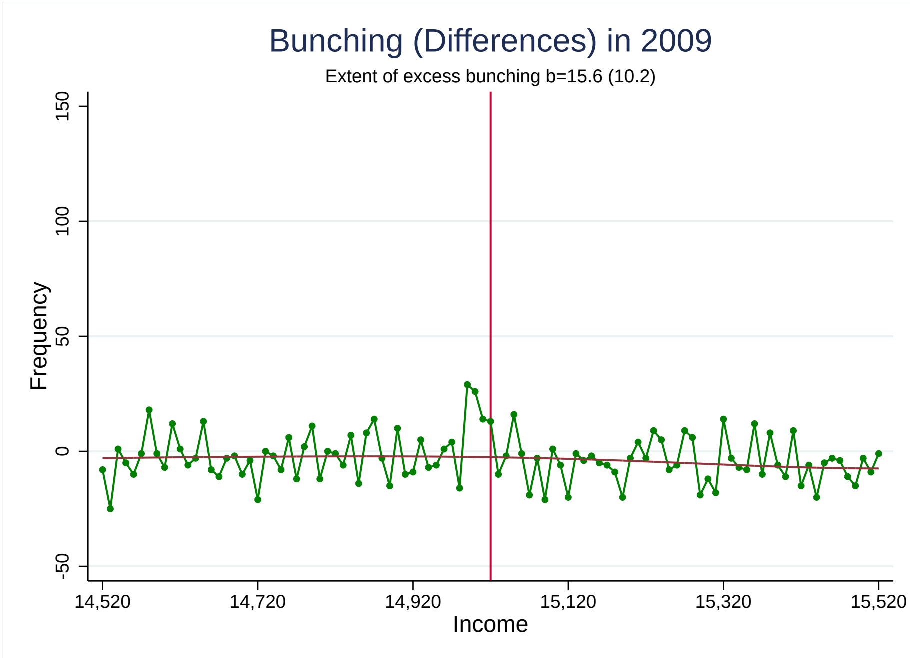


Figure 1.4: Difference-bunching at the (new) 2009 notch point



Recall that the ETI is the percent change in taxable income over the percent change in the (net of) tax rate. By creating a dominated region, notches induce marginal tax rates that exceed 100%. This fact can mechanically result in estimates of ETI appearing small. As a consequence, adjustments of taxable income by one or two percent to avoid a large (>100% marginal tax rate) penalty can seem like a small behavioral response.

However, it must also be noted that large numbers of people report incomes that lie in the strictly dominated range. Indeed, many more people report a dominated income than report an income that just avoids the notch. For example, 217 people (7%) reported an income within ten euro of the threshold in 2007, with 3,019 people (93%) reporting an income in the dominated range.[22] Although this proportion falls quite dramatically (e.g. to 2% for the income levy in 2009) the proportion of people

[22]The 7% figure is inclusive of those reporting that same income in the year prior to the notch. My estimate of those actually responding the notch is closer to 5%.

responding to these notches remains small throughout the entire period. This will further tend to make the absolute level of the behavioral response appear small. Consequently I argue that most insight can be gained from comparing responses over the cycle, and thus that the relative responses are the best point of comparison. Nonetheless, it is informative to directly estimate the ETI for these notches. Doing so requires assumptions on the counterfactual, namely over incomes that the responders would have earned absent the notch. In particular, Kleven and Waseem (2013) show that a reduced form elasticity can be estimated by placing an upper-bound on the incomes at which people respond to the notch. For illustrative purposes suppose, absent the notch, that responders would have been uniformly distributed between the notch threshold and an interval six times the size the dominated region. The dominated region is typically about €500, so this is comparable to assuming that people do not respond to the notch if they earn more than €3,000 above the threshold.[23] Under this counterfactual, we can estimate an ETI of about 0.06 for those that respond.[24] Of course if only 10% of people are responding, then the estimates of ETI for the full population will be ten times smaller; if nobody responds to the notch, the estimated ETI will be zero. I find that the fraction responding varies from 5.2%-5.4% in 2007–2008, to less than 1% in the subsequent years.

1.4.4 Kolmogorov-Smirnov Tests

Under an assumption of stationarity, the difference-bunching estimates above provide a clean test of excess bunching at the specified notch point. This section performs a similar test but generalizes the test to differences in distributions rather

[23] Changes in this assumption will of course change the estimates of ETI. For example, assuming responders come only from the dominated region is equivalent to assuming Leontief preferences.

[24] The implied marginal tax rate for the median agent in this range is 28%, an increase from the pre-notch rate of 20%. Using the notation of Kleven and Waseem and defining the income upper-bound as six times the dominated region, then for the median responder $\Delta z^\star/z^\star \approxeq 0.077$.

than differences at a particular point in distributions. Further, it relaxes the stationarity requirement. The Kolmogorov-Smirnov (KS) statistic is a non-parametric way of comparing two distributions. The KS procedure finds the largest gap between the two distributions, and for this reason it is applicable to testing for bunching. Under a null of two empirical densities being equal, the KS statistic then tests if the largest difference is statistically significant.

The KS test relaxes the stationarity assumption as it compares two probability densities rather than two unscaled histograms. Therefore the test is immune to a multiplying the income distribution by a scalar, i.e. it is immune to transformations that do not affect the relative magnitude of different bins within the window. A related advantage of the KS test is that the test-statistic is a form of rank test, i.e. it is completely distribution free.

Formally let $F(x)$ and $G(x)$ be two empirical distribution functions of sample sizes n_F and n_G with a null $H_0 : F(x) = G(x)$. For our purposes these distributions are the same interval about a notch in the income distribution in adjacent years, e.g. between €25,500 and €26,500 in 2007 and 2008. Now, define the test-statistic D as:

$$D = \sup_x |F(x) - G(x)|$$

The idea of the KS statistic is to measure if D is different enough from its expected value to reject the null. For the calculation of significance, Smirnov (1939) showed that the asymptotic limiting distribution of D has an evaluable form:

$$\lim_{n_F, n_G \to \infty} \mathcal{P}\left\{\sqrt{n_F n_G/(n_F + n_G)} D_{n_F, n_G} \leq z\right\} = 1 - 2\sum_{i=1}^{\infty}(-1)^{i-1} \exp(-2i^2 z^2)$$

The Kolmogorov-Smirnov results for all years are shown in Table 1.2. Interpretation of the results is straightforward. The distributions about the notch from 2006 to 2007 return a test statistic of 0.0236, and the p-value easily rejects the null that the distributions are the same. This means that there is excess mass about the notch in 2007 relative to 2006. Similarly, we can reject the null of no excess mass moving from the 2007 to the 2008 income distributions. However, we find no evidence of excess mass for any year after 2008.

Table 1.2: Kolmogorov-Smirnov Test Results

Year	Test Statistic	p-value
2007	0.0236	0.001
2008	0.0230	0.001
2009	0.0098	0.565
2010	0.0110	0.460
2011	0.0101	0.705
2012	0.0086	0.894
2013	0.0078	0.946
2009	0.0180	0.146
2010	0.0158	0.299

The table shows the test statistic and p-values of differences in distributions about the notches from years 2007 through 2010. The final two rows are for the Income Levy.

1.4.5 Fixed Effect Poisson Estimates

The second estimation procedure used to test for excess bunching is more parametric. The difference-bunching method developed in Section 1.4.3 is based on the changes in the number of people in a €10 income bin. Difference estimates have the advantage of removing any 'inherent' (i.e. time-invariant) properties of particular bins, such as round-number bunching. An alternative empirical approach is to provide each bin with a fixed effect. This will also control for all inherent properties of the bin. Indeed, when $T = 2$, first difference estimation is equivalent to fixed effect estimation.

Particularly when $T > 2$ there are advantages to pursuing both difference- and fixed effect-based estimation procedures. The approach of this section does just that. Borrowing from the RCT literature, we can interpret a notch as a treatment and test if the treatment has a statistically significant effect on the number of people reporting incomes near the affected bin:

$$B_{it} = a_i + \delta_t + \beta \boldsymbol{X}_{it} + \epsilon_{it}$$

where B_{it} is the number of people in income bin i at time t, a_i is a bin fixed effect, δ_t is a year fixed effect, $\boldsymbol{X}_{it}$ is an indicator of treatment status, and ϵ represents an error term. One can estimate this model using OLS with bin fixed effects, but as the number of people in a bin is a count (i.e. a discrete number) it is more appropriate to place restrictions on the error term.[25]

One such structure is provided by the Poisson fixed effect model (Cameron and Trivedi, 2005). The primary advantage of this method is that the estimation is based on changes in the number of people in a bin, and therefore round-number bunching is captured by the bin fixed effects. The Poisson FE estimator is unusual in nonlinear panel models in that does not suffer from an incidental parameters problem. Formally the model specifies that $y_{it} \sim \mathcal{P}\left[\boldsymbol{\alpha}_i \exp(\boldsymbol{x}'_{it}\boldsymbol{\beta})\right]$. Then $\mathbb{E}\left[y_{it}|\boldsymbol{\alpha}_i, \boldsymbol{x}_{it}\right] = \boldsymbol{\alpha}_i \exp(\boldsymbol{x}'_{it}\boldsymbol{\beta}) = \exp(\gamma_i + \boldsymbol{x}'_{it}\boldsymbol{\beta})$.

[25]As a practical matter, the effects reported below are relatively unresponsive to changes in the estimation procedure.

Defining $\lambda_{it} = \exp(\boldsymbol{x}'_{it}\boldsymbol{\beta})$,

$$\ln L(\beta, \alpha) = \ln \left[\prod_i \prod_t \{\exp(-\alpha_i \lambda_{it})(\alpha_i \lambda_{it})^{y_i t} / y_{it}!\,\} \right]$$
$$= \sum_i \left[-\alpha_i \sum_t \lambda_{it} + \ln \alpha_i \sum_t y_{it} + \sum_t y_{it} \ln \lambda_{it} - \sum_t \ln y_{it}! \right]$$

Taking the first-order condition on α_i yields $\hat{\alpha}_i = \sum_t y_{it} / \sum_t \lambda_{it}$. Substituting this back into the log-likelihood equation and differentiating with respect to $\boldsymbol{\beta}$ yields:

$$\sum_i \sum_t \left[y_{it}\boldsymbol{x}_{it} - y_{it} \left(\sum_s \lambda_{is}\boldsymbol{x}_{is} \right) / \left(\sum_s \lambda_{is} \right) \right] = 0$$
$$\Rightarrow \sum_{i=1}^{N} \sum_{t=1}^{T} \boldsymbol{x}_{it} \left(y_{it} - \frac{\lambda_{it}}{\bar{\lambda}_i} \bar{y}_i \right) = 0$$

where again $\lambda_{it} = \exp(\boldsymbol{x}'_{it}\boldsymbol{\beta})$ and $\bar{\lambda}_i = T^{-1} \sum_t \exp(\boldsymbol{x}'_{it}\boldsymbol{\beta})$.

The results of the estimation are shown in Table 1.3. The coefficients of a Poisson regression are slightly complex: in particular $\beta = \log(c_1) - \log(c_0)$ where c_i is the expected count of people in an income bin depending on whether the dummy treatment variable is 1 or 0. For this reason I exponentiate the coefficients so they represent incidence rate ratios (IRRs), where the IRR $= e^{\beta} = \frac{c_1}{c_0}$. The interpretation of the IRR is the factor increase in the count of people in the particular income bin. For example, an IRR of 1.3 indicates a 30% increase in the count of people in the income bin. The null hypothesis for inference is H_0 : IRR = 1. Standard errors are robust to heteroskedasticity.

Column 1 is the primary column in the table, and shows the treatment effects for each year. For example, the coefficient on the Health Levy 2006 dummy indicates that there was more than twice as many people in the effect income bin while that treatment lasted. Consistent with all previous evidence, we find that the responses

Table 1.3: Poisson FE Estimation (with placebos)

	Incidence Rate Ratios			
	(1)	(2)	(3)	(4)
Health Levy 2006	2.161***	2.289***	2.183***	2.223***
	(0.017)	(0.019)	(0.018)	(0.019)
Health Levy 2007	2.473***	2.443***	2.555***	2.627***
	(0.021)	(0.022)	(0.023)	(0.024)
Health Levy 2008	1.374***	1.374***		
	(0.012)	(0.012)		
Health Levy 2009	1.422***		1.422***	
	(0.011)		(0.011)	
Health Levy 2010	1.548***			1.548***
	(0.013)			(0.013)
Income Levy 2009	1.330***		1.299***	
	(0.010)		(0.010)	
Income Levy 2010	1.317***			1.286***
	(0.011)			(0.011)
USC 2012	1.005	0.997	1.030***	0.986*
	(0.009)	(0.008)	(0.009)	(0.008)
USC 2013	0.936***	0.928***	0.960***	0.918***
	(0.009)	(0.009)	(0.009)	(0.008)
Income Bin & Year FE	Yes	Yes	Yes	Yes
Year of Focus	All	2008	2009	2010
N	17760	13320	13320	13320

Table shows the results of Poisson (fixed effect) regression of the effect of notches on the count of people in income bins that avoid the threshold. Coefficients are scaled to incidence rate ratios (IRRs), representing the factor increase in counts. An IRR of one indicates no effect of the treatment. Column 1 includes all years. Treatment effects pre-2008 are several times larger than treatment effects post-2008. Columns 2 through 4 are 'placebo regressions' which test if the results are robust to omitting years.

are significantly larger in early years. The treatment effects are approximately three times as large prior to 2008 than they are afterwards.

Recall that the USC and Income Levy tax penalties are lower in absolute terms than those for the Health Levy. Therefore, if we think there may be fixed costs of adjusting reported income, we may expect a lower incidence ratio for the later Levies than for the Health Levy. However this alone can not explain the pattern of results in Table 1.3.

The largest absolute tax penalties were enforced by the Health Levy between 2008 and 2010, with the penalty in 2010 (€1,040) being more than twice as large as the penalty in 2007 (€499). Despite this, the treatment effect is much larger for the 2007 notch than the 2010 notch. This fact is inconsistent with a fixed cost of adjustment explanation.

Table 1.3 also includes placebo tests of the muted responses post-2008. As the Health Levy threshold at €26,000 did not vary between 2008 and 2010, one might be tempted to think that the fixed effect for the €26,000 income bin is misleadingly inflated by the repeated treatment. This is incorrect. A simple demonstration of this is shown in Columns 2 through 4 where the specification is run omitting 'repeated' years. Although this changes the coefficients, they are only affected after the fourth decimal place; therefore the results are essentially identical. The repeated threshold does not unduly inflate the fixed effect to the detriment of the subsequent treatment effects. The coefficients truly are smaller during the recession.

1.4.6 Who Bunches?

Under the innocuous assumptions that income is increasing in hours worked and that individuals enjoy leisure, notches create regions of the income distribution that are strictly dominated by lower pre-tax incomes. By substituting labor for leisure to escape the tax penalty, individuals can increase both consumption and leisure.

One striking feature of the bunching estimates is the pervasiveness of suboptimal behavior. Even though bunching is clearly observed in the pre-recession years, more people report income in the strictly dominated region than in an interval just below the notch threshold. This has been found in previous research (Kleven and Waseem, 2013; Chetty et al., 2011) and attributed to broadly defined optimization frictions. While it is obvious that the characteristics of the employee will determine reported income, the nature of tax system administration in Ireland means it is likely that the characteristics of the employer also play a role. One contribution of this paper is to provide insight into the employee-employer matches that display suboptimal behavior.

Defining a particular notch threshold as x, we have observed excess bunching to the left of x. Consequently let us call anyone whose annual income Y is close to (within €100) but below the threshold x, i.e. $Y \in (x - 100, x]$ as a "buncher". Who are the bunchers? Who are the "non-bunchers", whose income gross income lies in the strictly dominated region?

$$\text{Dominated Region} = \begin{cases} 1 & \text{if income in dominated region} \\ 0 & \text{if income} \in (\text{Threshold-€100, Threshold}] \end{cases}$$

One useful feature of the dataset is the inclusion of demographic information on the individuals and basic characteristics on the employers. For employees, the relevant demographics are age, nationality, and sex.[26] On the employer side, the most important variables are the number of employees, industry (NACE code), and legal structure. NACE is the standard industry classification code in Europe, comparable

[26]Unfortunately no data on e.g. education are made available to researchers.

to SIC and NAICS in the United States. In particular, the NACE 2 classification system is used.[27] Legal structure captures the form of incorporation.[28] Another variable of interest is the number of weeks during the year the employee worked for the firm, which I split into an indicator for 52 weeks or not.[29]

The binary nature of the Dominated Region variable leads naturally to the standard analytical approaches for limited dependent variables, namely probit and logit models (Wooldridge, 2002). Formally, suppose a latent variable $y_i^\star = \boldsymbol{x}_i\boldsymbol{\beta} + \epsilon_i$ where the error $\epsilon_i \sim \mathcal{N}(0,1)$ and is independent of $\boldsymbol{x}_i$. Then the realization in the data $y_i = \mathbb{1}[y_i^\star > 0]$. The distribution of y_i given $\boldsymbol{x}_i$, then, is $\mathcal{P}(y_i = 1|\boldsymbol{x}_i) = \mathcal{P}(y_i^\star > 0|\boldsymbol{x}_i) = \mathcal{P}(\boldsymbol{x}_i\boldsymbol{\beta} + \epsilon_i > 0|\boldsymbol{x}_i) = \mathcal{P}(\epsilon_i > -\boldsymbol{x}_i\boldsymbol{\beta}|\boldsymbol{x}_i) = \boldsymbol{\Phi}(\boldsymbol{x}_i\boldsymbol{\beta})$ where $\boldsymbol{\Phi}(\cdot)$ denotes the standard normal cumulative distribution function for the probit model. The logit model is essentially the same as probit, but rather than $\boldsymbol{\Phi}(\cdot)$ denoting the standard normal, the operational assumption is that the errors follow a logistic distribution implying that $\boldsymbol{\Phi}(a) = \frac{\exp(a)}{1+\exp(a)}$. Though some instrument would be needed to ascertain causality, estimating the determinants provides insight into the characteristics of those who list incomes in a strictly dominated region relative to a tax-advantaged region. Table 1.4 shows the determinants of reporting income as tax-advantaged or in the strictly dominated region. Due to space constraints I present results for two examples: the Health Levy, and "any notch". The odd-numbered columns in Table 1.4 show estimates using the probit model, and the even-numbered columns show comparable figures using logits. Positive coefficients indicate a greater

[27] The NACE 2 classification system is a four-digit industry identifier. For example, 85 indicates "Education", 855 indicates "Other education", and 8553 indicates "Driving school activities".

[28] Individual proprietorship; Partnership; Co-operative society; Public Limited Company; Private Unlimited Company; Private Limited Company; Statutory Body; Branch of a Foreign Company; Other. There is substantial heterogeneity within and between, say, individual proprietorships and public limited companies. Unfortunately, most companies that would be generally classified as foreign-owned are registered for the purposes of taxation as domestic in Ireland, and thus few firms are registered as foreign-held.

[29] The qualitative interpretation is the same if weeks is used in the continuous form.

likelihood of registering income in the strictly dominated region.

The results are largely consistent with a model that smaller firms in informal sectors are more likely to report tax-advantageous incomes, similar to results found in Feldman and Slemrod (2007). Having multiple jobs makes one less likely to report a tax-advantaged income. One possible cause is that switching employers mid-year makes precise control of annual income more difficult.[30] Consistent with this result, working with an employer for fifty-two weeks of the year implies one is much less likely to be in the strictly dominated region.

Different forms of legal incorporation are strongly associated with different reporting behaviors. The legal incorporation coefficients are relative to the base of 'Sole proprietorships'. The more informal forms of business (sole proprietorships, small partnerships, etc.) increase the likelihood of reporting income in the tax-advantaged region. All coefficients are positive and significant, implying that employees of sole proprietors are the most able to adjust incomes. Sole proprietors are generally smaller companies, and this is consistent with the positive coefficient on firm size. Working for a larger firm makes a person more likely to be in the dominated region. This is perhaps due to the increased transaction costs of renegotiating with many employees through a HR department rather than on the relatively ad-hoc basis available to less established firms. The effect is small, although it is precisely estimated.

The sectoral indicators are relative to the retail/wholesale industry. The sectors with negative coefficients relative to retail, and therefore the sectors with the lowest tendency to report incomes in the dominated region, are construction and agriculture. In both of these sectors, substantive portions of business are conducted in informal settings. The sectors with the largest positive coefficients are utilities and public

[30] It is also more likely that income trajectories ("career concerns") are more relevant for those that actively switch employers.

Table 1.4: Determinants of Exceeding Notch Thresholds

		Health Levy		Any Notch	
		(1)	(2)	(3)	(4)
Personal Characteristics	Number of employers	0.019***	0.021***	0.006	0.006
	Irish	0.003	0.003	0.003	0.003
	EU 2004	0.020**	0.021**	0.020*	0.019**
	Male	0.007	0.006	-0.006	-0.006
	Fifty-two weeks	-0.037***	-0.039***	-0.063***	-0.066***
	Age (decade)	-0.004	-0.004	-0.003	-0.002
	Self-employment	-0.160***	-0.148***	-0.210***	-0.205***
Legal Incorporation	Partnership	0.037**	0.036**	0.042**	0.042***
	Co-Operative	0.053*	0.052*	0.041	0.041
	Public Limited	0.102***	0.105***	0.082***	0.082***
	Private Unlimited	0.106***	0.107***	0.065***	0.063***
	Private Limited	0.054***	0.053***	0.040***	0.039***
	Statutory Body	0.062***	0.063***	0.023*	0.020
	Foreign Branch	0.100***	0.102***	0.048**	0.046**
	Other	0.098***	0.104***	0.063***	0.061***
Industry	Agriculture	-0.035	-0.036	-0.017	-0.016
	Utilities	0.042	0.044	0.042	0.042
	Construction	-0.030***	-0.032***	-0.029**	-0.030**
	Wholesale/retail	-0.020**	-0.020**	0.007	0.008
	Transport	-0.023	-0.024	-0.005	-0.005
	Hotels/restaurants	-0.032**	-0.033**	0.029**	0.030**
	Information Technology	0.015	0.014	0.003	0.002
	Finance	0.020	0.019	0.007	0.006
	Real estate	-0.011	-0.008	-0.008	-0.006
	Professional/scientific	-0.008	-0.010	-0.012	-0.012
	Admin and support	-0.008	-0.010	0.010	0.010
	Public Administration	0.032**	0.032*	0.030**	0.030*
	Education	0.002	0.001	0.012	0.014
	Health/social work	0.004	0.002	0.011	0.012
	Arts	-0.008	-0.008	-0.006	-0.005
	Other service	-0.028	-0.028	0.000	0.001
	Within house/other	0.008	0.009	0.027	0.030
Firm Size	Employees ('000s)	0.002**	0.002*	0.002***	0.002***
	Workforce separation (%)	0.000	0.000	0.000*	0.000**
Sample Size		20,625	20,625	33,508	33,508
Wald χ^2		787.6	815.4	2955.1	2790.3
(Pseudo-)R^2		0.059	0.059	0.084	0.084

Table shows average marginal effects for Probit (odd-numbered columns) and Logit (even-numbered) regressions on reporting an income that fell in the strictly dominated tax-disadvantaged region relative to reporting income below but within €100 of the notch threshold. All regressions include year fixed effects and are clustered at the individual level. Legal incorporation effects are relative to a base value of Sole proprietorships. Industry effects are relative to a base value of Manufacturing.

administration. The utilities in Ireland are almost own state-owned enterprises. Construction firms avoid taxes, but government agencies do not. This finding is consistent with Paulus (2015).

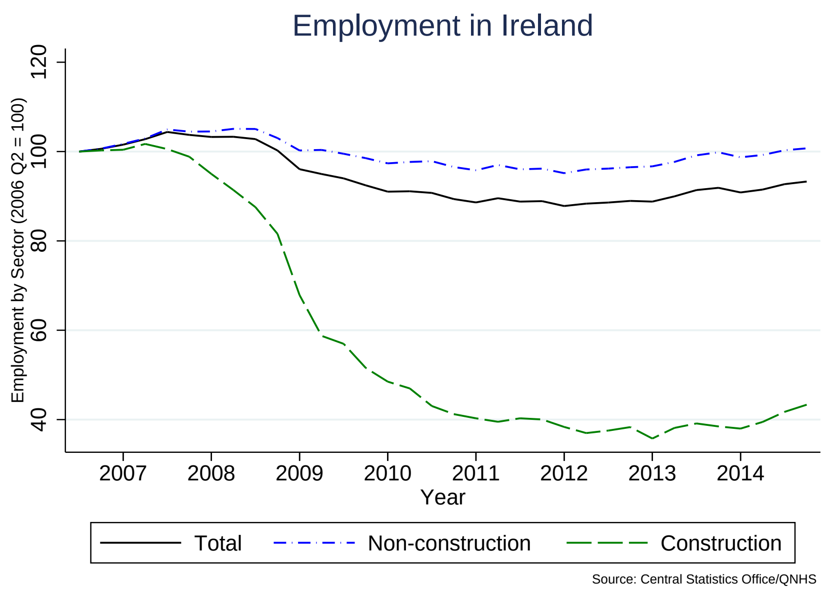
Men are more likely to be in the dominated region for the Health Levy than women, but less likely to be in the dominated region for other notches. This result is perhaps surprising, given the evidence that women have a larger labor supply elasticity (Devereux, 2004). Irish people are slightly more likely to be in the dominated region than non-natives, but this result is not significant. Citizens of countries that only joined the EU in 2004, who generally have little experience with the Irish tax system, are more likely to report tax disadvantaged incomes.

Overall we conclude that the characteristics of both employer and employee (the "employer-employee pairing") predict reporting behavior. The importance of this result becomes more apparent when one considers the employer-employee matches most likely to be affected by the economic downturn in Ireland.

1.4.7 Counterfactual Estimates

As emphasized above, Ireland experienced severe business cycle volatility between 2006 and 2013. The changes in the labor market were stark. Unemployment rose from 4.9% at the start of 2008 to 13.1% by January 2010. It peaked at 15.2% in 2012. Further, the high unemployment rate omits the large number of discouraged workers: in a country with a population of 4.5 million, the labor force declined by 340,000 people.

Figure 1.5: Displacement of construction workers



The effect was widespread but fell disproportionately on the construction sector, an industry traditionally associated with employment of younger men. Figure 1.5 shows that the level of employment in all non-construction sectors was approximately constant over the decade, whereas the numbers employed in construction fell by more than half.

The method developed in DiNardo, Fortin and Lemieux (1996), hereinafter "DFL", is well-suited to a decomposition of bunching at notches and has been used previously in the literature (Onji, 2009). Rather than bluntly removing certain segments of the data, the DFL technique asks what would the income distribution look like in year t had it the characteristics from year $t-1$.

The DFL decomposition is a generalization of the method of Oaxaca and Blinder. The Oaxaca-Blinder decomposition can be seen as measuring how the average pay of how one group (e.g. women) would be paid if they had another group's (e.g. men's) characteristics. Instead of focusing attention exclusively on the mean, the

DFL technique generalizes this intuition to the entire distribution of wages.

When applied to this paper, the DFL technique constructs a counterfactual wage density after the introduction of a notch had the employer-employee attributes remained as they were before the notch. That is, it decomposes the excess bunching into a component explained by changes in the characteristics of the employer-employee match and an unexplained residual component. The component explained by differences in attributes is the gap between the actual post-notch distribution and the generated counterfactual distribution. The residual component is attributed as due to the notch.

Consider the case of the introduction of a notch in 2007. The question is not just how the wage distribution changed between 2006 and 2007, but to disentangle the changes in the composition of the labor force from the excess bunching due to the introduction of the notch itself. The formal explanation below follows DiNardo (2002). From the definition of conditional probability, note that we can define the observed income distributions in 2006 in 2007 as:

$$\int f^{2006}(y)dy \equiv \int f^{2006}(y|x)h(x|t=2006)dx$$
$$\int f^{2007}(y)dy \equiv \int f^{2007}(y|x)h(x|t=2007)dx$$

Now consider the distribution of wages in 2007 if the covariates x had the same distribution as in 2006, and compare it to the actual distribution of wages in 2007:

$$\text{Counterfactual } f^{2007}_{2006}(y) \equiv \int f^{2007}(y|x)h(x|t=2006)dx$$
$$\text{Actual } f^{2007}_{2007}(y) \equiv \int f^{2007}(y|x)h(x|t=2007)dx$$

Note that the counterfactual described differs from its actual distribution only in what set of x variables are to be "integrated over". In general $h(x)$ will have several explanatory variables and integration over several covariates could be impossible.

As in the Oaxaca-Blinder case, the key will be to define a weight θ such that

$$\int f^{2007}(y|x)h(x|t=2006)dx = \int \theta f^{2007}(y|x)h(x|t=2007)dx$$

Such a weight would transform a potentially impossible problem into a relatively simple one: weighting the 2007 distribution. To do so, pool the 2006 and 2007 data and observe that by definition:

$$h(x_j = x_0) = \frac{h(x_j|t=2006)P_{2006}}{\mathcal{P}(t=2007|x_j=x_0)}$$
$$h(x_j = x_0) = \frac{h(x_j|t=2007)P_{2007}}{\mathcal{P}(t=2007|x_j=x_0)}$$

where P_{2007} is the probability that the observation is from 2007 and

$$\rho^{2007}(x) \equiv \mathcal{P}(t=2007|x_j=x_0)$$
$$\rho^{2006}(x) \equiv \mathcal{P}(t=2006|x_j=x_0)$$

are the propensity scores associated with being either post- or pre-notch. The insight here is that propensity scores are much easier to integrate over than multidimensional objects like $h(x_j|t=2006)$. Estimates for ρ can be generated from a logit of the pooled data with the year of notch introduction (or previous year) being the dependent variable. The predicted probability from the logit is an estimate of the propensity score.

The actual and counterfactual distributions differ only in the $h(\cdot)$ term. The weight is a function of the propensity score and two constants:

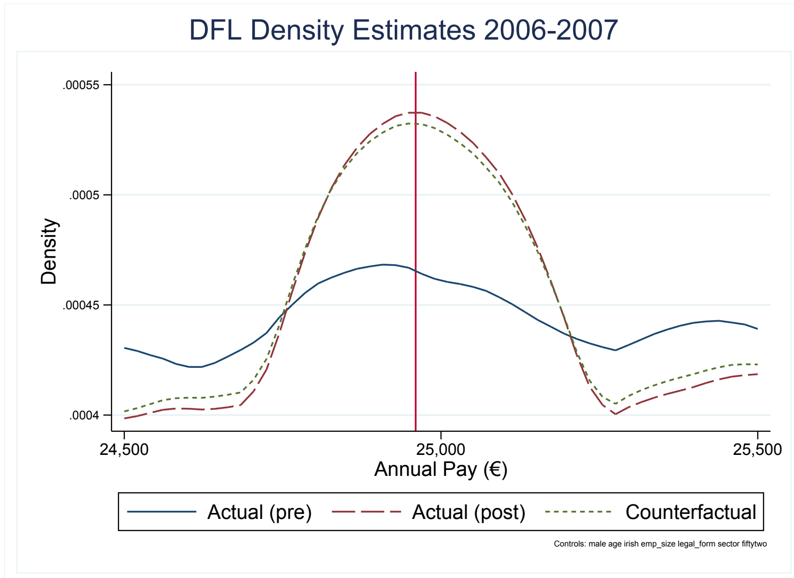
$$
\begin{aligned}
\text{Counterfactual } f_{2006}^{2007} &\equiv \int f^{2007}(y|x)h(x|t=2006)dx \\
&= \int \left(\frac{\rho^{2006}(x)}{1-\rho^{2007}(x)}\right)\left(\frac{P_{2007}}{P_{2006}}\right) f^{2007}(y|x)h(x|t=2006)dx \\
&= \int \theta f^{2007}(y|x)h(x|t=2007)dx
\end{aligned}
$$

DiNardo et al. (1996) propose estimating $\hat{\rho}^{2006}$ with a probit or a logit, and using the results to construct sampling weights $\hat{\theta}_i$. Normalizing these weights so that they sum to unity lets the researcher construct counterfactual (kernel-based) wage densities.

Figures 1.6 and 1.7 show these counterfactual densities for the notches from 2007 and 2010. The figures have three lines. The first two are the actual observed densities before and after the introduction of the notch. We observe excess bunching about the notch the year of its introduction. The dotted line represents the counterfactual estimates. The counterfactual is constructed by scaling the original distribution based on changes in the characteristics of the employer-employee match. The gap between the actual distribution post-notch and the counterfactual is the unexplained residual component. As we can see in Figure 1.6, the counterfactual can explain a considerable portion of the gap between the two empirical densities, albeit with some excess bunching remaining.

This evidence indicates that the composition bias can explain much of the observed change in densities. However Figure 1.7 provides a less compelling case. Although the counterfactual can explain approximately half of the difference in distributions, the relative size of the residual component has increased substantially relative to Figure 1.6. This provides initial evidence that the composition effect alone cannot explain the cyclicality in responses, and that there may be additional constraints on behavior over the cycle.

Figure 1.6: Counterfactual estimates of bunching based on changes in covariates



Counterfactual wage density from 2006 to 2007. Accounting for changes in the characteristics of the employer-employee match explains much of the difference in the empirical distributions.

Figure 1.7: Counterfactual estimates of bunching based on changes in covariates

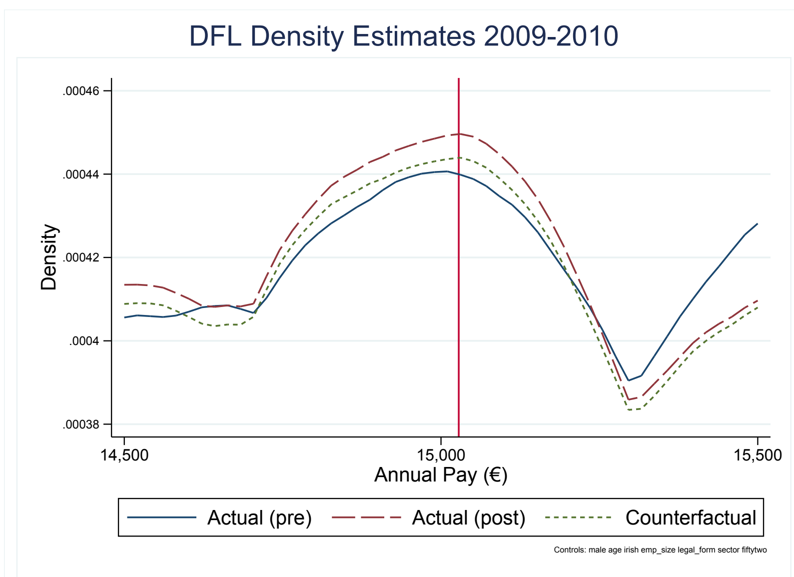


Figure 1.7 shows the counterfactual wage density from 2009 to 2010. Changes in the covariates do a less impressive job matching the 2010 density.

1.4.8 Reweighted Estimates

The preceding analysis suggests that the changes in the labor force can explain much of the cyclical pattern in bunching behavior. For example, the recession disproportionately affected construction workers who were relatively adept at avoiding the notch. However the counterfactuals' explanatory power are far from perfect; a residual exists. This section investigates the residual component. Exactly how much of the change in bunching can be explained by changes in the composition of the labor force? Holding the composition of the labor force fixed, do the predictors of bunching themselves change over the cycle? Do conditions change for those that kept their jobs?

Although the labor market changed most dramatically for those that lost their jobs, there is no question that conditions also changed for those that remained in employment. For example, within the constant subset of employees who remained matched with the same firm, Table 1.5 shows that the dynamics of wage setting changed dramatically over the period.

Table 1.5: Changes in wage dynamics for job-stayers

Year	Proportion Freezes	Proportion Cuts	Proportion Increases
2005-06	2.5%	17.2%	80.4%
2006-07	2.5%	17.6%	79.9%
2007-08	2.8%	22.9%	74.2%
2008-09	3.3%	52.7%	44.0%
2009-10	4.4%	55.2%	40.3%
2010-11	6.8%	39.3%	53.9%
2011-12	10.3%	34.2%	55.5%
2012-13	10.0%	34.3%	55.7%

Table shows the proportion of job-stayers whose pay was frozen within ±0.1% of previous year's pay, cut, or increased. Source: Doris et al. (2013)

This table shows, each year, the percent of "job-stayers"[31] who faced wage freezes,

[31] Job-stayers are people who remained in employment with the same firm over the entire sample.

cuts, or increases. There are several noteworthy features of this table. Primarily, we note that *a majority* of workers received cuts to nominal pay in both 2009 and 2010. This shows a remarkable degree of flexibility in the labor market. The fact that a labor market with such wage flexibility saw a dramatic decrease in the extensive margin of labor demand has been the source of research (Doris et al., 2013).

It is also remarkable that the proportion of people experiencing pay-freezes quadrupled between 2007 and 2012. Albeit determined by the nature of proportions, the final column confirms that the proportion receiving pay increases halved between 2006 and 2010. These are clear indicators that the circumstances of employment substantially changed over the period, even for those that saw no perturbation in their employer-employee match. This table confirms the intuitive notion that recessions affect not just the circumstances of those that lose their job, but also the circumstances of people that remain in employment.

In this section I investigate this further by reweighting the analysis for pre- and post-recession workers. In this respect, we compare the post-recession sample as if they had the same observable characteristics as the pre-recession sample. Conditional on the effectiveness of the reweighting procedure, this holds the composition of the labor force constant between the two periods. This provides a sense of the importance of compositional effects.

Any two samples can be reweighted to look similar to each other in observed characteristics. I present 2006 and 2010 for illustrative purposes, as they represent two years of the same notch (the Health Levy) either side of 2008. This reweighting is achieved through inverse probability weighting. Inverse probability weighting is a procedure wherein observations are given weights according to their probability of attriting from the sample based on their covariates. For example, 2010 observations who work in construction are given additional weight to compensate for the loss of jobs in construction between 2006 and 2010. I base the reweighting on covariates that are less controllable by worker alone: age, gender, Irish citizenship, form of incorporation, and sector. The results are surprisingly insensitive to variations of this procedure, e.g. including firm size as a matching variable. In an effort to statistically decompose as much of the variation as possible, I also reweight based on a full suite of characteristics (full-time status, number of employers, self-employment status, age, nationality, sex, firm size, firm sector, legal form of incorporation, workforce separation percentage), though these additional variables do not alter explanatory power substantially.

Table 1.6 shows how the reweighted 2010 sample compares to the 2006 sample. The analysis is restricted to people who earned between €20,000 and €30,000. The procedure does a good job: the characteristics are quite similar. For example, recall from Figure 1.5 that the fraction of workers employed in construction decreased by about half (from 11% to 6%) between 2006 and 2010. After reweighting the 2010 sample, the difference is closer to 1%.

Table 1.6: Sample means after re-weighting

	2006	2010	Difference
Age	35.6	35.6	0.07
Self-employed (%)	4.1	4.6	0.5
Irish (%)	67.6	67.8	0.25
Construction (%)	11.3	12.3	1.01
Male (%)	53.2	53.7	0.52
Firm size	1,461	1,244	217

Table shows the means of key variables after reweighting the 2010 sample to have the same characteristics as the 2006 sample.

Using the reweighted sample, we can analyze the determinants of bunching absent any bias due to changes in labor force composition. Table 1.7 presents evidence

Table 1.7: IPW-adjusted marginal effects

	2006	2010 (reweighted)
Number of employers	0.0089	0.0218*
Firm Size ('000s)	-0.0009	0.0056**
Fifty-two weeks	-0.0335**	0.0267***
Age (decade)	-0.0057	-0.005
Self-employment	-0.1278***	-0.2378***
Irish	0.0183	0.0105
Male	-0.0038	-0.0185**
EU 2004	0.0298	0.0495***
Workforce separation (%)	-0.0004**	0.0006***

Table shows selected mean marginal effects from probit regression on the determinants of bunching. The first column shows the results for the notch at €22,880 in 2006. The second column shows the results for the 2010 notch, but with the results weighted so the covariates match those of the 2006 sample.

that the economic environment of those that remained in employment changed, and in particular that the determinants of bunching varied. The first column of results in Table 1.7 shows the marginal effects of a selection of covariates from a probit on the probability of reporting a tax-disadvantaged income in 2006. Recall that, for example, self-employed people have a lower probability of reporting a tax-disadvantaged income. The right-hand column of Table 1.7 presents the results for the 2010, but reweighting the sample to 'look like' the 2006 sample. We can thus interpret the right-hand column of Table 1.7 as the determinants of bunching "as if the composition of the labor force were unchanged" from 2006.

The overall impression is similar, but note that the coefficients do change. For example, although neither are substantially different from zero, the marginal effect of having multiple employers approximately doubles. Note also that the the marginal effect of workforce separation[32] switches sign. This could indicate that the nature of separations vary, for example from voluntary to enforced, between 2006 and 2010. The self-employment indicator is highly significant and is the largest absolute determinant in both specifications, but the size of the effect doubles.

It is reassuring that the results between the 2006 sample and the 2010 reweighted sample largely coincide.[33] Nonetheless, the marginal effects move a non-trivial amount. As discussed above, workforce separation (and full-time status) change sign in an important way. Many coefficients (number of employers, firm size, male, citizen of EU 2004 enlargement countries) move from statistically insignificant to significant. As discussed in the context of the pre-recession determinants of bunching, one strategy the literature has proposed that may rationalize reporting an income in the dominated region is 'career concerns', i.e. that the long-term benefits of maintaining a given wage may outweigh the short-term tax consequences. Similarly, it is likely that long-term stability may be closer to the fore during a recession. For example, suppose demanding a renegotiation of wages increases the probability of losing a job; then, if outside options are lower during a recession, it may be rational for people to forego salary renegotiations despite short-term tax consequences.

Controlling for selection through inverse probability weighting also permits estimating the treatment effect of recession on taxpayer responsiveness. Holding the composition of the labor force constant, how much more likely are people to report an income in the dominated region during the recession? In the illustrative example of comparing responses to the Health Levy in 2006 and 2010, I find a treatment effect of about 25%. Specifically, I estimate an Average Treatment Effect (ATE) of 4.09% on a base of 16.23%. The interpretation is that after controlling for attrition from

[32] The fraction of employees who separate from the firm in that year.

[33] I include the 'fifty-two weeks' variable, which changes sign, as indication that this is not uniformly the case.

the labor force, 12% of people would be responsive to this notch during a recession versus 16% pre-recession. The ATE of 4.09% is estimated on a full suite of explanatory variables; using only information on industrial sector to control for attrition changes this number of 4.69%, a non-trivial but not particularly substantial amount. In all specifications, approximately 25% of the change in bunching behavior cannot be explained by changes in characteristics. The remainder is due to unobservables and changes in the magnitude of the marginal effects of observables.

One explanation is that it is harder to find opportunities to avoid taxes during recessions, for example by switching jobs or working a different number of hours. Owners and managers may be less receptive to worker requests when labor is in abundant supply, or if they suspect the firm is not viable in the long run. If workers' outside options are lowered during a recession, they may be less likely raise second-order considerations with their employer. Alternatively, social norms may constrain renegotiation of wages during a time of redundancies. Indeed, it is plausible that a sense of national crisis may enhance people's willingness to contribute to taxation. These are far from the only possible explanations; many other plausible interpretations exist to could help explain the changes in bunching behavior. It is likely that several factors play a role.

1.5 Conclusion

This paper identifies how the response of taxpayers to sharp changes in income tax liabilities ("notches") depends on the phase of the business cycle. Notches induce marginal tax rates in excess of 100%, and thus they provide very strong incentives to adjust reported income. I investigated the response using administrative tax return data from the universe of employees in Ireland. As the decline in output in Ireland during the Great Recession was four times more severe than in the United States, the responses to tax incentives there provide a clear test of behavior over the business cycle. Further, the identifying variation—changes in those notch thresholds—facilitates an identification strategy that was not possible in comparable studies where thresholds remain constant.

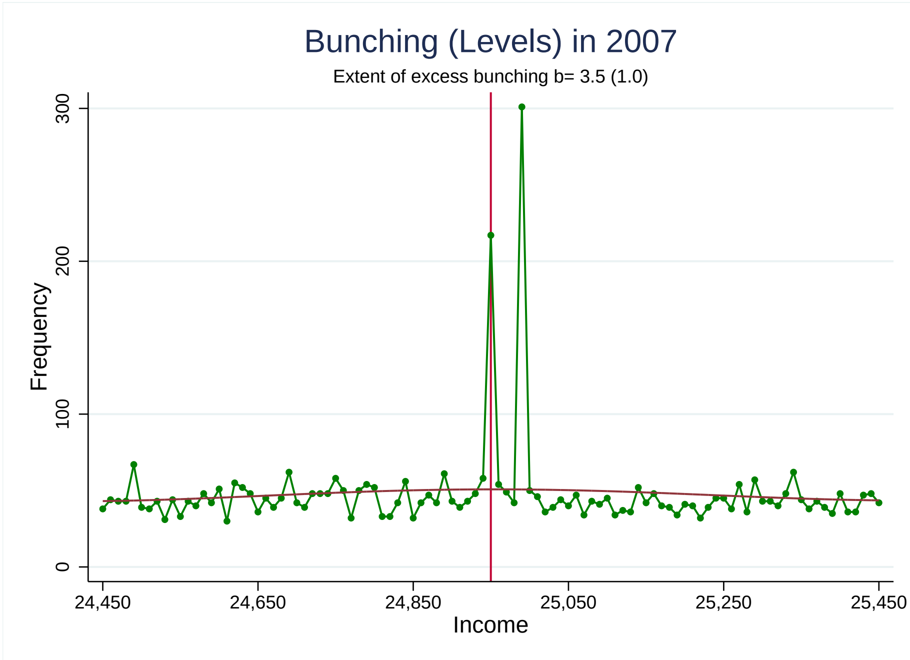
There is unambiguous evidence of tax-advantageous reporting behavior in the pre-recession period. The signs of income manipulation are not present after 2008. Estimating the magnitude of responses to the 'treatment' of a new notch shows that responses are three-to-four times larger at the peak of the cycle than at the trough, despite the treatment intensity being largest at the height of the recession. This points to taxpayer responses being smaller during a recession. My estimates of the elasticity of taxable income are four times larger for 2006 than for 2010.

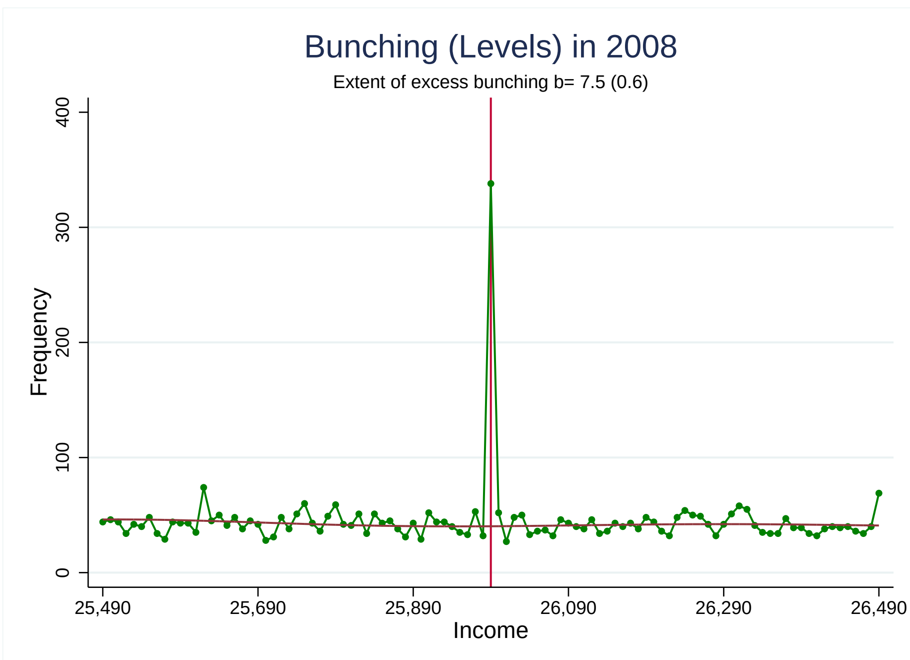
The characteristics of the employer-employee pairing are good predictors of reporting a tax-advantaged income. For example, individuals with self-employment income tend to successfully avoid exceeding notch thresholds, and construction workers report more tax-advantaged incomes than comparable workers in public utilities. By focusing on taxable income, we incorporate all margins (both legal and illegal) that may adjust to taxation.

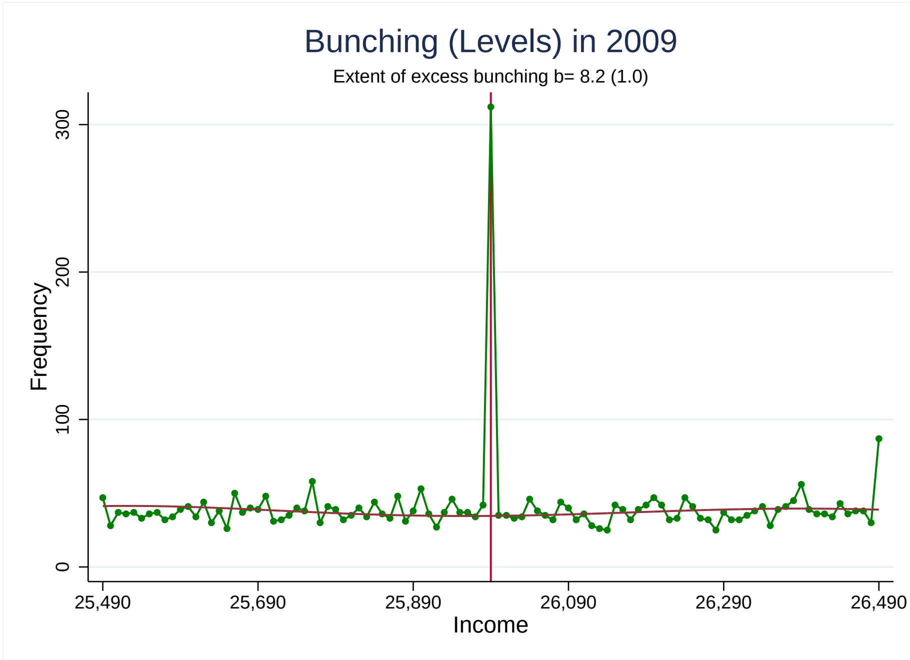
Non-random attrition from the labor force is a sufficient condition for the elasticity of taxable income to vary over the cycle. Counterfactual wage distributions indicate that much of the changes in bunching behavior can indeed be explained by changes in the composition of the labor force: the recession disproportionately reduced employment in sectors which had previously exhibited above-average ability to report tax-advantaged incomes. However the predictive power of counterfactuals is far from complete. Thus the results appear not to be merely a composition effect: using probability weights to control for changes in the labor force, I demonstrate that composition effects are large but the determinants of avoidance behavior themselves change over the cycle. This is consistent with the presence of additional constraints on tax reporting behavior during a recession. The precise source of the additional constraints on behavior can be speculated on. For example, there may be greater restrictions on hours worked or fewer opportunities to switch jobs during a recession; there may be additional concerns about wage negotiations while redundancies are occurring within a firm; or managers in failing firms may be focused on alternative considerations and relatively less attentive to employee tax matters.

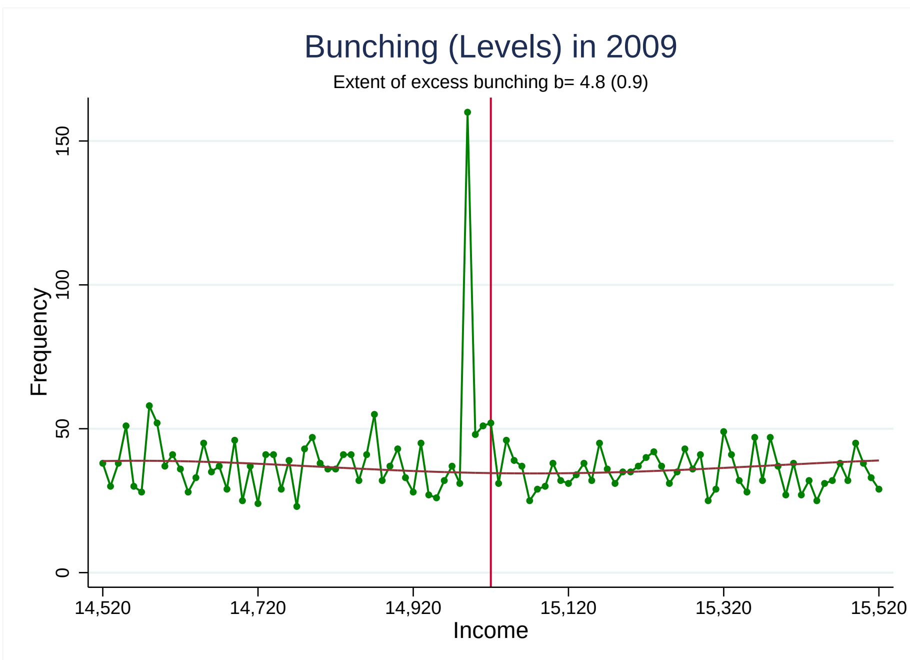
A macroeconomics literature exists about whether the magnitude of the fiscal multiplier varies over the cycle. This paper identifies an analogous effect on the revenue collection side. The evidence from Ireland, which experienced severe economic volatility between 2006 and 2013, is that the response of taxpayers to changes in tax rates is substantially lower during a recession. A cycle-dependent taxpayer response has considerable implications for policy implementation. A decreased taxpayer response generates a lower deadweight loss of taxation, which implies the costs of taxation are lower during a recession; alternatively a decreased ability of taxpayers to avoid taxes suggests less resources need to be allocated to enforcement during a recession.

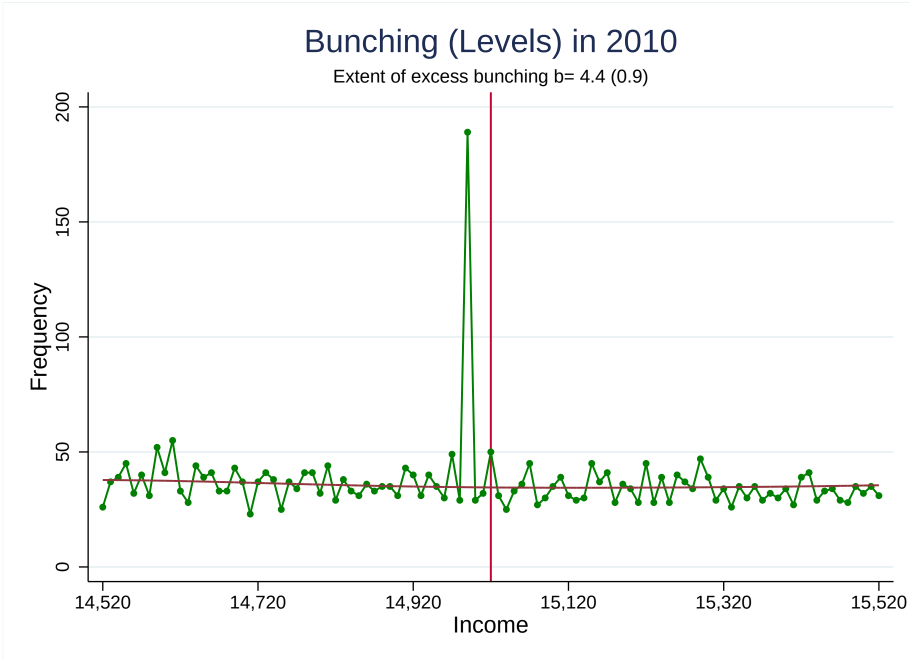
1.6 Bunching (Levels) Figures

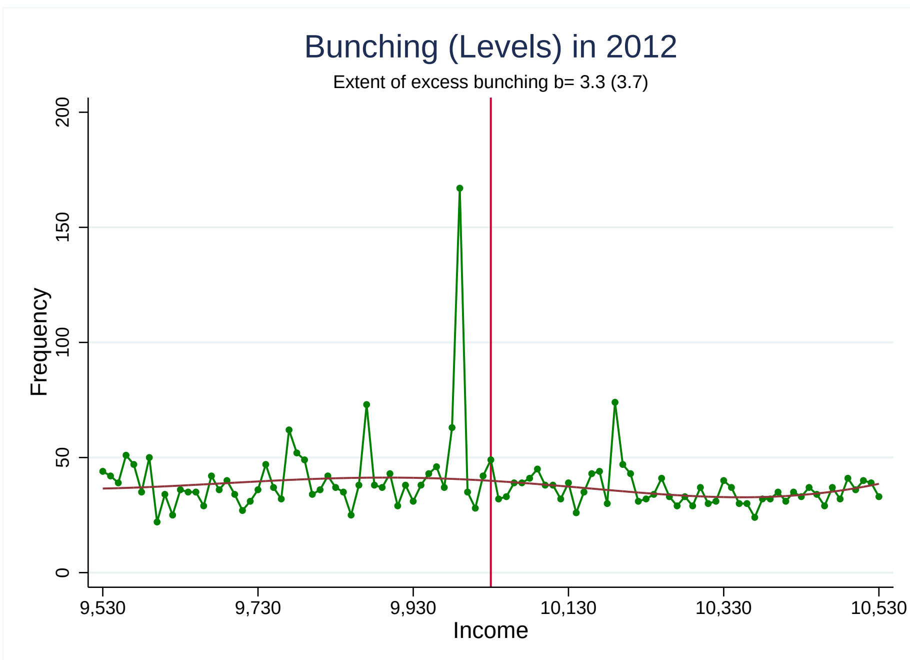

Bunching (Levels) in 2007
Extent of excess bunching b= 3.5 (1.0)
Frequency
0
100
200
300
24,450
24,650
24,850
25,050
25,250
25,450
Income

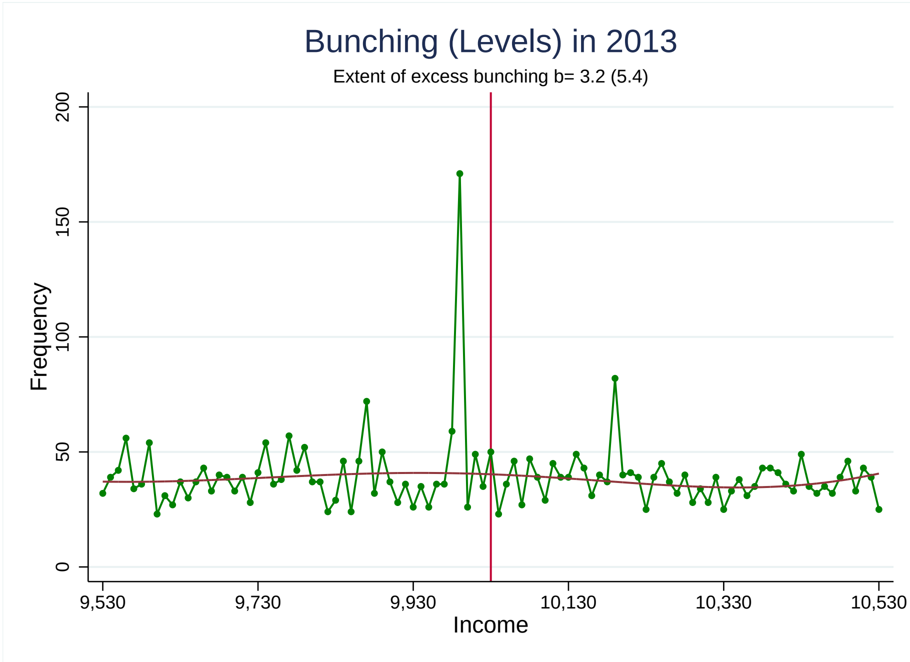

Bunching (Levels) in 2008
Extent of excess bunching b= 7.5 (0.6)
Frequency
0
100
200
300
400
25,490
25,690
25,890
26,090
26,290
26,490
Income


Bunching (Levels) in 2009
Extent of excess bunching b= 8.2 (1.0)
Frequency
0
100
200
300
25,490
25,690
25,890
26,090
26,290
26,490
Income


Bunching (Levels) in 2009
Extent of excess bunching b= 4.8 (0.9)
Frequency
0
50
100
150
14,520
14,720
14,920
15,120
15,320
15,520
Income

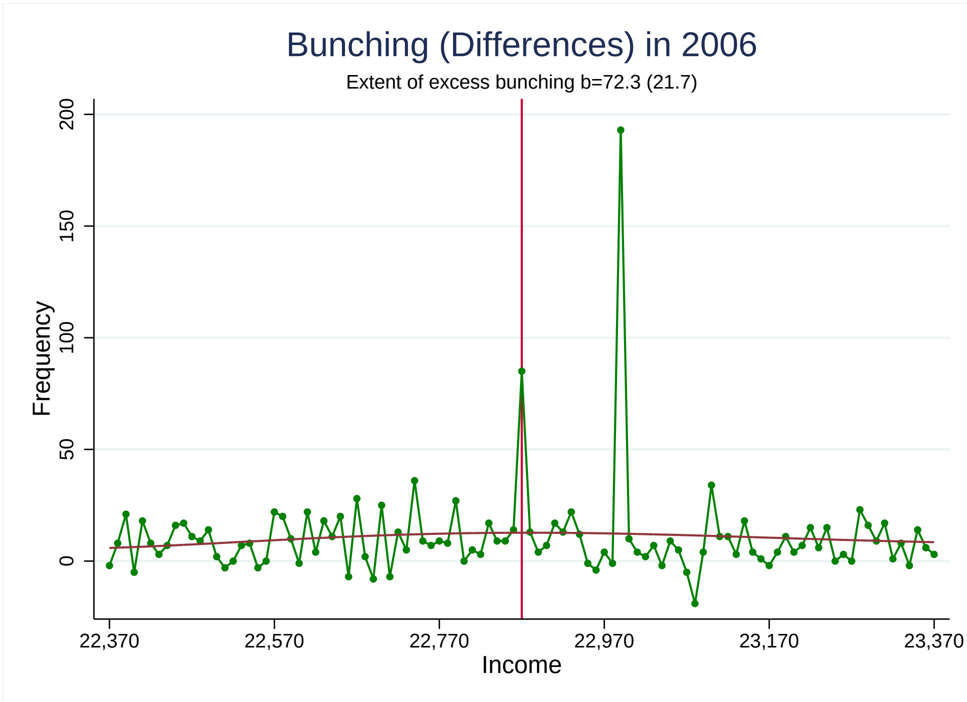

Bunching (Levels) in 2010
Extent of excess bunching b= 4.4 (0.9)
Frequency
0
50
100
150
200
14,520
14,720
14,920
15,120
15,320
15,520
Income

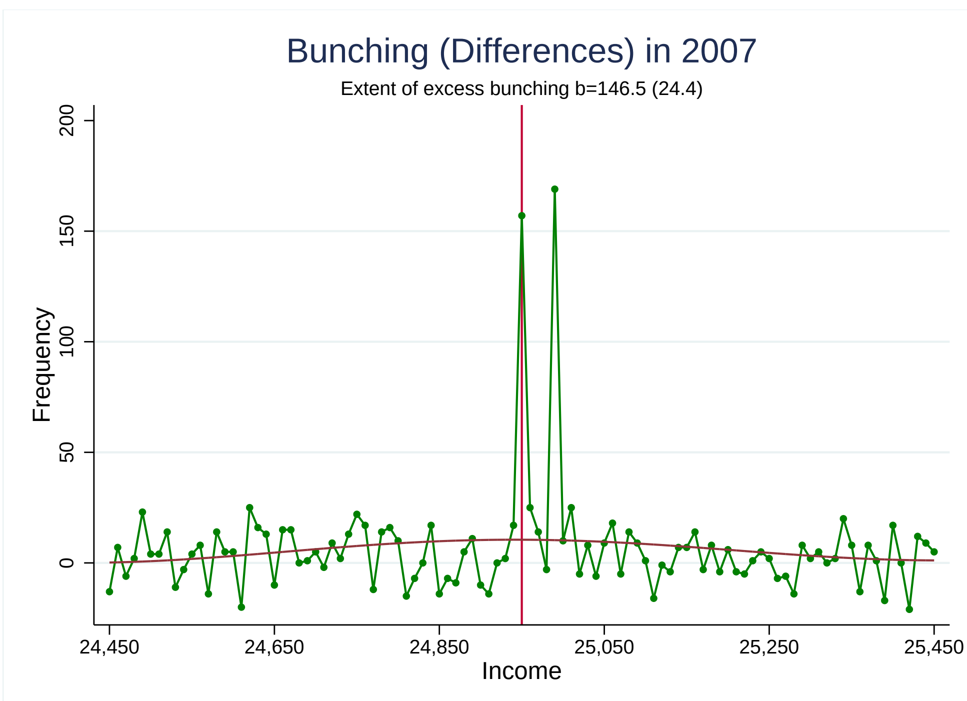

Bunching (Levels) in 2012
Extent of excess bunching b= 3.3 (3.7)
Frequency
0
50
100
150
200
9,530
9,730
9,930
10,130
10,330
10,530
Income

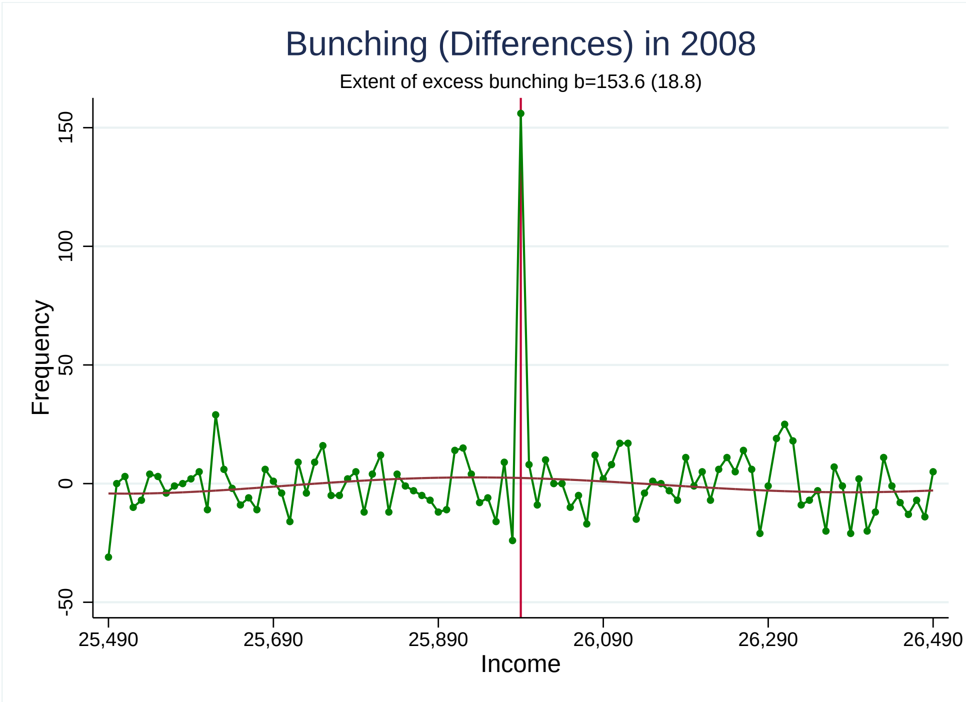


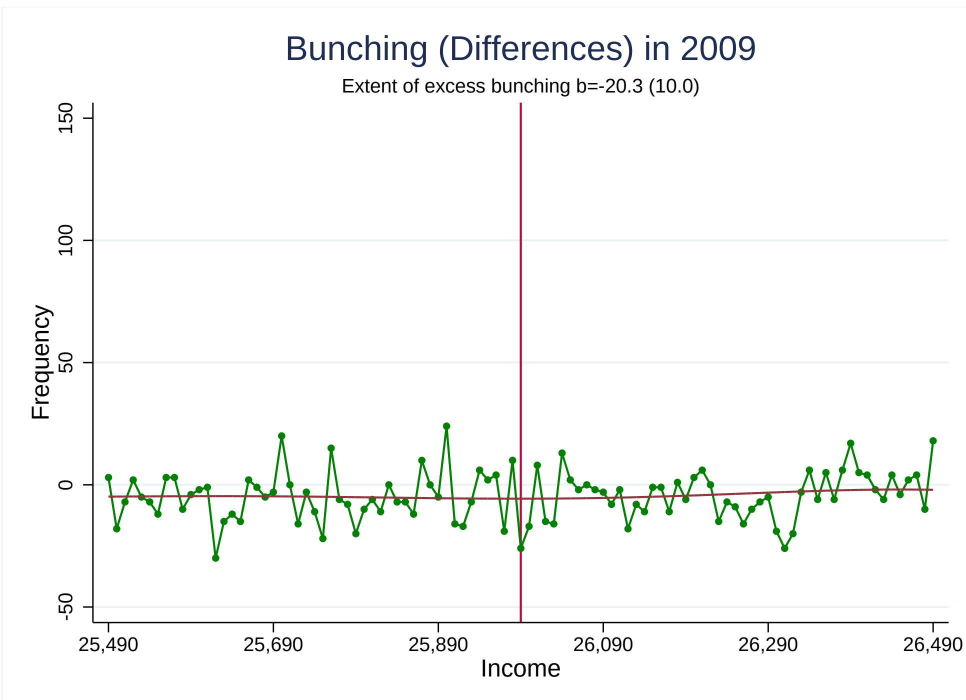
Bunching (Levels) in 2013
Extent of excess bunching b= 3.2 (5.4)
Frequency
0
50
100
150
200
9,530
9,730
9,930
10,130
10,330
10,530
Income

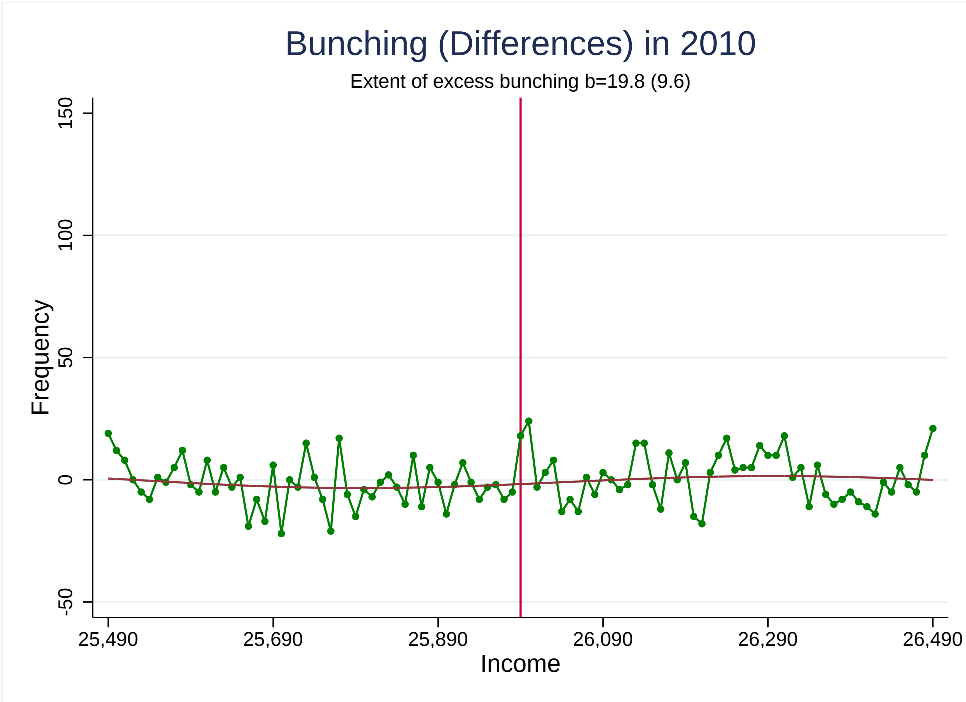
1.7 Bunching (Differences) Figures

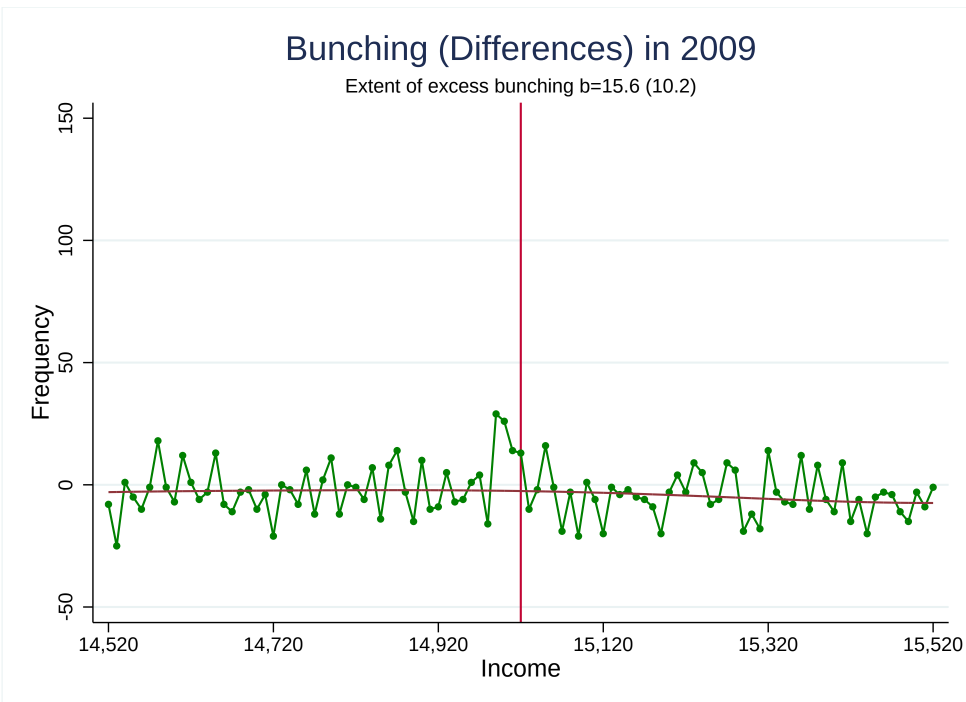


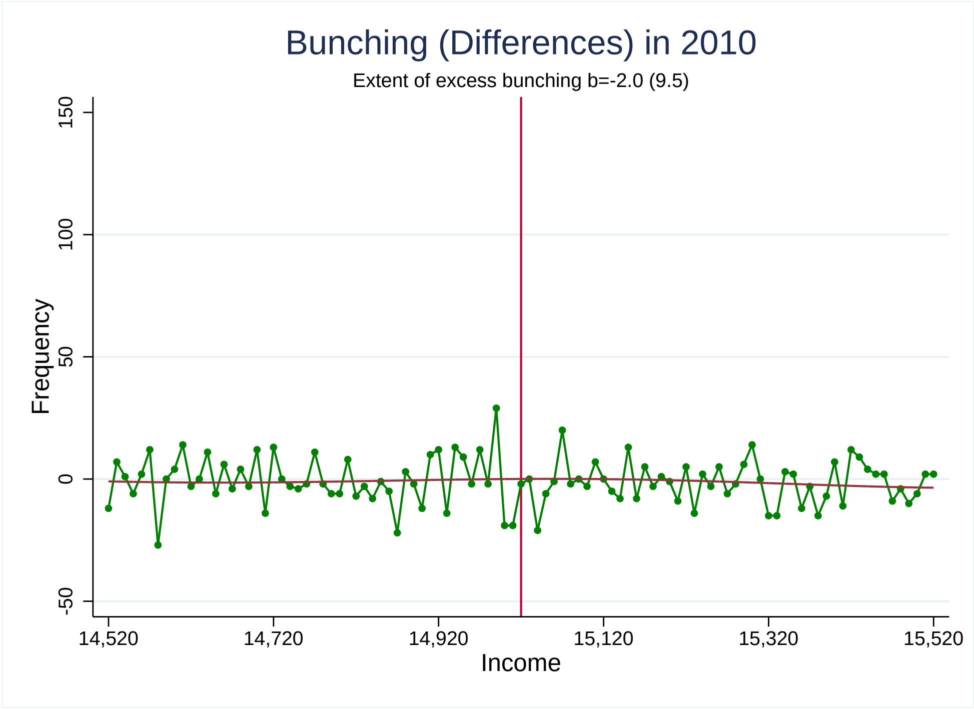


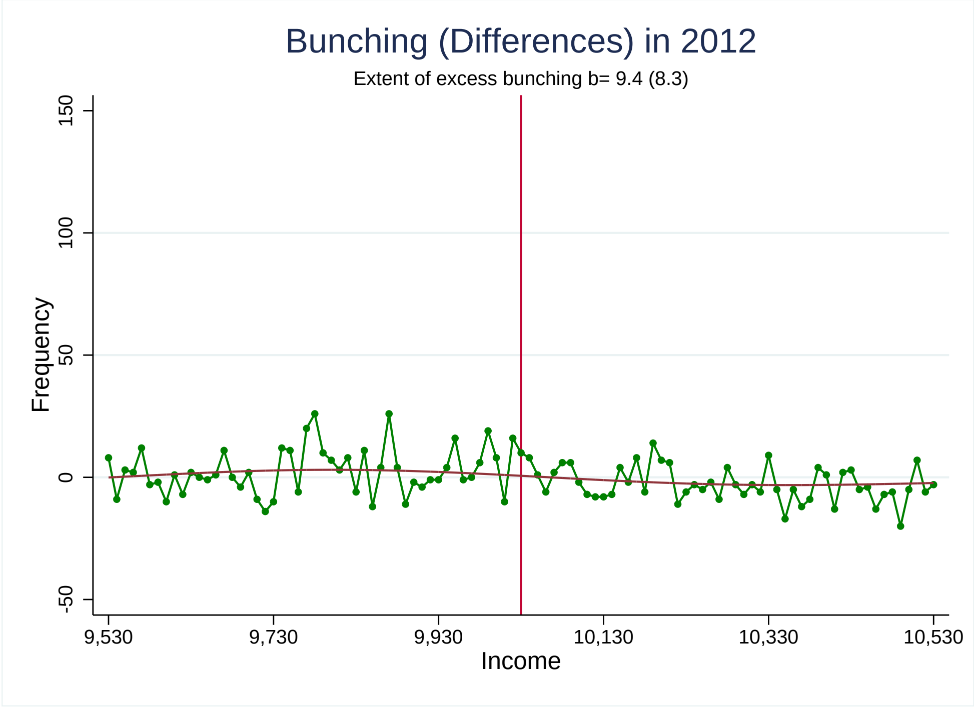

Bunching (Differences) in 2008
Extent of excess bunching b=153.6 (18.8)
Frequency
-50
0
50
100
150
25,490
25,690
25,890
26,090
26,290
26,490
Income

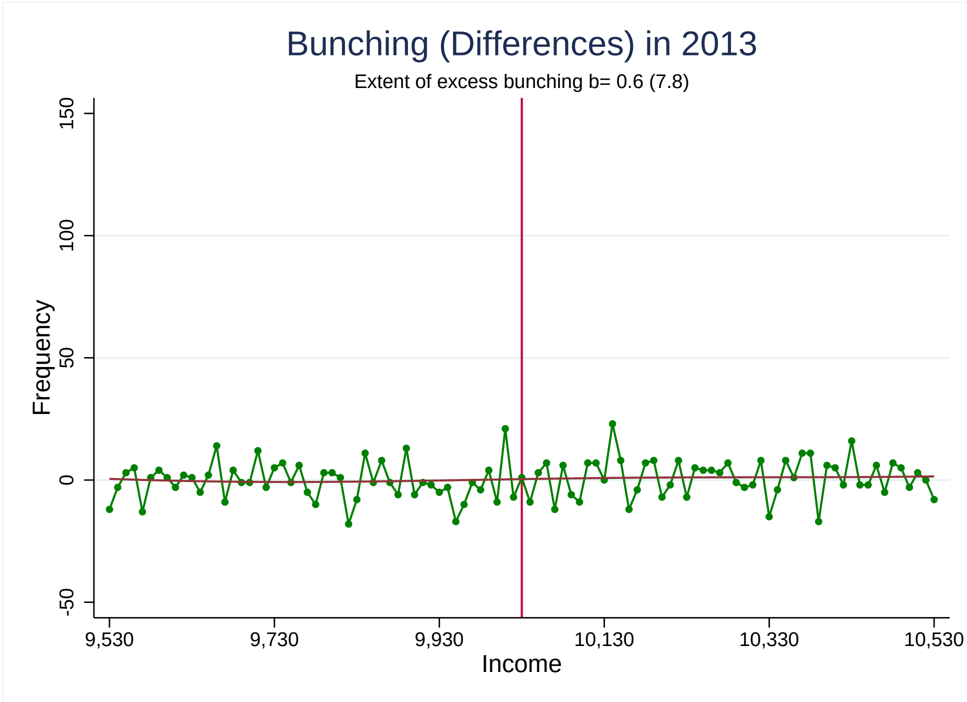

Bunching (Differences) in 2009
Extent of excess bunching b=-20.3 (10.0)
Frequency
-50
0
50
100
150
25,490
25,690
25,890
26,090
26,290
26,490
Income


Bunching (Differences) in 2010
Extent of excess bunching b=19.8 (9.6)
Frequency
150
100
50
0
-50
25,490
25,690
25,890
26,090
26,290
26,490
Income

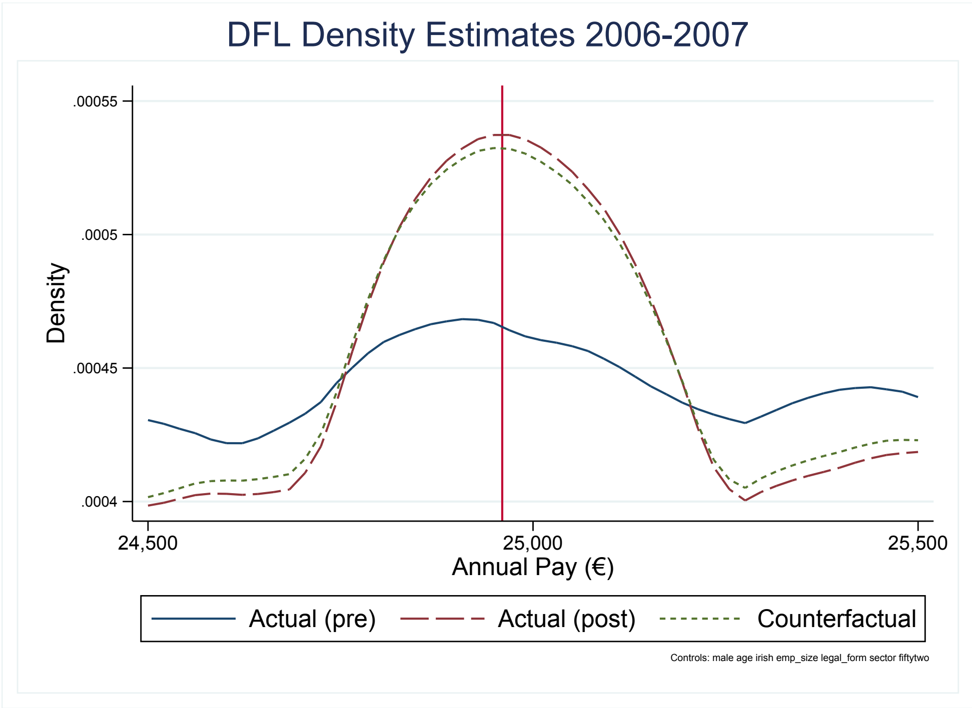

Bunching (Differences) in 2009
Extent of excess bunching b=15.6 (10.2)
Frequency
150
100
50
0
-50
14,520
14,720
14,920
15,120
15,320
15,520
Income

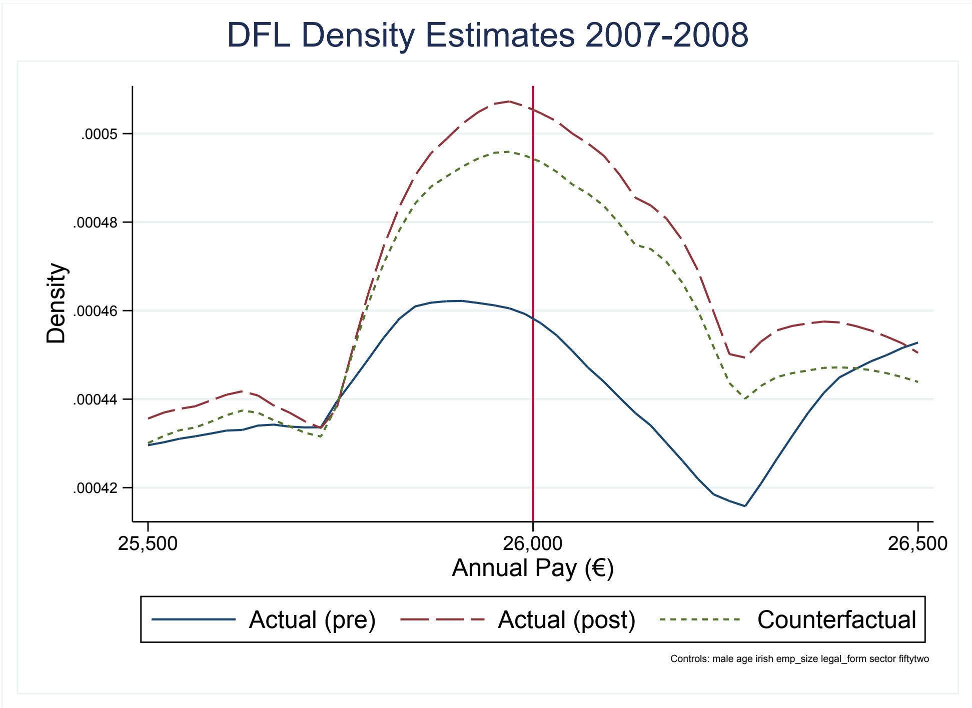

Bunching (Differences) in 2010
Extent of excess bunching b=-2.0 (9.5)
Frequency
150
100
50
0
-50
14,520
14,720
14,920
15,120
15,320
15,520
Income

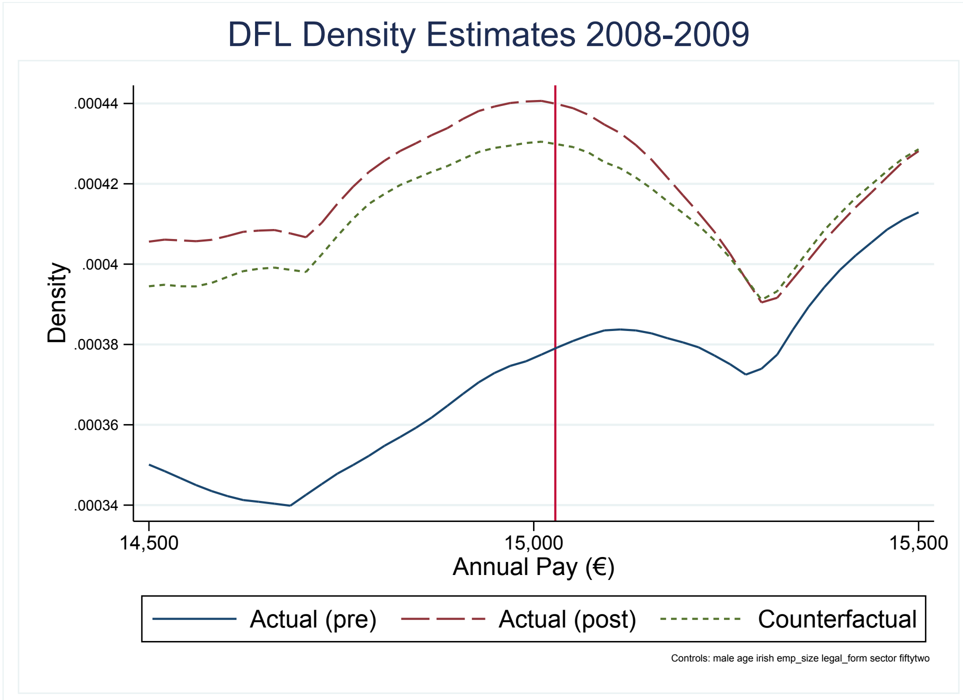

Bunching (Differences) in 2012
Extent of excess bunching b= 9.4 (8.3)
Frequency
150
100
50
0
-50
9,530
9,730
9,930
10,130
10,330
10,530
Income

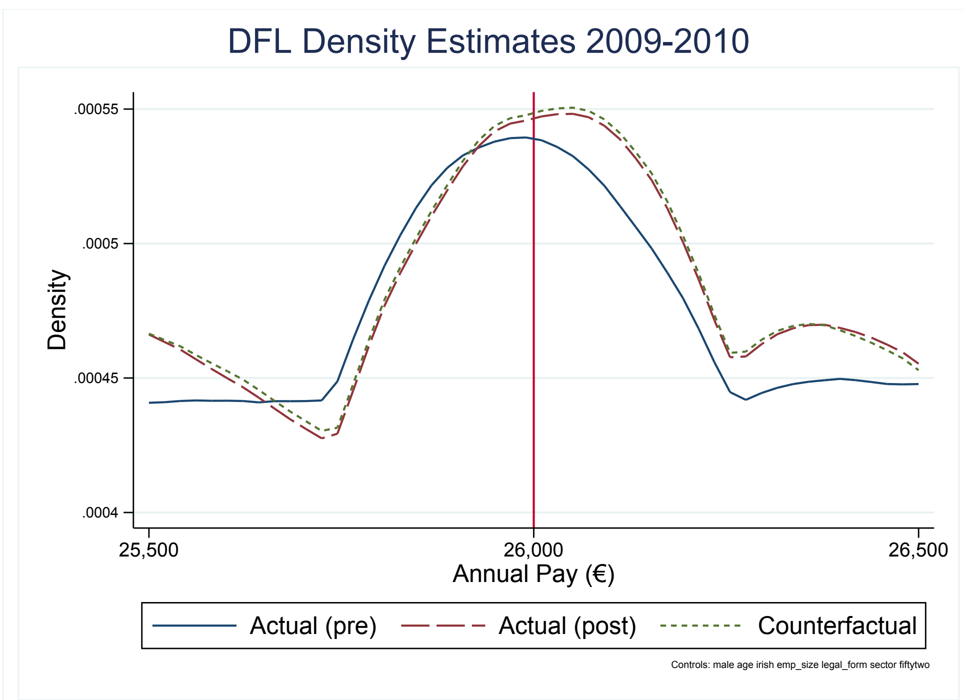

Bunching (Differences) in 2013
Extent of excess bunching b= 0.6 (7.8)
Frequency
150
100
50
0
-50
9,530
9,730
9,930
10,130
10,330
10,530
Income

1.8 DFL Counterfactual Figures






DFL Density Estimates 2008-2009
Density
.00044
.00042
.0004
.00038
.00036
.00034
14,500
15,000
15,500
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo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.0005
.00045
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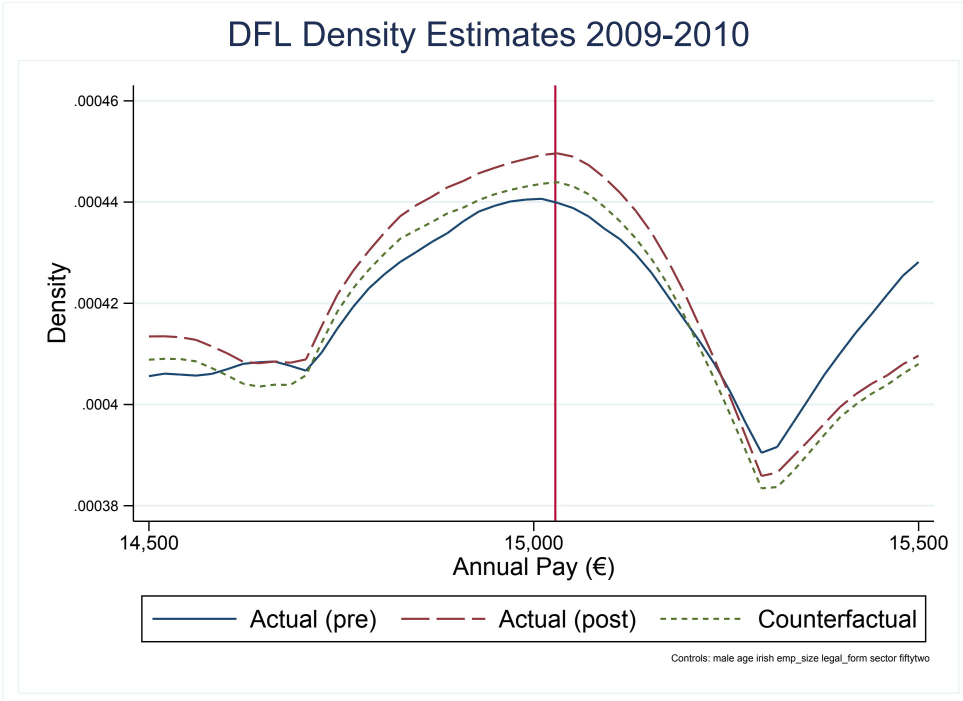
26,500
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo


DFL Density Estimates 2009-2010
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.00042
.0004
.00038
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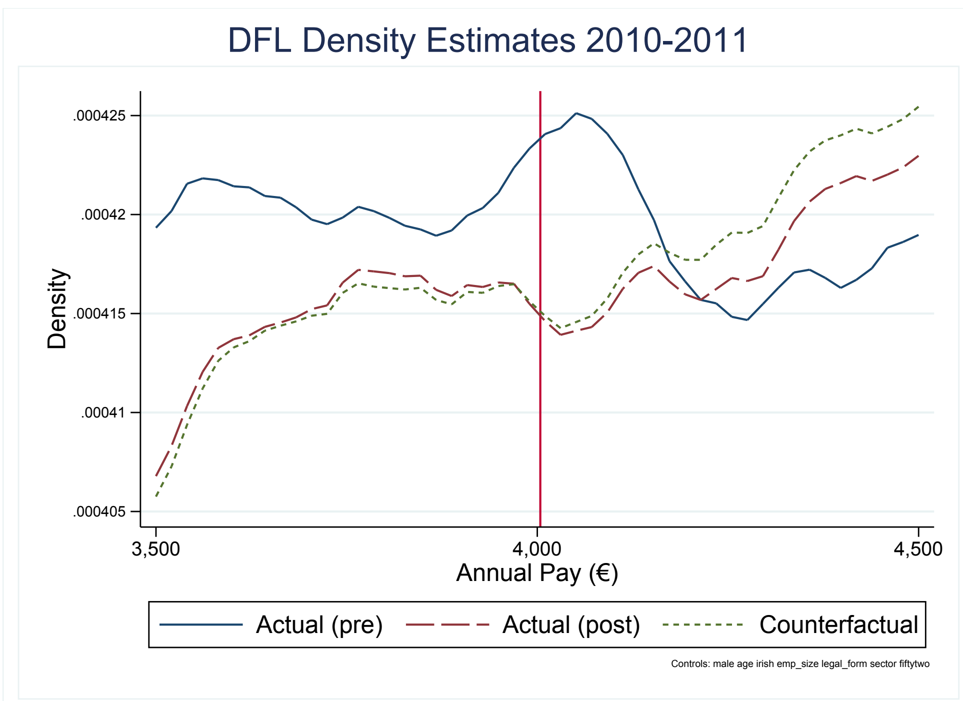
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo


DFL Density Estimates 2010-2011
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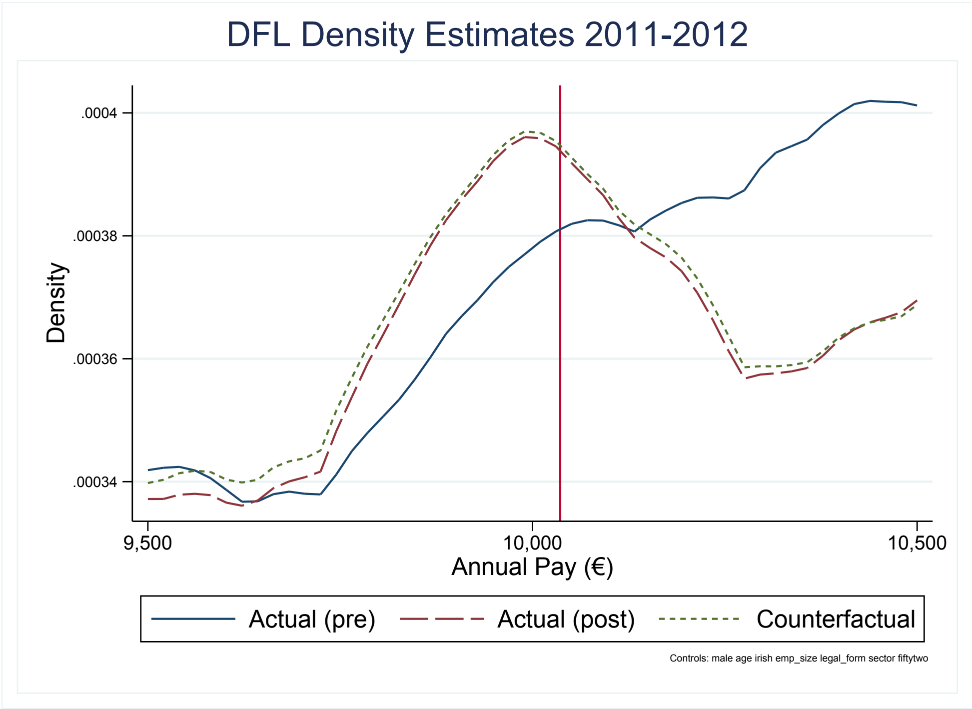
.000415
.00041
.000405
3,500
4,000
4,500
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo



DFL Density Estimates 2011-2012
Density
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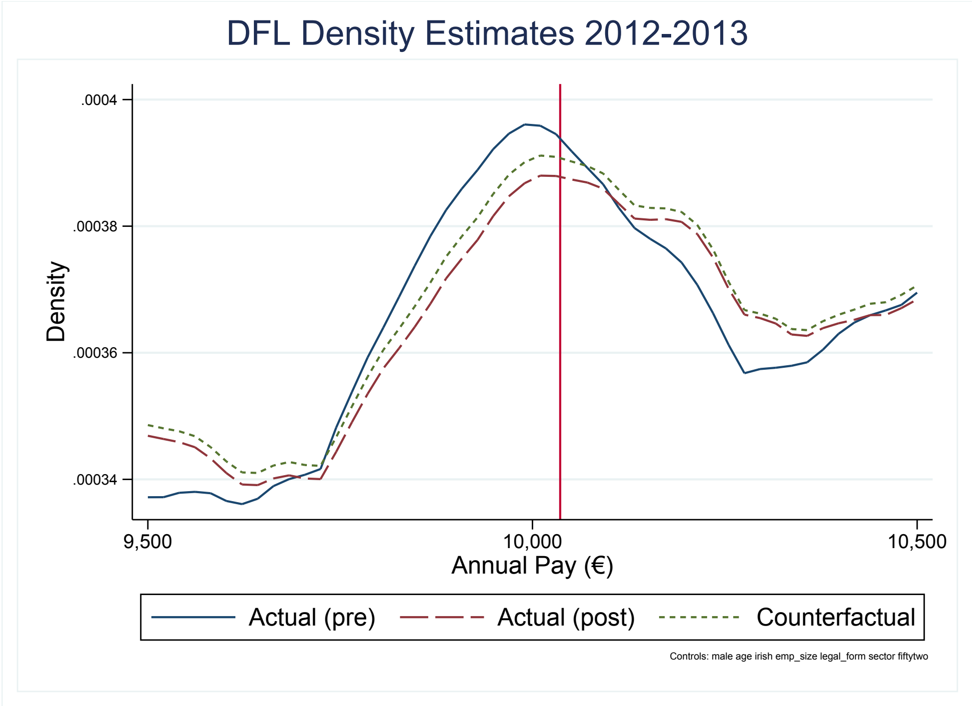
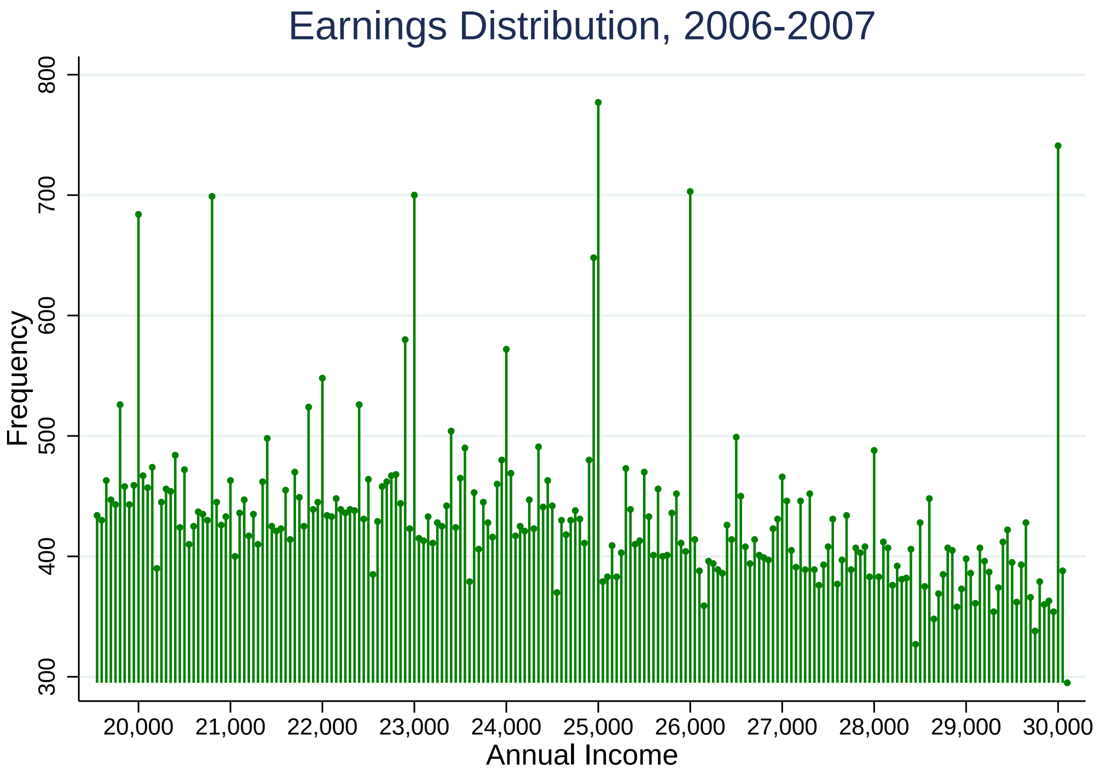
.00038
.00036
.00034
9,500
10,000
10,500
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo



DFL Density Estimates 2012-2013
Density
.0004
.00038
.00036
.00034
9,500
10,000
10,500
Annual Pay (€)
Actual (pre)
Actual (post)
Counterfactual
Controls: male age irish emp_size legal_form sector fiftytwo

1.9 Round-Number Bunching



Histogram of earnings in €50 bins, pooled data from 2006 and 2007. There is clear round-number bunching for multiples of €1,000.

CHAPTER II

Political Fragmentation and Fiscal Policy

2.1 Introduction

The paper investigates how legislative fragmentation affects the size of government. The conventional wisdom (cf. Weingast et al. (1981) Lijphart (1984), Austen-Smith (2000), Milesi-Ferretti et al. (2002)) is that a larger number of political parties leads to an increase in a country's taxes, spending, and transfers.

This paper makes both theoretical and empirical contributions that challenge the conventional wisdom. I model the government's choice of tax rates, public good provision, and level of transfers as a function of n coalition partners. Like most models of government, coalitions choose inefficiently high tax rates to fund transfers to their political allies. The model predicts that an increase in coalition size reduces this inefficiency. In particular, an increase in coalition size lowers taxes, spending, and transfers. All three of these predictions are supported in data from a large panel of OECD countries. I estimate that moving from a two- to three-party legislature lowers tax revenue by 6.7%, expenditure by 9.5%, and transfers by 5.4%

I first replicate the relationship predicted by the conventional wisdom by excluding country fixed effects. Excluding country fixed effects is equivalent to assuming they all have no effect. Including fixed effects and relaxing this assumption completely changes the results. Rather than a positive relationship, I find negative and statistically significant coefficients. These results are robust to a host of potentially important variables such as the ideological composition of government, changes in the tax base, and electoral cycle effects.

In 2008 the share of the United States' total government expenditure over GDP was 37% while the average for EU countries was 47%. Electoral incentives facing governments can help explain this variation. The correspondence between politics and taxes is crucial in determining society's preferred size of government. This paper investigates the effect of increased legislative fragmentation (as measured by the seats-weighted number of parties in parliament) on tax policy, and thus the paper contributes to understanding the effects of secular fragmentation. Additionally, the paper has implications for policies to increase the number of political parties, such as the 2011 electoral reform referendum in the UK.

Research linking political science and tax policy, and the debate about the 'varieties of capitalism' (Hall and Soskice, 2001), is not new. The empirical work of Lijphart (1984, 1999) showed statistically significant relationships between countries' number of political parties and tax policies. Lijphart concluded that fragmentation leads to broad, consensus-based coalitions which cause governments to become "kindler, gentler". Investigating the related question of the link between tax policy and electoral systems, Milesi-Ferretti, Perotti and Rostagno (2002) show that the proportionality of electoral systems increases public spending and transfers, and that these results hold even within the subset of proportional representative (PR) systems. Kontopoulos and Perotti (1999) were firm enough in the expectation that fragmentation increased expenditure to perform a one-sided test on the coefficient of interest. More recent research has questioned the robust of these findings (Martin and Vanberg, 2013; Bäck and Lindvall, 2015; Heller, 2016).

Theoretical models have drawn similar conclusions to the conventional wisdom. The 'Law of $1/n$' (Weingast, Shepsle and Johnsen, 1981) models spending as a function of the number of districts in a country, and finds that over-spending is increasing in the number of districts. The 'veto player' model (Tsebelis, 2002) focuses on the ability of a coalition of $n \geq 1$ 'veto players' to change a policy. The intersection of sets of desirable policies is defined as the 'winset' for this coalition. It is straight-forward to understand the logic that the winset is decreasing in n. One way to 'grease the wheels' is by increasing the payoffs to veto players, which requires higher taxes in equilibrium. Austen-Smith (2000) finds that electoral rules that encourage a greater number parties will head to higher spending and transfers. Therefore these alternative frameworks draw the same conclusion as the conventional wisdom: increasing the number of parties makes agreement more difficult, and thus higher spending (e.g. political pork) results.

The empirical analysis of this paper contradicts this view. Pettersson-Lidbom (2012) finds similar results from two quasi-experiments in Finland and Sweden. This paper is the first to draw these conclusions both theoretically and with a dataset on a large number of developed countries. Although the empirical section may not be as cleanly identified as a small quasi-experimental setting, the inclusion of many countries in the analysis reduces concerns about external validity.

2.2 Theoretical Model

The research question of this paper is how a change in the size of an n-member coalition affects tax policy. More generally, I ask how the collective outcomes for a group are affected by the size of that group. This setting is a form of a common pool problem.

Most public finance common pool models are driven by the disconnect of taxing power and spending power, cf. Persson and Tabellini (2002). In these models, decentralized units choose expenditure levels and the central government raises taxes to meet the liabilities that fall due. Each decentralized unit, knowing that they will only have to pay a fraction of an additional dollar of expenditure, increases spending.

This approach is equivalent to the usual public finance assumption of government requiring an exogenous amount of revenue. Suppose for example that the demand for some goods becomes less elastic, and so consumers are less responsive to taxes. The exogenous revenue assumption rules out the possibility of e.g. governments increasing tax revenue when the excess burden of taxation falls. I do not make this assumption.

I will model the coalition's choice of policy as type of a common pool problem. However the coalition will simultaneously choose income and expenditure policies, and therefore trade off the marginal benefits and marginal costs of taxation. In this framework taxes and expenditure become endogenous.

There are two types of public good, G_1 and G_2. The first are the traditional pure public goods that benefits all of society, e.g. national defence, weather forecasts. The second are "local public goods" which are particularistic and targeted to one large group rather than the general public, e.g. agricultural subsidies, social security transfers. A simpler version of the model with a single type of public good, and where taxes are determined residually, can be found in Chapter 7 of Persson and Tabellini (2002).

A government consists of $i = 1, \ldots, n$ political groups. Politician i in government optimizes group i's welfare. This is the sum of the group's after tax income $(1-\tau)Y_i$, an increasing and strictly concave function f of the pure public good G_1, and an increasing and strictly concave function g of group i's local public good G_2. For convenience I assume that $Y_i = Y \; \forall i$ and that all n parties split G_2 equally. The results hold for more general specifications. Formally, given a national income M, the politician optimizes the following problem:

$$\max_{G_1, G_2, \tau} (1-\tau)Y + f(G_1) + g\left(\frac{G_2}{n}\right) \tag{2.1}$$

$$\text{s.t. } G_1 + G_2 \leq \tau M \tag{2.2}$$

This formulation that departs from much of the previous literature on policy formation. For instance, the Weingast, Shepsle and Johnsen (1981) 'Law of $1/n$' assumes a norm of reciprocity, which "facilitates a process of mutual support and logrolling". This increases inefficient spending in the n number of represented districts. In contrast, I make no such assumption. Rather, this formulation is intuitively very simple: it is a welfare maximization problem. An n-member coalition is formed, this coalition commands a majority, and they agree to maximize the welfare of the groups that comprise the coalition. This contrasts with other models of government (e.g. Austen-Smith and Banks (1988), Milesi-Ferretti and Spolaore (1994)) which do not focus on secular fragmentation. Assumptions about particular norms could of course facilitate alternative equilibrium outcomes. However, no such outcome could Pareto dominate the solution offered by this formulation.

The structure of this problem has been chosen as a compromise between tractability and generality. For example, for simplicity G_2 is defined as a single good, though it could be generalized to a vector of goods $G_2^i, i = 1, \ldots, N$ that are more specifically defined than the current framework permits. Both $f(.)$ and $g(.)$ can be any increasing and concave function. The quasi-linear objective function is additively-separable: this formulation greatly simplifies the analysis as it eliminates cross-partial derivatives, though I suspect relatively innocuous conditions on these derivatives would result in the same substantive conclusion as this model.

It is important to note that I framed the maximization problem as the individual choice of each of the n politicians. Aggregating the decision up to the group level is equivalent to multiplying Equation (2.1) by n and not altering the constraint in (2.2). This would be a monotonic transformation of the original problem and would not affect the solution. The problem can thus be interpreted either at the level of the individual politician, or the coalition as a whole. Both interpretations are valid.

The planner's problem is not a simple rescaling of Equation (2.1). The planner wants to maximize welfare for all $N > n$ groups in society. As all N groups' welfare are affected by tax rates and global public goods, the planner's problem multiplies the first two components (but not the third) of the maximand by N. The third component, the distribution of G_2 only affects the n groups. Consequently the planner's problem is:

$$\max_{G_1, G_2, \tau} N(1-\tau)Y + Nf(G_1) + ng\left(\frac{G_2}{n}\right) \tag{2.3}$$

$$\text{s.t. } G_1 + G_2 \leq \tau M \tag{2.4}$$

The solution to this problem leads to the efficient allocation, and our first result.

Proposition 1. *Under the efficient allocation,* $g'\left(\frac{G_2}{n}\right) = Nf'(G_1)$.

Proof. Forming a Lagrangian with multiplier λ and taking the first order necessary condition with respect to τ, we find that $\lambda = \frac{NY}{M}$. The FOCs on G_1 and G_2 show that $f'(G_1) = \frac{\lambda}{N} = \frac{Y}{M}$ and $g'\left(\frac{G_2}{n}\right) = \lambda = \frac{NY}{M}$. Thus $g'\left(\frac{G_2}{n}\right) = Nf'(G_1)$. □

The planner chooses spending so that the marginal utility of G_1 is N times larger than the marginal utility of G_2. In contrast to the optimal allocation, the solution to the government's problem of Equations (2.1) and (2.2) sees over-provision of G_2. This result is formalized in Propositions 2 and 3.

Proposition 2. *In equilibrium, the coalition chooses spending such that* $g'\left(\dfrac{G_2}{n}\right) = nf'(G_1)$.

Proof. Forming a Lagrangian with multiplier λ and taking the first order necessary condition with respect to τ, we find that $\lambda = \frac{Y}{M}$. The FOCs on G_1 and G_2 show that $f'(G_1) = \lambda = \frac{Y}{M}$ and $g'\left(\frac{G_2}{n}\right) = \frac{\lambda}{n} = \frac{nY}{M}$. Thus $g'\left(\frac{G_2}{n}\right) = nf'(G_1)$. □

Proposition 3. *The coalition over-provides* G_2 *in equilibrium.*

Proof. In both Proposition 1 and Proposition 2, $f'(G_1) = \frac{Y}{M}$. Therefore the provision of G_1 is unchanged (and optimal) in both scenarios. However $g'\left(\frac{G_2}{n}\right)$ changes from $Nf'(G_1)$ in Proposition 1 to $nf'(G_1)$ in Proposition 2. As $n < N$, the marginal utility from G_2 is higher under the optimal allocation than when provided by the coalition. This implies that G_2 is lower in the optimal allocation. Therefore the coalition over-provides G_2. □

Proposition 4. *In equilibrium, the coalition sets a tax rate* $\tau > \tau^{\star}$.

Proof. From Proposition 3 we see G_1 is identical in both scenarios but that the coalition over-provides G_2. Constraint (2.2) implies the government may not run a deficit, and local non-satiation ensures this becomes a balanced budget rule. With spending above that implied by the planner's solution and a balanced budget rule, the coalition sets tax rates above the optimal level. □

These results are reasonably standard. A government of n-coalition partners over-provides local public goods targeted at its constituents at the cost of excessively high taxation. These results are not the focus of this paper. Rather this paper asks how these results are affected by political fragmentation. I model fragmentation as an increase in n. Does fracturing of the coalition make the over-provision of G_2 worse?

The answer is not immediately clear. With more groups, it is quite intuitive that demands for group-specific projects grow larger. Conversely, one could argue that a splintered coalition is forced to shift resources to projects of common agreement. This could be more desirable than dividing G_2 among an ever-larger number of claimants.

In this model, the latter effect is what dominates. Fragmentation lowers taxes and reduces the over-provision of targeted transfers. These results are somewhat counter-intuitive and challenge the conventional wisdom that more fragmented political systems have larger public sectors. The results are formalized in Propositions 5 and 6.

To prove these results it is convenient to reformulate the coalition's problem. Recall that the coalition chooses $\tau, G_1,$ and G_2. Assuming the budget constraint holds with equality, the choice of any two of these variables will determine the third. Therefore I define α as the fraction of tax revenue devoted to G_2, i.e. $G_2 = \alpha\tau M$. Consequently the proportion of government revenue allocated to G_1 is the complementary fraction $(1-\alpha)$. By reformulating the coalition's problem this way, we can condense it into a single maximand in two variables (α and τ) and three parameters (Y, M, and n):

$$\max_{\alpha,\tau} \Pi = (1-\tau)Y + f\left((1-\alpha)\tau M\right) + g\left(\frac{\alpha\tau M}{n}\right)$$

The government chooses tax rate τ. Fraction $1-\alpha$ of the total revenue τM is spent on G_1. Fraction α is spent on G_2, the politically-targeted spending. I refer to α as "transfer intensity".

Differentiating this expression with respect to τ and α leads to the following first-order conditions:

$$\frac{\partial \Pi}{\partial \tau} : -Y + (1-\alpha)Mf'\left((1-\alpha)\tau M\right) + \frac{\alpha M}{n} g'\left(\frac{\alpha \tau M}{n}\right) = 0 \tag{F1}$$

$$\frac{\partial \Pi}{\partial \alpha} : -\tau M f'\left((1-\alpha)\tau M\right) + \frac{\tau M}{n} g'\left(\frac{\alpha \tau M}{n}\right) = 0 \tag{F2}$$

and the following set of second-order and cross-partial derivatives:

$$\frac{\partial F1}{\partial \tau} = \left((1-\alpha)M\right)^2 f''\left((1-\alpha)\tau M\right) + \left(\frac{\alpha M}{n}\right)^2 g''\left(\frac{\alpha \tau M}{n}\right) < 0 \tag{2.5}$$

$$\begin{aligned}\frac{\partial F1}{\partial \alpha} =& - M\left[f'\left((1-\alpha)\tau M\right) + (1-\alpha)\tau M f''\left((1-\alpha)\tau M\right)\right] \\ &+ \frac{M}{n}\left[g'\left(\frac{\alpha \tau M}{n}\right) + \frac{\alpha \tau M}{n} g''\left(\frac{\alpha \tau M}{n}\right)\right]\end{aligned} \tag{2.6}$$

$$\frac{\partial F2}{\partial \tau} = \frac{\partial F1}{\partial \alpha}$$

$$\begin{aligned}\frac{\partial F2}{\partial \alpha} =& (\tau M)^2 f''\left((1-\alpha)\tau M\right) + \left(\frac{\tau M}{n}\right)^2 g''\left(\frac{\alpha \tau M}{n}\right) < 0 \\ =& (\tau M)^2\left[f''\left((1-\alpha)\tau M\right) + \left(\frac{1}{n^2}\right) g''\left(\frac{\alpha \tau M}{n}\right)\right] < 0\end{aligned} \tag{2.7}$$

The comparative statics addressing how policy responds to fragmentation will also require differentiating the first-order conditions (F1) and (F2) with respect to n:

$$\frac{\partial F1}{\partial n} = -\left(\frac{\alpha M}{n^2}\right)\left(g'\left(\frac{\alpha \tau M}{n}\right) + \frac{\alpha \tau M}{n} g''\left(\frac{\alpha \tau M}{n}\right)\right) \tag{2.8}$$

$$\frac{\partial F2}{\partial n} = -\left(\frac{\tau M}{n^2}\right)\left(g'\left(\frac{\alpha \tau M}{n}\right) + \frac{\alpha \tau M}{n} g''\left(\frac{\alpha \tau M}{n}\right)\right) \tag{2.9}$$

It is instructive at this point to note an assumption of the model. Suppose for now that α were pre-determined and the government's only choice variable were τ. We can see how n affects taxes by computing the sign of $\frac{\partial^2 \Pi}{\partial \tau \partial n}$, i.e. computing the sign of Equation (2.8). Clearly $-\frac{\alpha M}{n^2}$ is negative. As g is concave, its first derivative is positive and its second is negative. The sign of the overall derivative thus depends on the sign of $g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n} g''\left(\frac{\alpha\tau M}{n}\right)$.

There is some ambiguity on this condition. My results require that the sign here is strictly positive. This implies that $-x\frac{g''(x)}{g'(x)} < 1$, which is mathematically equivalent to the coefficient of relative risk aversion being less than one. Note that this requirement, that $g'(x) + xg''(x) > 0$, is true for a broad class of concave functions, such as $g(x) = x^\beta$ where $\beta \in (0, 1)$.

Intuitively, an increase in fragmentation has two effects: each party now receives a smaller share of the pie (which will increase the marginal utility of targeted public goods), but also that targeted public goods have become more expensive (as each additional dollar is now split among a larger group). The sign of $g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n} g''\left(\frac{\alpha\tau M}{n}\right)$ determines which effect dominates. If positive, then the latter effect dominates.

To proceed, I assume that $g'(x) + xg''(x) > 0$. Under this assumption we may conclude that Equation (2.8) is negative: an increase in n will lower taxes. Similar conclusions can be drawn about Equation (2.9).

Comparative statics are more complex for the multivariate optimization problem. This requires us to account for cross-partial effects of τ on α, etc. Firstly, given that both of the first-order conditions (F1) and (F2) will equal zero in equilibrium, we can use the Chain Rule to note that:

$$\begin{bmatrix} \dfrac{\partial F1}{\partial \tau} & \dfrac{\partial F1}{\partial \alpha} \\ \dfrac{\partial F2}{\partial \tau} & \dfrac{\partial F2}{\partial \alpha} \end{bmatrix} \begin{bmatrix} \dfrac{\partial \tau}{\partial n} \\ \dfrac{\partial \alpha}{\partial n} \end{bmatrix} = - \begin{bmatrix} \dfrac{\partial F1}{\partial n} \\ \dfrac{\partial F2}{\partial n} \end{bmatrix} \tag{2.10}$$

With these derivatives, we have a system of equations that implicitly define how our variables of interest are affected by n:

$$\begin{bmatrix} \dfrac{\partial \tau}{\partial n} \\ \\ \dfrac{\partial \alpha}{\partial n} \end{bmatrix} = - \begin{bmatrix} \dfrac{\partial F1}{\partial \tau} & \dfrac{\partial F1}{\partial \alpha} \\ \\ \dfrac{\partial F2}{\partial \tau} & \dfrac{\partial F2}{\partial \alpha} \end{bmatrix}^{-1} \begin{bmatrix} \dfrac{\partial F1}{\partial n} \\ \\ \dfrac{\partial F2}{\partial n} \end{bmatrix} \tag{2.11}$$

We can apply Cramer's Rule to Equation (2.11) to derive our comparative statics results. By signing $\frac{\partial \tau}{\partial n}$ and $\frac{\partial \alpha}{\partial n}$, we see how taxes and transfer intensity respond to an increase in n. The first key comparative static is $\frac{\partial \tau}{\partial n}$, how the tax rate responds to a change in fragmentation. A positive coefficient would indicate that taxes go up when the number of parties increases.

Proposition 5. *Fragmentation leads to lower taxes.*

Proof. Applying Cramer's Rule to (2.11):

$$\frac{\partial \tau}{\partial n} = - \frac{\dfrac{\partial F1}{\partial n}\dfrac{\partial F2}{\partial \alpha} - \dfrac{\partial F1}{\partial \alpha}\dfrac{\partial F2}{\partial n}}{\dfrac{\partial F1}{\partial \tau}\dfrac{\partial F2}{\partial \alpha} - \dfrac{\partial F1}{\partial \alpha}\dfrac{\partial F2}{\partial \tau}} \tag{2.12}$$

The numerator of this comparative static is $\left(\dfrac{\partial F1}{\partial n}\dfrac{\partial F2}{\partial \alpha}\right) - \left(\dfrac{\partial F1}{\partial \alpha}\dfrac{\partial F2}{\partial n}\right)$. Focusing for now on the first two terms:

$$\frac{\partial F1}{\partial n} = - \left(\frac{\alpha M}{n^2}\right) \left[g'\left(\frac{\alpha \tau M}{n}\right) + \frac{\alpha \tau M}{n} g''\left(\frac{\alpha \tau M}{n}\right)\right] \tag{2.13}$$

$$\frac{\partial F2}{\partial \alpha} = (\tau M)^2 \left[f''((1-\alpha)\tau M) + \left(\frac{1}{n}\right)^2 g''\left(\frac{\alpha \tau M}{n}\right)\right] \tag{2.14}$$

Omitting the arguments of functions for clarity, we conclude that their product equals

$$\frac{\partial F1}{\partial n}\frac{\partial F2}{\partial \alpha} = -\left(\frac{\alpha\tau^2M^3}{n^2}\right)\left[f''(.) + \left(\frac{1}{n}\right)^2 g''(.)\right]\left[g'(.) + \frac{\alpha\tau M}{n}g''(.)\right] \tag{2.15}$$

Calculating the other terms in the numerator is simplified by substitution from Equation (F2)

$$\frac{\partial F1}{\partial \alpha} = (-\tau M^2)\left[(1-\alpha)f''((1-\alpha)\tau M) - \alpha\left(\frac{1}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.16}$$

$$\frac{\partial F2}{\partial n} = -\left(\frac{\tau M}{n^2}\right)\left[g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n}g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.17}$$

Again omitting arguments for clarity, their product equals

$$\frac{\partial F1}{\partial \alpha}\frac{\partial F2}{\partial n} = \left(\frac{\tau^2M^3}{n^2}\right)\left[(1-\alpha)f''(.) - \alpha\left(\frac{1}{n}\right)^2 g''(.)\right]\left[g'(.) + \frac{\alpha\tau M}{n}g''(.)\right] \tag{2.18}$$

The numerator of $\frac{\partial \tau}{\partial n}$ is thus equal to Equation (2.15) minus Equation (2.18). Calculating this, we conclude that the numerator is:

$$\frac{\partial F1}{\partial n}\frac{\partial F2}{\partial \alpha} - \frac{\partial F1}{\partial \alpha}\frac{\partial F2}{\partial n} = \underbrace{\left(-\frac{\tau^2M^3}{n^2}\right)}_{<0}\underbrace{\left[f''(.)\right]}_{<0}\underbrace{\left[g'(.) + \frac{\alpha\tau M}{n}g''(.)\right]}_{>0} \tag{2.19}$$

The denominator of this comparative static is also the difference of two products. In terms of the first product, we know that

$$\frac{\partial F1}{\partial \tau} = (M)^2\left[(1-\alpha)^2 f''((1-\alpha)\tau M) + \left(\frac{\alpha}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.20}$$

$$\frac{\partial F2}{\partial \alpha} = (\tau M)^2\left[f''((1-\alpha)\tau M) + \left(\frac{1}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.21}$$

We conclude that

$$\frac{\partial F1}{\partial \tau}\frac{\partial F2}{\partial \alpha} = (\tau^2 M^4)\left\{(1-\alpha)^2\left[f''(.)\right]^2 + \left(\tfrac{1}{n}\right)^2\left(\alpha^2+(1-\alpha)^2\right)\left[f''(.)\right]\left[g''(.)\right] + \alpha^2\left(\tfrac{1}{n}\right)^4\left[g''(.)\right]^2\right\} \tag{2.22}$$

In terms of the second product in the denominator,

$$\frac{\partial F1}{\partial \alpha} = (-\tau M^2)\left\{(1-\alpha)f''\left((1-\alpha)\tau M\right) - \left(\frac{1}{n}\right)^2 \alpha g''\left(\frac{\alpha\tau M}{n}\right)\right\} \tag{2.23}$$

Further, because $\dfrac{\partial F2}{\partial \tau} = \dfrac{\partial F1}{\partial \alpha}$ it follows that $\dfrac{\partial F1}{\partial \alpha}\dfrac{\partial F2}{\partial \tau} = \left(\dfrac{\partial F1}{\partial \alpha}\right)^2$. Therefore

$$\frac{\partial F1}{\partial \alpha}\frac{\partial F2}{\partial \tau} = (\tau^2 M^4)\left\{(1-\alpha)^2\left[f''(.)\right]^2 - 2\alpha(1-\alpha)\left(\tfrac{1}{n}\right)^2\left[f''(.)\right]\left[g''(.)\right] + \left(\tfrac{1}{n}\right)^4\alpha^2\left[g''(.)\right]^2\right\} \tag{2.24}$$

The denominator is equal to Equation (2.22) less Equation (2.24). This equals:

$$\frac{\partial F2}{\partial \alpha}\frac{\partial F1}{\partial \tau} - \frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial \alpha} = \underbrace{\left(\frac{\tau^2 M^4}{n^2}\right)}_{>0}\underbrace{\left[f''\left((1-\alpha)\tau M\right)\right]}_{<0}\underbrace{\left[g''\left(\frac{\alpha\tau M}{n}\right)\right]}_{<0} \tag{2.25}$$

When including the numerator from Equation (2.19) and the denominator from Equation (2.25), we get the following result:

$$\frac{\partial \tau}{\partial n} = -\frac{\left(-\dfrac{\tau^2 M^3}{n^2}\right)\left[f''\left((1-\alpha)\tau M\right)\right]\left[g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n}g''\left(\frac{\alpha\tau M}{n}\right)\right]}{\left(\dfrac{\tau^2 M^4}{n^2}\right)\left[f''\left((1-\alpha)\tau M\right)\right]\left[g''\left(\frac{\alpha\tau M}{n}\right)\right]} \tag{2.26}$$

or, simplified:

$$\frac{\partial \tau}{\partial n} = \frac{\underbrace{\left[g'\left(\frac{\alpha \tau M}{n}\right) + \frac{\alpha \tau M}{n} g''\left(\frac{\alpha \tau M}{n}\right)\right]}_{>0}}{\underbrace{M}_{>0} \underbrace{\left[g''\left(\frac{\alpha \tau M}{n}\right)\right]}_{<0}} \tag{2.27}$$

$$\therefore \frac{\partial \tau}{\partial n} < 0$$

□

We conclude that fragmentation lowers tax rates.

Corollary 1. *Fragmentation leads to lower expenditure.*

Proof. Equation (2.27) shows that tax rates fall as fragmentation increases. By budget balancing, this implies that expenditure also falls. □

Our second key comparative static is $\frac{\partial \alpha}{\partial n}$, how the proportion of resources for the targeted local public good respond to a change in fragmentation. The sign of the comparative static indicates whether more or less 'pork' occurs with more parties. Proposition 6 shows that the relationship is negative.

Proposition 6. *Fragmentation leads to lower transfer intensity.*

Proof. We can calculate the sign of this derivative by applying Cramer's Rule to Equation (2.11):

$$\frac{\partial \alpha}{\partial n} = -\frac{\frac{\partial F2}{\partial n}\frac{\partial F1}{\partial \tau} - \frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial n}}{\frac{\partial F2}{\partial \alpha}\frac{\partial F1}{\partial \tau} - \frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial \alpha}} \tag{2.28}$$

Note that the denominator here is equal to the denominator in Proposition 5. The numerator of this comparative static is $\left(\frac{\partial F2}{\partial n}\frac{\partial F1}{\partial \tau}\right) - \left(\frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial n}\right)$. For the first two terms, we know from Equation (2.8) that

$$\frac{\partial F2}{\partial n} = -\left(\frac{\tau M}{n^2}\right)\left(g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n} g''\left(\frac{\alpha\tau M}{n}\right)\right) \tag{2.29}$$

and from Equation (2.5) that

$$\begin{aligned}\frac{\partial F1}{\partial \tau} &= ((1-\alpha)M)^2 f''((1-\alpha)\tau M) + \left(\frac{\alpha M}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right) \\ &= (M)^2\left[(1-\alpha)^2 f''((1-\alpha)\tau M) + \left(\frac{\alpha}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right)\right] \end{aligned} \tag{2.30}$$

Therefore the product $\frac{\partial F2}{\partial n}\frac{\partial F1}{\partial \tau}$ equals

$$-\left(\tfrac{\tau M^3}{n^2}\right)\left[(1-\alpha)^2 f''((1-\alpha)\tau M) + \left(\tfrac{\alpha}{n}\right)^2 g''\left(\tfrac{\alpha\tau M}{n}\right)\right]\left[g'\left(\tfrac{\alpha\tau M}{n}\right) + \tfrac{\alpha\tau M}{n} g''\left(\tfrac{\alpha\tau M}{n}\right)\right] \tag{2.31}$$

Now the latter two terms in the numerator. Knowing that $\frac{\partial F2}{\partial \tau} = \frac{\partial F1}{\partial \alpha}$, Equation (2.23) tells us that

$$\frac{\partial F2}{\partial \tau} = (-\tau M^2)\left[(1-\alpha) f''((1-\alpha)\tau M) - \alpha\left(\frac{1}{n}\right)^2 g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.32}$$

Finally,

$$\frac{\partial F1}{\partial n} = \left(-\frac{\alpha M}{n^2}\right)\left[g'\left(\frac{\alpha\tau M}{n}\right) + \frac{\alpha\tau M}{n} g''\left(\frac{\alpha\tau M}{n}\right)\right] \tag{2.33}$$

Combining these two together, and temporarily omitting arguments of functions for clarity, we get

$$\frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial n} = \left(\tfrac{\tau M^3}{n^2}\right)(\alpha)\left[(1-\alpha)f''(.) - \alpha\left(\tfrac{1}{n}\right)^2 g''(.)\right]\left[g'(.) + \tfrac{\alpha\tau M}{n}g''(.)\right] \tag{2.34}$$

Formulating the full numerator as the difference between Equation (2.31) and Equation (2.34)

$$\frac{\partial F2}{\partial n}\frac{\partial F1}{\partial \tau} - \frac{\partial F2}{\partial \tau}\frac{\partial F1}{\partial n} = \left(-\tfrac{\tau M^3}{n^2}\right)\left[(1-\alpha)^2 f''(.) - \left(\tfrac{\alpha}{n}\right)^2 g''(.) + \alpha(1-\alpha)f''(.) + \left(\tfrac{\alpha}{n}\right)^2 g''(.)\right] \tag{2.35}$$

$$= \underbrace{\left(-\frac{\tau M^3}{n^2}\right)}_{<0}\left[\underbrace{(1-\alpha)f''(.)}_{<0}\right] \tag{2.36}$$

Recall that the denominator here is equal to the denominator in Proposition 5, i.e. the denominator is Equation (2.25). Combining these we find that

$$\frac{\partial \alpha}{\partial n} = -\frac{\left(-\dfrac{\tau M^3}{n^2}\right)\left[(1-\alpha)f''((1-\alpha)\tau M)\right]}{\left(\dfrac{\tau^2 M^4}{n^2}\right)\left[f''((1-\alpha)\tau M)\right]\left[g''\left(\tfrac{\alpha\tau M}{n}\right)\right]} \tag{2.37}$$

which simplifies to

$$\frac{\partial \alpha}{\partial n} = \frac{\underbrace{(1-\alpha)}_{>0}}{\underbrace{M}_{>0}\underbrace{\left[g''\left(\dfrac{\alpha\tau M}{n}\right)\right]}_{<0}} \tag{2.38}$$

$$\therefore \frac{\partial \alpha}{\partial n} < 0$$

□

Corollary 2. *Fragmentation lowers the level of transfers.*

Proof. By Equation (2.38), the fraction of revenue going to transfers falls when fragmentation increases. By Corollary 1, the levels of expenditure falls. Combined, these imply that the level of transfers falls. □

2.2.1 Summary of Implications

The model presented is an n-member common pool problem. The coalition simultaneously choose the tax rate τ, and the fraction α of tax revenue directed to local public goods/transfers. Politically motivated goods are over-produced, and the government chooses tax rates that are inefficiently high. In this regard, the model matches the existing literature.

The focus of the model is how these results depend on the level of fragmentation, labeled n. The analysis predicts that taxes fall as n increases. This also implies that spending falls when n increases. The comparative statics also predicted that transfers fall when n increases. These are the main predictions of the model, and they do not coincide with the conventional wisdom. I test these predictions in Section 2.3.2.

The theory provides a stronger testable implication than those listed above. It is clear that both transfers and spending should decrease as n increases. However, α is defined as transfers as a fraction of government revenue, not just transfers as a fraction of GDP. I refer to α as "transfer intensity". The model predicts that α, transfer intensity, should fall as n increases.

A further implication of the model is nonlinearity in the marginal effects. As both τ and α are fractions bounded by $[0, 1]$ we do not expect a constant effect of a change in n. In particular, a marginal change in n at low levels (e.g. from two to three parties) is expected to have a larger effect than a change at high levels (e.g. from six to seven parties). I test this prediction in Section 2.3.11.

2.3 Empirical Analysis

2.3.1 Data Description

The predictions of Section 2 can be tested empirically. The data (Armingeon et al., 2012a) are from the Institute of Political Science at the University of Bern. This includes measures of political competition as well as primary macroeconomic variables such as government revenue and social security transfers for 23 countries[1] from 1975–2010.[2] We see that nations with more parties have larger government sectors.

[1] Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, UK, and USA.

[2] The data extend back to 1960 but are less reliable pre-1975. For example, in some specifications I include national debt as a control variable. Prior to 1975, more than half (60%) of values for debt are missing, whereas 8% are missing for post-1975. For legislative fragmentation, 11% of the values are missing for the period before 1975, and 0.1% are missing for the period after.

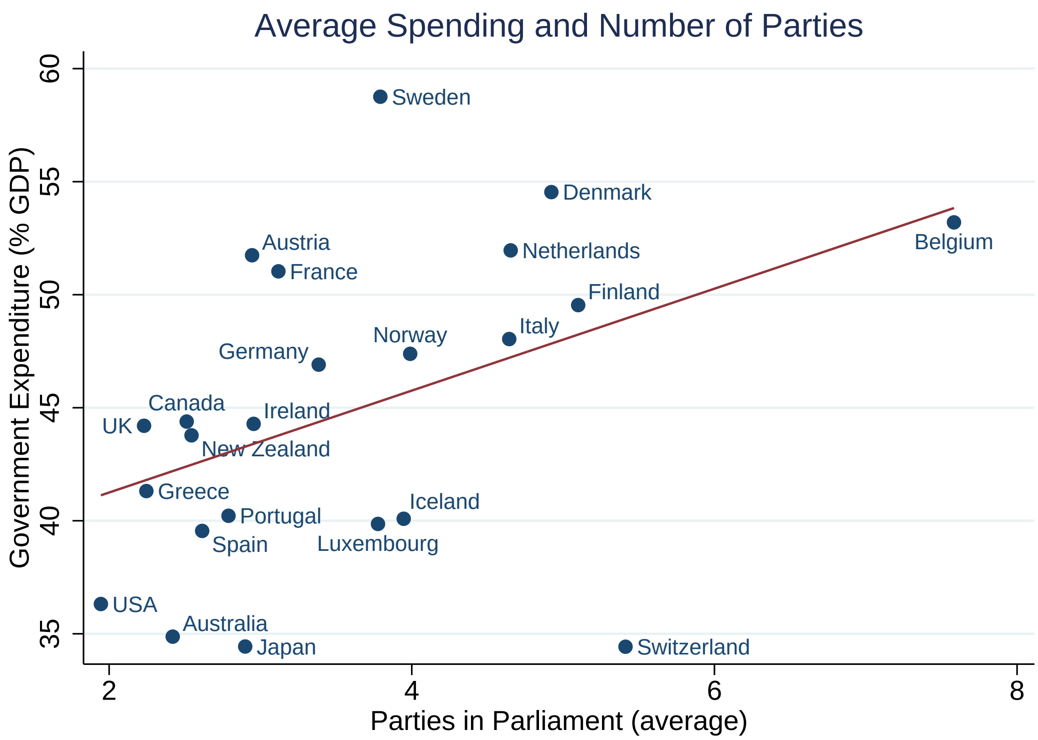


Figure 2.1: The conventional wisdom on spending

Measuring the number of political parties in a country is a non-trivial exercise. Although there are over 400 parties registered in the United Kingdom, three dominate parliament. Similarly, the United States is considered a two-party system, despite the existence of Libertarians, Greens, etc. To accounts for this, Lijphart (1984) uses the 'effective number of parties', taking weighted averages of parties' importance in elections and parliament. The computation is comparable to the Herfindahl concentration index. In a legislature with m parties, and where s_i denotes the vote share for party i,

$$\text{Effective number of parties in parliament } = \left(\sum_{i=1}^{m} s_i^2 \right)^{-1}$$

Over the period 1975–2010 there is a reasonable amount of variation in the measure of legislative fragmentation. For example, Germany has a mean value of 3.38 and a standard deviation of 0.49, while Italy's mean and standard deviation are 4.64 and 1.31 respectively. The median value for all countries is 3.2 with a standard deviation of 1.4. The US has particularly low values: the mean is 1.95 with a standard deviation of 0.06.

Table 2.1: Summary statistics for 23 countries, 1975–2010

	Mean	Std. Dev	N	Min	Max
Gov't receipts (% GDP)	42.56	8.40	785	24.35	63.20
Gov't expenditure (% GDP)	45.14	8.18	785	26.07	70.54
Gov't transfers (% GDP)	13.68	3.88	820	4.34	23.89
Eff. parties parliament	3.59	1.43	826	1.69	9.07
Transfers (% Gov't receipts)	31.94	7.33	783	11.89	55.41
Unemployment benefits (% Gov't receipts)	3.06	2.15	636	0.00	11.56
Old age benefits (% Gov't receipts)	16.28	6.01	639	4.82	35.42
Active labor market programs (% Gov't receipts)	1.54	0.89	566	0.00	4.72

Recall that the main predictions of the model were that taxes, spending, and transfers fall as the number of coalition partners rise. For the purposes of the empirical analysis, my measures are total tax receipts as a percent of GDP, total outlays of government as a percent of GDP, and social security transfers as a percent of GDP. Summary statistics are provided in Table 1.

In addition, the theory makes a sharper prediction: that transfers as a fraction of government revenue falls when fragmentation increases. This fraction, labeled α, has several empirical analogs. The data permit testing this prediction with four variants of economic transfers: all social security transfers, unemployment benefits, old age benefits, and expenditure on active labor market programs. Unemployment benefits and active labor market programs are clearly expenditure targeted at specific groups more vulnerable to labor market fluctuations; and old age benefits are not pure public goods.

I primarily measure political competition by the effective number of parties in parliament. Therefore the empirical results in Section 2.3.2 measure the impact of legislative fragmentation on tax policy. I find that legislative fragmentation is indeed correlated with tax policy. As we will soon see, I find that its impact is of the opposing sign and is statistically different from the conventional approach.

Legislative fragmentation, of course, is distinct from executive/government fragmentation. For example, fragmentation that is restricted exclusively to opposition parties may not correspond to increased executive fragmentation. Some previous work (cf. Kontopoulos and Perotti (1999)) emphasize the importance of this distinction. Therefore in Section 2.3.4 I will largely repeat the analysis of legislative fragmentation but instead use measures of executive fragmentation.

2.3.2 Legislative Fragmentation

The empirical analysis is based on a country fixed effect model:

$$y_{it} = a_i + \delta_t + \beta x_{it} + \epsilon_{it}$$

where y_{it} is the outcome (e.g. tax receipts as a fraction of GDP) in country i during year t; the a_i variables are country fixed effects; δ_t represents year fixed effects; x_{it} are country-year covariates (such as legislative fragmentation); and ϵ_{it} is the error term. The standard condition for parameter identification,

$$\mathbb{E}\left[\epsilon_{it} | x_{it}, a_i, \delta_t\right] = 0$$

holds when the change in level of fragmentation is exogenous conditional on fixed effects.

The fixed effects model exploits within country variation, rather than between country variation, to derive results. The estimation is thus based on changes in the number of parties within a country. This approach captures all time-invariant, country-specific heterogeneity, and isolates that effect from any (time-invariant) spurious relationships between countries' number of parties and public finances. Estimation with country fixed effects therefore entirely nests many other approaches. For example, the differences due to ethnolinguistic fractionalization as calculated by Alesina et al. (2003), are embedded into country fixed effects.

Identification is not compromised by disgruntled electorates changing party allegiances, e.g. switching from Democrats to Republicans. Identification requires that, conditional on observable characteristics, the level of fragmentation is exogenous. This is a much more reasonable claim. Moreover, cleanly identified evidence of these results have been found by Pettersson-Lidbom (2012). This alleviates concerns that these results are not causal.

Of course any time-varying heterogeneity could also bias the estimator. This is less likely to be a problem with shorter time-horizons and wider cross-sections. For this reason, I repeat the procedure on a wider sample of 35 countries, including those previously behind the Iron Curtain, which is available for the year 1990–2010. These results are in Section 2.3.5.

Table 2 presents the main empirical contribution of the paper. It shows the results, with and without country fixed effects, of regressing tax policy on legislative fragmentation. Standard errors are robust to heteroskedasticity and serial correlation, and are consistent even under cross-sectional dependence (Driscoll and Kraay, 1998). As suggested by Newey and West (1994), I use the standard lag length of

$floor\left[4(T/100)^{2/9}\right]$. This equals 3 for my time horizon. The results are not sensitive to longer lengths. For example, increasing the lag length from 3 to 10 changes the standard error in Column 2 from 0.3187 to 0.3369. This decreases the t-stat from 3.28 to 3.10.

Table 2.2: Decline in taxes, spending, and transfers

	Receipts		Expenditure		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties parliament	2.309***	-1.045***	1.894***	-1.939***	0.615***	-0.697***
	(0.198)	(0.319)	(0.178)	(0.430)	(0.0816)	(0.156)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	783	783	783	783	818	818

Driscoll-Kraay standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Let us first look at the results on tax revenue presented in Columns 1 and 2. Column 1 presents the 'conventional wisdom' estimates, based on between-country regressions. My preferred specification, including country fixed effects, is shown in Column 2.

The first column presents evidence supporting Lijphart (1984)'s conclusion that more parties leads to higher tax receipts. These results are positive and significant at the 1% level. Column 1 suggests that if the UK moved from a three- to a four-party system, the fraction of output collected by the government would increase by about 2.3 percentage points.

Column 2, which has the potential to nest Column 1 but isolates any fundamental country heterogeneity, shows a negative coefficient. This suggests that moving from a three- to four-party system would lower tax revenue by about one percentage point. This result is also highly significant. Crucially, however, it is of different sign. The different approaches in Columns 1 and 2 reaches opposite conclusions. As predicted by the group maximization problem in Section 2, increased legislative fragmentation is associated with lower tax receipts.

Next we look at government expenditure. The columns have the same interpretation as before. Our coefficients again change sign: Column 3 suggests increasing the number of parties by one will increase government expenditure by about 2 percentage points; Column 4 suggests it would decrease expenditure by a similar amount. Again, the results are of opposing sign, and counter to the conventional wisdom. As predicted by the model, we find lower spending with more fragmentation.

What of transfers? The pattern emerges again. The between country estimator finds a positive effect, the within country estimator a negative effect, and the difference is significant. The between estimate suggests an increase in fragmentation leads to a 0.6 percentage point increase in social security transfers as a fraction of GDP. The within estimate suggests the same increase in fragmentation would reduce social security transfers by 0.7 percentage points.

Are these results 'real' or, for example, driven by outliers? One method to check for a robust relationship is to take a nonparametric approach to the data. As suggested by Chetty et al. (2014), we can visualize the conditional expectation functions using binned scatterplots. This approach is comparable to a scatter plot, but takes the y-axis average of the points within equal-sized x-axis bins. Binned scatterplots also allow for the inclusion of control variables, which can change regression slopes and intercepts and shift the relative position of visualized data points. Thus including control variables (such as fixed effects) will change the location of the points on the graph. The binscatters, with and without fixed effects are shown in Figure 2.2.

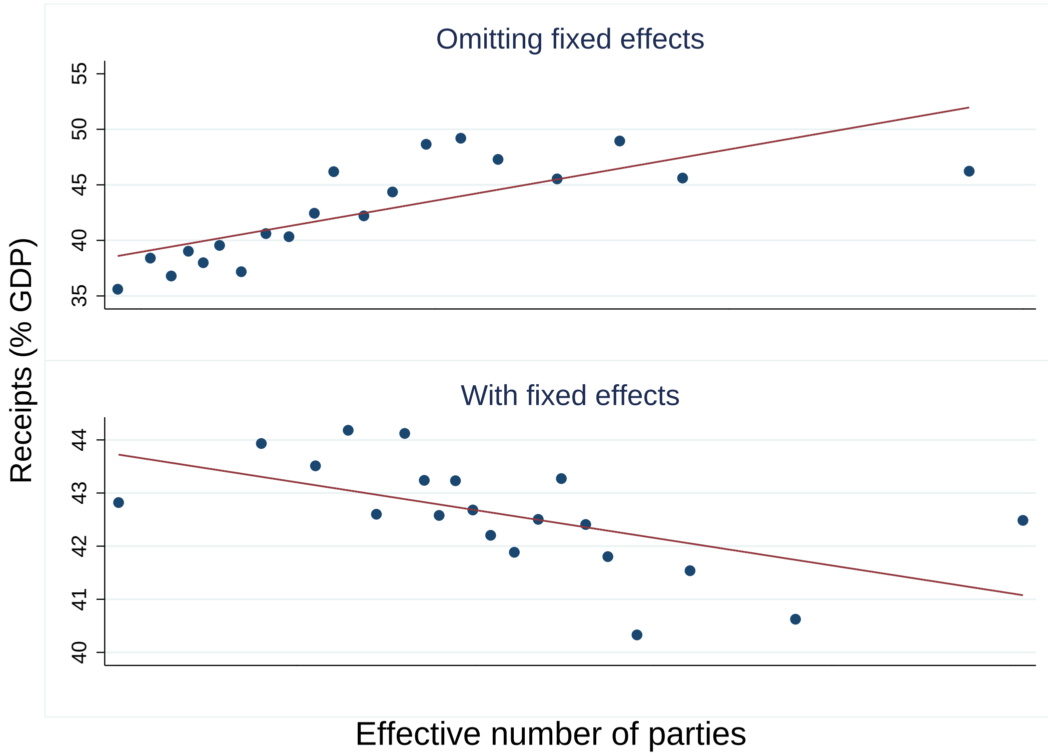


Figure 2.2: Nonparametric binned scatterplots show reversal of relationship

This nonparametric approach provides visual evidence that receipts fall as the number of parties increases. Almost identical plots can be produced for expenditure and transfers.

To see if the results are confounded by serial correlation, Figure 2.3 shows scatter bins in first differences i.e. $\Delta Y_{it} = \beta \Delta X_{it} + \Delta \epsilon_{it}$. Results are no longer significant for expenditure, very close to zero for transfers, but remain significant for receipts.

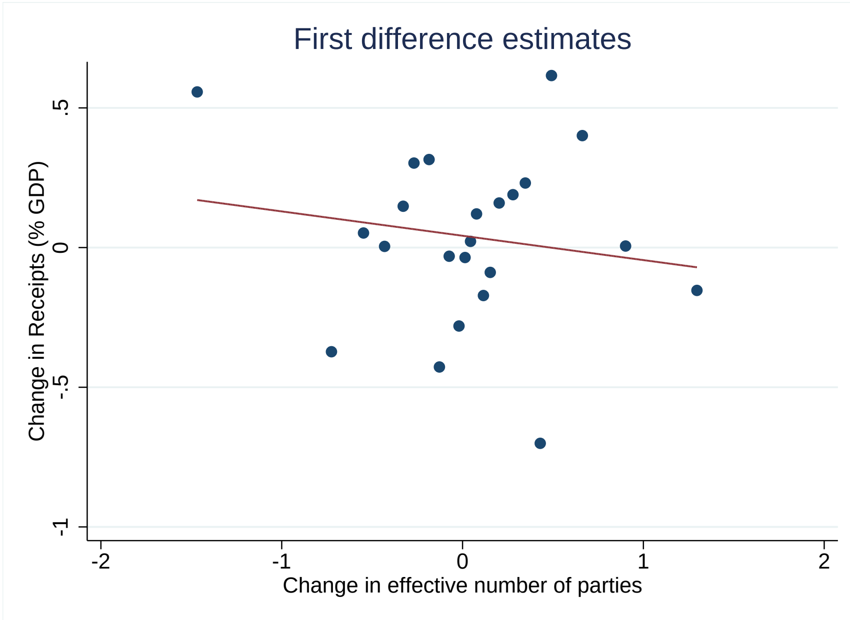


Figure 2.3: Binned scatterplots show negative relationship in first differences.

The model makes a further, stronger prediction. Not just is it expected that transfers fall but that transfers as a fraction of government revenue falls. I call this "transfer intensity". The prediction that transfer intensity falls is tested in Table 3. This time, both between and within estimates suggest a negative sign. Again, the prediction is validated by the fixed effects estimates, and the result is significant at the 1% confidence level.

Table 2.3: Effects on social transfers as % of government revenue

	Transfers Intensity	
	(1)	(2)
Eff. parties parliament	-0.278	-0.892***
	(0.216)	(0.275)
Year FE	Yes	Yes
Country FE	No	Yes
N	781	781

In addition to measuring α with all social security transfers, I confirm that the prediction holds also for sub-components of transfers. In particular, the data permit testing this prediction with unemployment benefits, old age benefits, and expenditure on active labor market programs. The results are in Table 4.

Table 2.4: Effects on Various Social Transfers

	Unemployment		Old Age Benefits		ALMPs	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties parliament	0.484***	-0.223**	0.210	-1.035***	0.208***	-0.138***
	(0.0576)	(0.0841)	(0.208)	(0.229)	(0.0287)	(0.0340)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	636	636	639	639	566	566

Again the results support the theory. Targeted transfers fall significantly when legislatures become more fractured. These holds true not just for social security transfers in total, but for each of the components in Table 4. These hypothesis provide a sharp test of the model from Section 2, and the results are unambiguous. For unemployment insurance, the coefficient implies an increase in the number of parties lowers the fraction of tax revenue directed to the unemployed falls for 0.2%. The corresponding numbers for pensions and labor market participation programs are 1% and 0.1% respectively.

Taken as a whole, the results reject the conventional wisdom. More fragmented parliaments are not associated with higher taxes, spending, and transfers. The opposite is true. As parliaments become more fragmented, the size of the public sector falls. This is true whether we look to total receipts, total expenditure, transfers as a whole, or differing components of transfers.

Are these results cleanly identified? Probably not. In any broad international analysis of public finances over a generation, it is unlikely that we can find clean exclusion restrictions. These results would be more convincing with quasi-experimental evidence.

Such evidence already exists. Pettersson-Lidbom (2012) provides evidence challenging the conventional wisdom. In the context of two natural experiments in Scandinavia, Pettersson-Lidbom (2012) finds than an increase in legislature size lowers the size of government. Given that the results are driven exclusively by two Nordic countries, it is reasonable to question the external validity of those results. This paper shows that the effects are true more generally. The results hold for a broad selection of OECD countries over the past forty years.

This should lead us to reevaluate our model of policy formation. The data support the model of Section 2 which, unlike other models in the literature, places few restrictions on the optimizing behavior of coalition partners.

2.3.3 Why such a different relationship?

The result that the inclusion of fixed effects reverses the sign of the relationship presents a puzzle. Why is there such a different relationship? I outline a model below which presents one possible mechanism.

Let country i be endowed, through nature and/or historical process, with a parameter θ_i. This parameter captures the extent of political disagreement within a country. Specifically, the preferences of citizens are evenly distributed over the interval $[0, \theta_i]$. In words, the larger a country's θ, the wider the spectrum of political views its citizens hold. France, for example, has prominent socialist as well as far-right parties. In contrast, the political landscape in the United States is constrained between center-right and conservative parties. Thus France's political spectrum is wider than the United States', and therefore $\theta_{FR} > \theta_{US}$. Graphically, consider the following country with a relatively small θ. For illustrative purposes, I divide the political space into $n = 2$ evenly sized segments, each representing a political party. Each party represents the views of their segment of the spectrum.

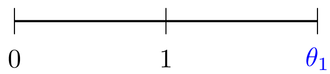


Figure 2.4: A country of type θ_1, with two parties

Consider alternatively a country of type $\theta_2 > \theta_1$. Here the spectrum of political beliefs is larger. Again, as an illustration, I divide the political space into evenly sized segments. However, there are more political parties in this country. This is because political beliefs are more dispersed. This seems intuitively reasonable, as a larger political spectrum leaves more 'room' for alternative parties. This country has seven parties. A government would require the support of at least $\theta_2/2$ of the electorate; for example, a coalition of parties 1 through 4.

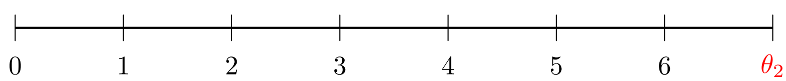


Figure 2.5: A country of type θ_2, with seven parties

Now consider the case where θ is positively correlated with preferences over the tax rate τ. That is, an increase in the spectrum of political views is more likely to facilitate economically liberal agendas than economically conservative ones. Then countries with higher values of θ will tend to have higher tax rates.

When coupled with the the number of parties increasing in the size of the political spectrum, a positive correlation between θ and τ is sufficient to obtain the conventional wisdom. Countries endowed with a wider range of views have more parties, and these parties are likely to support larger government sectors. If one were to regress taxes, spending, and/or transfers on the number of parties in a country, we would expect to find a positive relationship. Fragmentation appears to inflate the size of government. That is precisely the conventional wisdom that has prevailed for decades.

This paper asks a slightly more nuanced question: when conditioning on country-specific political factors, what then is the effect of fragmentation? That question can be addressed by including country fixed effects. Controlling for all time-invariant, country-specific factors isolates the impact of fragmentation from the effects of a country's θ parameter. As discussed above, fixed effects capture many likely candidates for the causes of differing values of θ, such as ethnolinguistic fractionalization or the particulars of a country's constitutional history.

We have seen in Table 2 that including country fixed effects suggests a negative relationship between fragmentation and taxes. What is the mechanism driving this result? The intuition is subtle. Consider the same country as described by Figure 2.5. The value θ_2 remains unchanged and without loss of generality suppose parties 1 through 4 are in a coalition government. Now suppose that the second party exogenously splits in two. This is depicted in Figure 2.6.

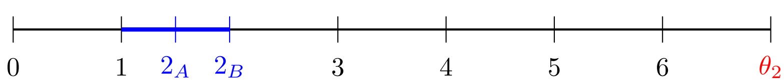


Figure 2.6: A country of type θ_2, now with more fragmentation

The segment previously occupied by Party 2 is now divided between 2_A and 2_B. It is instructive to note that the split was independent of this country's θ and that the extent of fragmentation has exogenously increased. The coalition now reevaluates its policy choices. Recall from Section 2 that the government chooses tax rates τ, global public good G_1, and targeted public good ("pork") G_2. All parties benefit from lower taxes, and the marginal benefit of lower taxes has remained constant. Conversely, recall that G_2 is divided among coalition partners. With an increase in fragmentation, an extra dollar in G_2 is distributed among more people, leaving less for each individual member. Consequently the parties desire a shifting of resources away from spending on G_2 towards lower taxes. Revenue falls, as does expenditure, as does transfer intensity.

The model above provides one plausible explanation what may be driving the results from Section 2.3.2 that suggest that an increase in fragmentation leads to a smaller public sector.. To examine whether these results are robust, the next sections repeat the empirical investigation of Section 2.3.2 with some modifications. Firstly I confirm the main results hold for executive fragmentation as well as legislative fragmentation. Secondly I test the results with a different, wider panel of OECD countries, including the new post-Soviet democracies. Thirdly I use alternative empirical measures of taxation and political fragmentation. Fourthly I show the results do not depend on the ideological composition of government. Fifthly I show the results are robust to the inclusion of macroeconomic controls. Finally I test if the results are robust to the phase of the electoral cycle.

2.3.4 Executive Fragmentation

The preceding section analyzed the effects of legislative fragmentation on tax policy. It is debateable whether the legislative branch is the appropriate object of study here. Arguably it is the executive branch which warrants closest inspection. Indeed the actors of the model in Section 2 are assumed to be in a government coalition. This section thus repeats the empirical tests above for executive fragmentation. In short, I demonstrate that the results hold for executive fragmentation as well as legislative fragmentation.

The data include details on the type of government in country i at time t. These are coded on a 1-7 scale by the political scientists leading the project. The summary

Table 2.5: Type of Government

	Freq.	Percent	Cum. Percent
Single party government	201	25.74	25.74
Minimal winning coalition	254	32.52	58.26
Surplus coalition	160	20.49	78.75
Single party minority	96	12.29	91.04
Multi party minority	65	8.32	99.36
Caretaker government	5	0.64	100.00
Total	781	100.00	

Fragmentation of government, on a 1-7 scale

statistics are included in Table 5. As we can see, there is considerable variation in the extent of executive fragmentation. For instance, minority governments have been in power for more a fifth of country-years in the OECD since 1975. Not surprisingly, this measure is positively correlated with legislative fragmentation.

Table 6 is the executive fragmentation analogue of Table 2. Instead of regressing policy outcomes on legislative fragmentation, Table 6 shows the results for executive fragmentation.

Table 2.6: Decline in taxes, spending, and transfers: executive (long)

	Receipts		Expenditure		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Executive Fragmentation	2.523***	-0.438***	1.844***	-0.396	0.633***	-0.0462
	(0.352)	(0.150)	(0.395)	(0.265)	(0.211)	(0.0714)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	723	723	723	723	753	753

The same pattern emerges. All within-country estimates demonstrate negative coefficients, albeit without significance for expenditure and transfers. However the results are of the opposing sign, and statistically different from, the effects predicted by the conventional wisdom.

2.3.5 More countries, shorter panel

A problem with analysis of the large-T panel data in Sections 2.3.2 and 2.3.4 is that the possibility of non-parallel trends increases in T, and this threatens identification. Consequently I repeat the analysis on a larger panel that includes post-Soviet countries. Obviously this requires shortening the time horizon. The data (Armingeon et al., 2012b) again come from the Institute of Political Science at the University of Bern. They include measures of political competition as well as primary macroeconomic variables such as government revenue for 35 countries[3] since 1990. Table 7 summarizes the data, and Table 8 presents the main regression results.

Table 2.7: Summary statistics for 35 countries, 1990–2010

	Mean	Std. Dev	N	Min	Max
Gov't receipts (% GDP)	41.90	7.36	719	24.30	63.13
Gov't expenditure (% GDP)	44.41	7.35	719	24.70	70.54
Soc sec transfers (% GDP)	13.42	3.45	728	5.55	23.66
Eff. parties parliament	3.81	1.46	762	1.74	10.92
Transfers (% Gov't receipts)	31.98	6.19	713	11.89	49.95
Transfers (% Gov't expenditure)	29.97	4.82	713	10.53	39.96

The results here are again fully supportive of the theory, just like the original results found of Section 2.3.2. It is useful to recall the results from Table 2. The coefficients found for the effect of fragmentation on receipts, outlays, and transfers were -1.045, -1.939, -0.697 respectively. The analogous coefficients here are -0.464, -0.732, and -0.204. The results in the longer sample are of the same sign and order of magnitude of the results in the original sample. Although slightly closer to zero, the coefficients remain significant at conventional levels. I interpret these results as

[3]Australia, Austria, Belgium, Bulgaria, Canada, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Latvia, Lithuania, Luxembourg, Malta, Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland, UK, and USA.

Table 2.8: Decline in taxes, spending, and transfers

	Receipts		Expenditure		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties parliament	1.456***	-0.464***	0.903***	-0.732**	0.331***	-0.204**
	(0.104)	(0.133)	(0.162)	(0.325)	(0.0911)	(0.0963)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	716	716	716	716	725	725

support for the model and the conclusion of Section 2.3.2

Further evidence can be seen in Table 9, the effects of legislative fragmentation on transfer intensity α. Although neither coefficient are found to be significant ($p < 0.14$), the sign confirms the negative relationship.

Table 2.9: Effects on α

	Transfers Intensity	
	(1)	(2)
Eff. parties parliament	-0.348	-0.491
	(0.228)	(0.315)
Year FE	Yes	Yes
Country FE	No	Yes
N	710	710

2.3.6 Different measure of taxation

The second robustness check is to use an alternative measure of taxation. Section 2.3.2 relied on total tax receipts as a fraction of GDP. This could be affected by issues such as windfall receipts from natural resource discoveries. Therefore in the spirit of Mendoza, Razin and Tesar (1994), I test the model with a more micro-founded measure of income tax. As we can see from Figure 2.7, these tax rate data map neatly to the conventional wisdom.

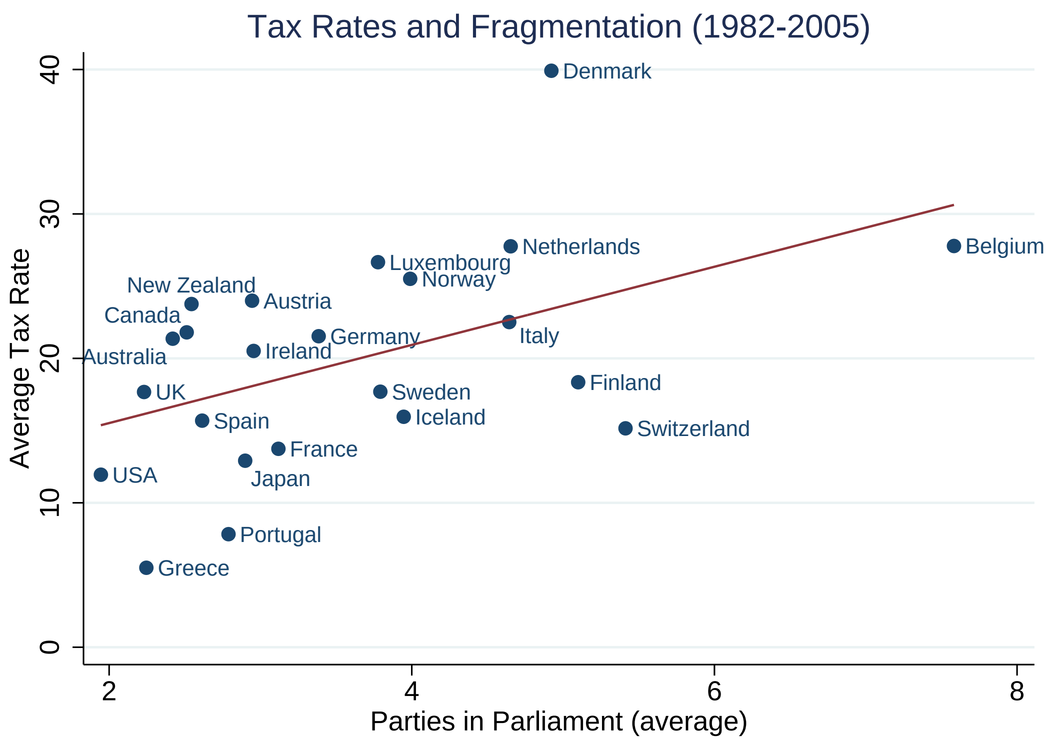


Figure 2.7: Relationship with alternative measure of taxation

These additional tax rate data come from Peter, Buttrick and Duncan (2010). This dataset emphasizes the actual tax rates paid by individuals at specified income levels (average wage, twice the average wage, etc.) rather than focusing on total receipts of the state. The main variable employed is the tax rate for the mean income level after adjusting for allowances, credits, local taxes, etc. This years included are 1982 through 2005.

Again there is a pattern of coefficients changing sign. The result on $4x$ average income, the coefficient of which is positive, seems to reject a theme of my model. However, the model does not make predictions about the progressivity of the tax schedule. The model is about overall tax rates, and is silent on taxes on higher

Table 2.10: Alternative Tax Measure

	Average Income		Avg Income x2		Avg Income x4	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties	0.921***	-0.253	1.097***	-0.210	0.988***	0.461
	(0.199)	(0.349)	(0.168)	(0.391)	(0.254)	(0.357)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	493	493	493	493	493	493

incomes. Consequently the most useful comparison then is the difference between Column 1 and Column 2, which measures taxes paid at average income levels. The results here are consistent with the model. The results with fixed effects are not statistically significant. This is perhaps not surprising as the inclusion of fixed effects reduces the number of degrees of freedom by 35. Although they are not significant, they are negative. Furthermore, they are significantly different from the positive coefficients predicted by excluding fixed effects.

2.3.7 Different measure of fragmentation

An alternative measure of legislative fragmentation that is closely correlated (but not identical) to the effective number of parties, was proposed by Rae (1968). This is a nonlinear transformation of the effective number of parties. If we define the effective number of parties as e, then the Rae measure equals $\frac{1}{1-e}$. Table 11 shows the regression output. It would be concerning if this transformation substantially changed the interpretation of my results.

Table 2.11: Alternative Fragmentation Measure

	Receipts		Outlays		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Rae Measure	0.357***	-0.154***	0.265***	-0.250***	0.0881***	-0.0613***
	(0.0174)	(0.0258)	(0.0204)	(0.0431)	(0.0111)	(0.0183)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	No	Yes	No	Yes
N	783	783	783	783	818	818

2.3.8 Ideological composition

Table 13 shows the effects of including controls for political ideologies. To ensure robustness, I measure political ideology at both the executive and legislative level. At the executive level, I include the fraction of cabinet posts held by people of differing political persuasions. At the legislative level, I control for the fraction of the parliament seats won by a country's major socialist, conservative, liberal, and religious parties. Summary statistics are provided in Table 12.

Table 2.12: Summary statistics for ideological variables

	Mean	Std. Dev	N	Min	Max
Right-wing gov't (%)	38.26	38.58	825	0.00	100.00
Centrist gov't (%)	23.92	30.30	825	0.00	100.00
Left-wing gov't (%)	34.79	37.89	825	0.00	100.00
Socialist par't (%)	28.36	17.19	826	0.00	63.60
Conservative par't (%)	17.20	20.25	826	0.00	74.80
Liberal par't (%)	12.55	17.75	826	0.00	67.10
Religious par't (%)	9.38	13.95	826	0.00	44.30

The odd-numbered columns in Table 13 investigates the effect of ideological divisions on the executive dimension. The even-number columns include controls for the legislative dimension. We can see that for the most part neither the executive nor legislative controls are statistically or economically significant. Moreover, they do not substantially alter the coefficients on the effective number of parties. The results that fragmentation lowers taxes, spending, and transfers remains robust.

Table 2.13: Effects of Ideological Composition

	Receipts		Outlays		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties	-1.149***	-1.097***	-2.183***	-1.899***	-0.796***	-0.719***
	(0.304)	(0.337)	(0.397)	(0.445)	(0.175)	(0.185)
Right-wing gov't (%)	-0.0303		-0.0742**		-0.0259	
	(0.0286)		(0.0354)		(0.0154)	
Centrist gov't (%)	-0.0331		-0.0736**		-0.0327**	
	(0.0264)		(0.0312)		(0.0132)	
Left-wing gov't (%)	-0.0262		-0.0631*		-0.0298*	
	(0.0279)		(0.0363)		(0.0151)	
Socialist par't (%)		0.0357*		0.0234		0.00619
		(0.0200)		(0.0340)		(0.0171)
Conservative par't (%)		0.0157		0.0177		0.0101
		(0.0174)		(0.0231)		(0.0132)
Liberal par't (%)		0.0543**		0.0221		0.00811
		(0.0253)		(0.0376)		(0.0177)
Religious par't (%)		-0.116***		-0.0206		-0.0303*
		(0.0381)		(0.0522)		(0.0169)
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	783	783	783	783	817	818

2.3.9 Macroeconomic controls

Perhaps the changes in tax policy are responses not to fragmentation, but to macroeconomic shocks. There as a further robustness check, I control for some macroeconomic variables. The variables included are summarized in Table 14.

Table 2.14: Summary statistics for 23 countries, 1975–2010

	Mean	Std. Dev	N	Min	Max
Gov't receipts (% GDP)	42.56	8.40	785	24.35	63.20
Gov't expenditure (% GDP)	45.14	8.18	785	26.07	70.54
Social security transfers (% GDP)	13.68	3.88	820	4.34	23.89
Eff. parties	3.59	1.43	826	1.69	9.07
International Trade (% GDP)	72.48	44.99	828	16.01	319.55
Population over 65 (millions)	4.87	7.51	818	0.02	40.54
National Debt (% GDP)	61.22	30.77	755	4.64	192.74

The inclusion of macro controls are potentially endogenous. Consequently I intentionally chose control variables that are at least partly outside the control of government: openness to international trade, population over 65, and national debt. International trade is well explained by the geographical size of a country and proximity to its neighbors; modern democracies have limited control on the size of its adult population; and national debt is a a stock variable that a government may have difficulty substantially affecting. Although these controls are likely less endogenous than variables such as deficit level or inflation rate, they should be interpreted carefully. Regression results with and without macro controls are included in Table 15.

Table 2.15: Effect of Macro Controls

	Receipts		Expenditure		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties	-1.045***	-0.667***	-1.939***	-1.277***	-0.697***	-0.334**
	(0.319)	(0.202)	(0.430)	(0.236)	(0.156)	(0.124)
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls	No	Yes	No	Yes	No	Yes
N	783	742	783	742	818	742

Macroeconomic factors do catch some of the residual variance and consequently influence our coefficients of interest. In general, the magnitudes drop by about half. The substantive results — and the sign of the relationship — remain the same.

2.3.10 Electoral cycle

The results are also robust to phases of the electoral cycle. Table 17, which is large and thus left to the appendix, illustrates this. I include controls for year before, year of, and year after election. These results are generally negative but insignificant. They are somewhat significant on receipts: taxes do indeed go down in an election year. In no specification do the electoral cycle variables meaningfully alter the main parameters of interest.

2.3.11 Heterogeneous treatments

As mentioned in Section 2.1, the model predicts nonlinearity in the marginal effects of n on tax policy. Both the tax rate τ and the fraction of revenue dedicated to transfers α are bounded by $[0, 1]$, so all OLS-like estimates such as those above provide linear approximations of the effect. As the outcome is bounded, these effects cannot hold over the complete range of the X variable. In particular, the model predicts that a change in n at low levels will have a larger effect than a change in n at high levels. We expect that results are stronger for smaller values of n. To test this theory, I split the sample into above- and below-median values of the effective number of parties.

We can see for receipts and outlays that the effect is about three times larger for the below-median values of n than the above-median values. Interestingly, the results appear approximately constant for transfers. I conclude that there is strong evidence of nonlinearity in effects on receipts and outlays, but no such evidence for transfers.

Table 2.16: Decline in taxes, spending, and transfers: heterogeneous treatments

	Receipts		Expenditure		Transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Eff. parties	-2.584***	-0.742**	-4.053***	-1.681***	-0.688*	-0.726***
	(0.760)	(0.333)	(1.004)	(0.399)	(0.400)	(0.161)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Above Median	No	Yes	No	Yes	No	Yes
N	396	387	396	387	406	412

2.4 Conclusion

This paper asks how political fragmentation affects fiscal policy outcomes. I modeled this as a common pool problem where a coalition of n members chooses tax and expenditure policies. Unlike other models which constrain the coalition's actions through norms, the coalition's choice essentially corresponds to a group welfare maximization problem. The coalition can fund two types of good: the pure public good which is shared by all, and the local public good which is targeted to political constituencies. The model shows that governments fund targeted public goods above the level a benevolent social planner would choose, and consequently that governments set tax rates that are inefficiently high. These results are standard in the literature.

Next the paper showed how these results respond to a change in n. Comparative static analysis indicates that taxes, spending, transfers, and transfer intensity fall as the coalition becomes more fragmented. Fragmentation reduces the inefficiency. These results are not standard. Over the decades, the theoretical and empirical literatures on the question have generated a consensus ("conventional wisdom") that fragmentation leads to a larger public sector. My results run counter to the conventional wisdom.

I test these results with data from dozens of OECD countries over the past forty years. I replicate the conventional wisdom using a naive estimation procedure. An improved specification which includes country fixed effects has results that are wholly different from the conventional wisdom. Rather than a positive relationship between fragmentation and the size of government, the relationship is negative. This holds true for receipts, expenditures, transfers, the fraction of government revenue assigned to transfers, and various forms of transfers. This results also hold true for legislative as well as executive fragmentation. The results are not affected by the ideological composition of government or parliament, or the phase of the electoral cycle.

The contributions of this paper are twofold. Firstly, it supports the empirical result of Pettersson-Lidbom (2012) with greater external validity than quasi-experimental settings can provide. The selection of data from a panel of developed nations lends the empirical section to a battery of robustness tests. The results are robust to different specifications, measures of executive fragmentation, alternative data sources, ideological controls, and electoral cycle effects. Secondly, the paper provides an intuitive theoretical foundation that motivate these results. The conventional wisdom in the literature is that more fragmented governments lead to larger public sectors. Both the theoretical and empirical sections suggest that the conventional wisdom is incorrect. The model in Section 2 could be extended to incorporate more nuance in the effect of fragmentation on legislative bargaining. This is an avenue for future work.

Appendix: Tax Policy and the Electoral Cycle

Table 2.17: Effects of Electoral Cycle on Receipts, Outlays, and Transfers

	Receipts				Outlays				Transfers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Eff. parties	-1.045***	-1.038***	-1.006***	-1.103***	-1.939***	-1.943***	-1.946***	-2.111***	-0.697***	-0.699***	-0.661***	-0.730***
	(0.319)	(0.319)	(0.336)	(0.314)	(0.430)	(0.432)	(0.458)	(0.430)	(0.156)	(0.159)	(0.158)	(0.144)
Election Year		-0.160	-0.0943	0.00554		0.0926	0.199	0.590		0.0662	0.126	0.309*
		(0.160)	(0.245)	(0.343)		(0.239)	(0.331)	(0.456)		(0.107)	(0.134)	(0.182)
Pre-election Year			0.0180	0.103			0.0496	0.376			0.0537	0.183
			(0.237)	(0.298)			(0.266)	(0.330)			(0.0998)	(0.124)
Post-election Year				0.0463				0.543				0.243*
				(0.267)				(0.365)				(0.142)
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	783	783	760	742	783	783	760	742	818	818	797	775

Driscoll-Kraay standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER III

Crime and the Labour Market in Ireland, 2003–2014

3.1 Introduction

This paper provides a detailed statistical analysis of relationship between crime and the labour market in Ireland during and after the Celtic Tiger. How does the number of burglaries in an area change as the number of people on the unemployment register[1] increases? I find that there is a robust and significant increase. I estimate a property crime elasticity of about 0.5. This means that a 10% rise in the number of people on the unemployment register in a county increases the number of property crimes in that county by 5%.

The data cover crimes reported from every police station in Ireland, and the conclusions are similar for both theft (estimated elasticity of 0.57) and burglary (0.49). As predicted by economic theory, the association with assault is much smaller: I estimate an assault elasticity of 0.06. Sex offences are also strongly correlated

[1] The 'Live Register' is an administrative count of the number of people registered for unemployment assistance. It measures how many people are receiving benefits while seeking employment, not unemployment per se. For this reason this subject of this paper is crime and the labour market, not crime and unemployment.

with increasing unemployment and underemployment. The finding that the labour market can determine domestic violence and/or sexual assault has been documented previously, cf. Aizer (2010) and Edmark (2005).

The time-frame in question, 2003–2014, is a unique chapter in Irish history. The first half captures the Celtic Tiger, a period of exceptional economic growth and prosperity. During this time, the government ran large surpluses, and hundreds of thousands of people from the newly enlarged EU migrated to Ireland. The construction sector grew by more than 10% per annum, and property prices soared.

The latter half includes the financial crisis and subsequent dramatic collapse of the Irish economy. The construction industry contracted by three-quarters. In just over three years, the unemployment rate rose from 4.5% to 14%. Ultimately, the Irish government required financial assistance from the European Union and the IMF. Such a huge contrast in fortune is essentially unprecedented in a developed economy. The period is thus not only an important chapter for Ireland, but a fascinating time in economic history more generally. The primary research question of this paper is how property crime responded.

The study of criminal behaviour as a consequence of the economic environment is not new. It came to the fore in economics with Becker (1968). Becker's contribution was seminal for subsequent empirical work. This literature is now vast. Analyses have been conducted in many contexts. For example, see Raphael and Winter-Ebmer (2001) and Levitt (1996, 1997) on the United States; Machin and Meghir (2004) on England and Wales; de Blasio and Menon (2013) on Italy; Carneiro et al. (2016) on Brazil; Dube and Vargas (2013) on Colombia; Fougére et al. (2009) on France; Flückiger and Ludwig (2015) for how fishing conditions affect maritime piracy; and Aslund et al. (2015) and Anderson (2014) for the effect of schooling laws on youth crime.

To my knowledge the most comparable analysis from Ireland is Denny, Harmon and Lydon (2004), over which this paper has at least three advantages. Firstly, the large changes in the labour market pre- and post-2008 provide near-ideal variation for estimation within a short horizon. Secondly, using local-level crime statistics, this paper can estimate relationships using within-county variation. This is advantageous because it eliminates many concerns about the crime-labour market relationship varying between differing geographic regions. Finally, rather than being restricted to data on burglary alone, the dataset used in this paper includes several classes of crime such as theft and assault.

Section 2 provides an overview of the data used in the analysis, while Sections 3 and 4 provide a variety of OLS-based estimates of the relationship. Due to concerns of omitted variable bias, the literature has generally not relied on OLS-based estimates alone. Consequently in Section 5 I present instrumental variables estimates that are based on the region-sector instrument proposed by Bartik (1991). Section 6 concludes.

3.2 Data Overview

The crime data used in this paper are the reported crime statistics provided by An Garda Síochána to the Central Statistics Office (CSO). The Garda PULSE system forms the basis of these reports. The analysis in Sections 3 and 4 are conducted at the Garda Division level. With the exception of Cork[2] and Dublin[3], Garda divisions largely coincide with county borders.[4] This allows me to analyze crime at a

[2]Cork is split into Cork City, Cork North, and Cork West.

[3]Dublin is split into six Dublin Metropolitan Regions: North, South, East, West, North Central, and South Central.

[4]The remaining divisions are Cavan/Monaghan, Clare, Donegal, Galway, Kerry, Kildare, Kilkenny/Carlow, Laois/Offaly, Limerick, Louth, Mayo, Meath, Roscommon/Longford, Sligo/Leitrim, Tipperary, Waterford, Westmeath, Wexford, and Wicklow.

disaggregated, local level. The data, which are made publicly available by the CSO, show how many crimes were recorded each quarter by Gardaí in every division.

I use the number of people on the Live Register to measure the health of the labour market. The Live Register (LR) data, also provided by the CSO, include information on composition by sex and age-group (under- or over-25), and are recorded at the social welfare office level. Live Register numbers are released monthly. Crime statistics are released quarterly. To ensure consistency, I combined LR numbers for three months into a quarterly average. I then aggregated the LR numbers from social welfare offices up to the Garda division level.

It is pertinent to note that the Live Register (LR) does not measure unemployment. The LR includes all people receiving Jobseekers' Allowance and/or Jobseekers' Benefit. This includes, for example, low-income part-time workers. This is a desirable feature for the purposes of this paper: by regressing crime on LR figures, I am capturing both underemployment as well as unemployment. This ensures that I capture the broad effect of changes in the labour market on crime, rather than the specific effect of strictly-defined unemployment.

Summary statistics are presented in Table 1. I focus on four types of crime. Two are standard measures of property crime: thefts and burglaries.[5]

The two additional types of crime are assault and sexual offences[6]. The implicit economic theory underlying the analysis is a standard Becker (1968)-type model that crime can be represented as an alternative to traditional employment. A negative shock to the economy transfers people from the labour market to the 'informal alternative'. Consequently we expect a strong relationship between unemployment

[5]Robbery is excluded because of its relative infrequency. The median number of thefts per quarter in a division is 465. The median number of robberies is 10. Robberies are included in the 'All property crime' variable.

[6]The vast majority of sex offences in the data (93%) are listed as rape.

Table 3.1: Summary statistics (21 local areas)

	Mean	Std. Dev	N	Min	Max
Live Register (thousands)	13.99	16.09	966	2.9	110.7
Live Register (logged)	9.22	0.74	966	7.98	11.61
Theft	849.1	1837.2	966	161	10484
Burglary	295.1	524.8	966	45	3301
Assault	153.5	188.0	966	39	1150
Sexual offences	20.1	28.9	966	1	247
All property crime	1174.8	2448.8	966	248	13694
All violent crime	174.7	215.9	966	45	1368
Population 2006 (thousands)	201.9	234.9	966	79	1187
Population 2011 (thousands)	218.5	251.7	966	86	1273

Statistics are calculated for Garda Divisions. Due to geographic proximity, districts in Cork and Dublin are aggregated to the county level.

and property crime, primarily through the mechanism of increased marginal utility of consumption from lower income levels. We have less reason to expect a strong relationship between unemployment and, say, assault. However there may still be an effect of unemployment on assault if e.g. the opportunity cost of incarceration is lower if one does not have a job. Similarly, I investigate the response of sexual offences to unemployment, supplementing the literature finding that the number of such offences can depend on labour market conditions.[7]

[7] This analysis assumes that the statistics reported to An Garda Síochána are an accurate measure of crimes committed. Of course if this assumption is violated, specifically if the rate of reporting changes between areas over the period, my results will be invalid. Due to their particularly personal nature, I suspect this is more likely to be the case for sexual offences than property crimes.

3.3 OLS Fixed Effects Estimates

The primary method of estimation is Ordinary Least Squares (OLS) with time and district fixed effects.[8] Thus the model is an unobserved heterogeneity model:

$$y_{it} = a_i + \delta_t + \beta x_{it} + \epsilon_{it}$$

where y_{it} is crime in district i at time t, a_i is a district (e.g. county) fixed effect, δ_t represents the time (quarterly $\times$ year) fixed effects, β is our coefficient of interest, x_{it} is the number of people on the Liver Register in district i at time t, and ϵ_{it} is the error term.

Unobserved heterogeneity models are estimated on changes within districts rather than between districts. This means that any and all time-invariant characteristics are controlled for in the analysis. Consequently concerns that e.g. Dublin might have consistently higher crime than Kildare are quelled by this estimation procedure. The localised nature of the data, which permits the inclusion of district fixed effects, thus gives us a much greater degree of confidence in the estimates. The inclusion of time fixed effects eliminates comparable concerns about time trends in crime: if the national crime rate was unusually high in, say, the third quarter of 2004, this will not distort the estimates.[9] The regression results are reported in Table 2.

Table 3.2: Effects of the numbers on the Live Register (in thousands) on crime

[8] This strategy has been used in other papers in the literature, e.g. Edmark (2005).

[9] Although not included here, the results are also robust to the inclusion of quadratic and cubic time trends.

	Theft	Burglary	Assault	Sexual	All Property	All Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Live Register	8.916***	1.536***	1.414***	0.339***	11.66***	1.728***
	(0.707)	(0.171)	(0.0823)	(0.0165)	(0.922)	(0.0916)
Garda Division FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	966	966	966	966	966	966
Adjusted R^2	0.995	0.981	0.989	0.909	0.995	0.990

Results show the relationship between the total number of people on the Live Register in a division and various forms of crime in that division. The data are quarterly from 2003–2014. Standard errors are clustered at the Garda Division level. All results are weighted by Census 2006 populations.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

It is reasonable to give each unit of observation (i.e. Garda division) an identical weight in the analysis. This would give all divisions equal importance in the estimation. A more nationally representative estimate is obtained by weighting districts by population. In our case, variations in populations can be quite large, e.g. Meath has approximately twice the population of Westmeath. Consequently all tables are weighted by their Census 2006 populations.[10]

Table 2 shows that property crime is well correlated with deteriorations in the labour market. The interpretation of the coefficient in column 1 is that a 1,000-person increase in the Live Register is associated with an increase of about 9 thefts per district, per quarter. Similarly an extra thousand people on the Live Register is expected to increase the number of violent crimes (defined as homicide, sex offences, and assaults) in each district in each quarter by about 1.7. Overall we can see that, holding everything else constant, property crime (defined as all thefts, burglaries, and robberies) is several times more responsive to unemployment than violent crime.

[10] I have also conducted the analysis with no weights, and using Census 2011 population weights. Magnitudes move around by changing population weights, but the qualitative interpretations remain the same.

Table 3.3: Estimates of the elasticity of crime with respect to Live Register figures

	Theft	Burglary	Assault	Sexual	All Property	All Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Live Register (logged)	0.569***	0.491***	0.0582	0.627***	0.540***	0.121
	(0.116)	(0.114)	(0.109)	(0.158)	(0.104)	(0.107)
Garda Division FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	966	966	966	966	966	966
Adjusted R^2	0.994	0.985	0.983	0.919	0.995	0.985

Results show the relationship between the logged number of people on the Live Register in a division and the logged form of various forms of crime in that division.

These results are all highly statistically significant, but that tells us little about the economic significance. Rather than reporting the effect in terms of absolute numbers, it is informative to consider the results in percentage terms. In particular, taking the log of both the number of crimes and the number of people on the Live Register permits the interpretation of the coefficients as elasticities: how a percent change in an independent variable leads to a percent change in the dependent variable. Table 3, which reports the results from this specification, further corroborates the evidence in Table 2. For example, the coefficient of 0.569 in first column of Table 3 implies that a 10% increase in the number of people on the Live Register in a district is associated with a 5.69% contemporaneous increase in thefts in that district. With an estimated elasticity of 0.491, the magnitude is very similar for burglary. Taking the results in Table 3 collectively, we conclude again that property crime is strongly positively correlated with the Live Register; that the effect on assaults is not statistically distinguishable from zero; that sex offences are surprisingly sensitive to the conditions of the labour market; and that overall the results are several times stronger for property crime than for violent crime.

3.4 First Difference Estimates

One further method to investigate the relationship is through first difference (FD) estimation. The FD approach estimates the same parameter as FE estimation, but rather than utilizing unit-specific fixed effects to capture unobserved heterogeneity, FD estimation removes unobserved heterogeneity by differencing adjacent periods. The FD approach is thus very similar to the within (FE) estimator approach, and identical in the two-period case, but requires a slightly weaker condition for consistency[11], and thus any great divergences in estimates should raise concerns. Table 4 presents these estimates using annual changes.

Table 3.4: First differences estimates (annual)

	Theft	Burglary	Assault	Sexual	All Property	All Violent
	(1)	(2)	(3)	(4)	(5)	(6)
Live Register (change)	65.93***	45.68***	6.597***	0.642***	120.5***	7.308***
	(3.934)	(2.541)	(0.449)	(0.104)	(6.928)	(0.511)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	231	231	231	231	231	231
Adjusted R^2	0.308	0.414	0.424	0.373	0.326	0.416

Results show the relationship between the change in the number of people on the Live Register in a division and the change in various forms of crime in that division.

The results in Table 4 provide further evidence that crime responds to the labour market. These results are based on annual changes, and thus the coefficients are expected to be approximately four times as large as the quarterly estimates in Table 2. This estimation method suggests that over the course of a year, a 1,000-person increase in the Live Register in a district is associated with an additional 120 property and an additional seven violent crimes. The magnitude of the results are comparable

[11]The strong exogeneity condition for FE is reduced to an adjacent-period exogeneity condition for FD.

to but larger than the earlier property crime estimates and essentially identical to the earlier estimates of the effect on violent crimes: the analogous estimates from Table 2 are 47 additional property crimes and seven additional violent crimes.

Taking the estimation results on the whole, I conclude that a deterioration in labour market conditions is associated with an increase in crime. In particular, my estimates suggest that a 10% increase in the number of people on the Live Register leads to a 5% increase in theft, burglaries, and robberies. As one might expect, there is also a positive relationship between a poor labour market and violent crime, but that the relationship is considerably weaker than the relationship between labour markets and property crime.

As mentioned earlier, the inclusion of district fixed effects removes concerns about any time-invariant omitted variables and quarter $\times$ year fixed effects capture time trends. However "there is nothing explicitly causal" (Levitt, 2001) about the interpretation of these parameters. For example, one might suspect that there is a problem of reverse causality: that companies relocate to avoid crime, and thus crime partly causes unemployment. For additional evidence on the effect of unemployment on crime I employ another, explicitly causal, identification strategy: an instrumental variable.

3.5 Instrumental Variable Estimates

Instrumental variables are a means of identifying causal relationships by generating estimates from a plausibly exogenous mechanism. This is a hugely popular estimation method and a large literature exists on the advantages and disadvantages of IVs, see e.g. Angrist, Imbens and Rubin (1996) and Bound, Jaeger and Baker (1995).

A valid instrument requires two components. First, it must be relevant to our the regressor. In this paper, this means that an instrument should be strongly correlated with local labour market conditions. Second, the instrument must be exogenous. For our purposes, this means that the instrument can only affect crime through its impact on unemployment and not through other means.

My instrumental variable strategy uses regional variation, sectoral intensity, and national growth rates to create an instrument similar to those made popular by Bartik (1991). This approach has been used previously in the literature on crime and unemployment (Gould et al., 2002).

The intuition behind the instrument is quite simple: some regions are more affected by sectoral shocks than others. Consider the construction sector. In 2006, more than 200,000 people were employed in construction. Starting in 2007, the construction sector in Ireland declined rapidly. With a decline in activity of 75%, regions with higher levels of employment in construction during the boom could be expected to see relatively more redundancies later. This is the intuition behind the Bartik instrument, but there is no need to restrict ourselves to the construction sector. By applying this logic across all sectors, we can generate powerful predictors of unemployment for each region. Specifically, let $s_{ir}(t)$ be industry i's share of total employment in region r at time t. Similarly let $g_{ir}(t)$ be the employment growth rate in industry i for region r between times $t-1$ and t. Now let $\hat{g}_{ir}(t)$ be the "almost-national" growth rate in industry i for region r between times $t-1$ and t. It is almost-national because it is the employment growth of that industry in all *other* regions. Formally, in an economy with R regions and I industries, $\hat{g}_{ir}(t) = (R-1)^{-1} \sum_{s \neq r}^{R} g_{is}(t)$. We can then define the Bartik instrument for the percent change in region r's employment between date $t-1$ and t as $z_r(t) = \sum_{i=1}^{I} \hat{g}_{ir}(t) s_{ir}(t-1)$.

The instrument's exclusion restriction is embedded into the creation of the 'almost-national' growth rate. By omitting region r's effect in the calculation of the national growth rate, we create a predicted growth rate that by design excludes the influence of region r.

For this portion of the analysis, the data on the labour market come from the CSO's Quarterly National Household Survey (QNHS). The QNHS details the numbers employed in each of the fourteen NACE-2 economic sectors[12] by region. Therefore the analysis is conducted at the regional[13] level. Consequently the analysis in this section will focus on changes in employment rather than changes in the Live Register, and by region rather than by Garda Division.

Although most Garda Divisions are easily aggregated up to regional level, e.g. Mid-East comprises the Kildare, Meath, and Wicklow Garda Divisions, complications arise for the Tipperary and Roscommon/Longford Garda Divisions.[14] To ensure consistency, I use crime data at the Garda Station level from the All-Island Research Observatory. This requires aggregation up from the station level.[15] Due to the highly localised nature of the data, they are not available for particularly sensitive offences such as sexual assault. However they are available annually for theft and burglary. I therefore restrict attention to these crimes.

The results of first-stage regression are shown in Table 5. The specification is a first difference estimate of changes in annual employment in each of the eight NUTS

[12] Agriculture, forestry and fishing; Construction; Wholesale and retail; Transportation and storage; Accommodation and food service; Information and communication; Professional, scientific and technical; Administrative and support services; Public administration and defence; Education; Human health and social work; Industry; Financial, insurance and real estate; and Other.

[13] The eight NUTS 3 regions of Ireland are Border, West, Midlands, Mid-East, Dublin, South-East, South-West, and Mid-West.

[14] South Tipperary is in the South-East Region, and North Tipperary is in Mid-West. Similarly, Roscommon is in the West region, and Longford part of Midlands.

[15] I classify crimes recorded in Nenagh, Templemore, and Thurles as North Tipperary and therefore as Mid-West. Crimes recorded in the Cahir, Clonmel, and Tipperary districts are attributed to the South-East region. Within the Longford/Roscommmon Division, Roscommon includes any crimes from the Boyle, Castlerea, Roscommon districts; Longford comprises Longford and Granard.

Table 3.5: First Stage results for Bartik Instrument

	Change in Employment
Bartik Instrument	0.754***
	(0.134)
Year FE	Yes
Excluded F-stat	31.49
N	72
r^2	0.873

Results show the relevance of the instrumented change in number of people employed in a region and the actual change as recorded in the QNHS. The data are annual from 2003–2013. Standard errors are clustered by NUTS Level 3 region. Results are weighted by Census 2006 populations.

3 regions between 2003 and 2013. Perhaps not surprisingly, the Bartik instrument is positively correlated with the change in employment. Further, the coefficient is near 1. Importantly, the relationship is significant and provides an F-statistic on the relevance of the excluded instrument equal to 31.49. The 'rule of thumb' for IV relevance is that the first-stage (excluded) $F \geq 10$. This is easily satisfied. Weak instrument tests are rejected with $p < 0.01$.

Table 3.6: Bartik Instrument estimates for the effect of employment on crime

	Theft	Burglary
	(1)	(2)
Change in Employment (Bartik)	-28.40***	-24.64***
	(6.196)	(3.700)
Year FE	Yes	Yes
N	72	72
r^2	0.177	0.639

Results show the (second-stage) relationship between change in employment (using Bartik instruments) and change in theft and burglary.

Table 6 presents the results from the second-stage of the IV regression. These are first difference estimates of (instrumented) annual numbers employed (*not* unemployed) on property crime. As discussed in Section 4, there are good reasons to believe that the annual estimates from first differences estimation are reasonable and satisfy the exogeneity assumptions of the procedure. The results match our prior expectation: an increase in the numbers employed result in a significant decrease in the number of property crimes. In particular, a 1,000-person increase in the numbers employed in a region reduce the annual number thefts in that region by about 28. This effect is again of the anticipated sign and of a reasonable magnitude. The effect for burglary is very similar. Overall these results indicate that an increase in employment of 1,000 reduces by about 50 the number of property crimes in a region. These estimates are very close to those obtained by the fixed effect estimates of Section 2, and therefore the IV results provide clear additional evidence that improvements in the labour market reduce crime.

3.6 Conclusion

This paper contributes to the literature on how crime responds to local labour market conditions. I created a unique dataset capturing both crime and labour market statistics at a disaggregated level. As Ireland is a developed country that has experienced fascinating business cycle volatility in the past decade, the results will be of interest both in Ireland and internationally. Exploiting the panel nature of the data, I estimated an elasticity of property crime with respect to unemployment and underemployment of about 0.5. The results are robust to estimation in levels, logs, and annual first differences. As anticipated, the relationship between unemployment and violent crime is much closer to zero.

For robustness I confirmed the primary results using an instrumental variables approach. Instrumenting changes in regional employment with region-specific shocks, I again estimated a a significant relationship between property crime and the labour market: an extra thousand people employed in a region is associated with about 50 fewer property crimes per year. The coefficients in this specification are significant, of the same sign, and of the same order of magnitude as the OLS-based estimates.

The overall picture suggests that job creation generates the positive externality of lower crime. Conversely, higher unemployment leads to higher crime. Consistent with the existing literature, I find that this relationship holds for property crime as well sexual assault. A cohesive crime reduction policy could thus include labour market activation measures. The data used in this project end in mid-2014. Recent trends in employment have been very favourable, and Live Register figures continue to fall. These continued labour market improvements will provide even more variation for analysis, and future data releases will enable further tests of this relationship.

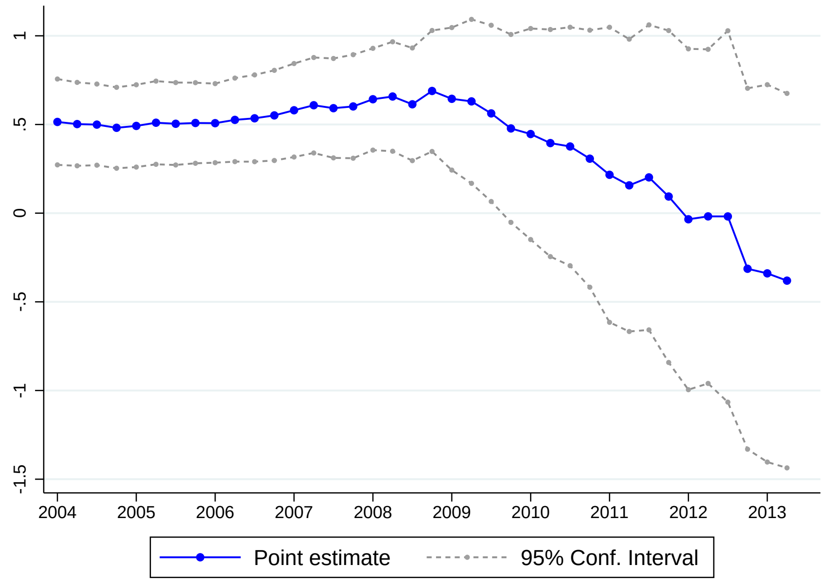
3.7 Appendix: Data Verification

In November 2014 the CSO announced it would delay publication of its quarterly crime statistics pending a full investigation into their reliability. This was prompted by a Garda Inspectorate report that had found considerable under-recording of crimes within the Pulse system. The CSO published its review of the administration of criminal statistics in June 2015. The primary finding was that as much as 18% of all recorded crime was not entered into the Pulse system and thus not received by CSO.

The estimation procedures above are robust to several types of misreporting. Principally, the statistical technique will not be biased if crime is consistently under- or over-reported in any particular region. Similarly, the results will not be biased if crime is misreported nationally at any particular time.

One method to test the veracity of the data is to conduct the same analysis over different time horizons, obtaining results for e.g. 2003-2008 and 2008–2013 separately.

Figure 3.1: Moving Average-Based Estimated Elasticities: Theft

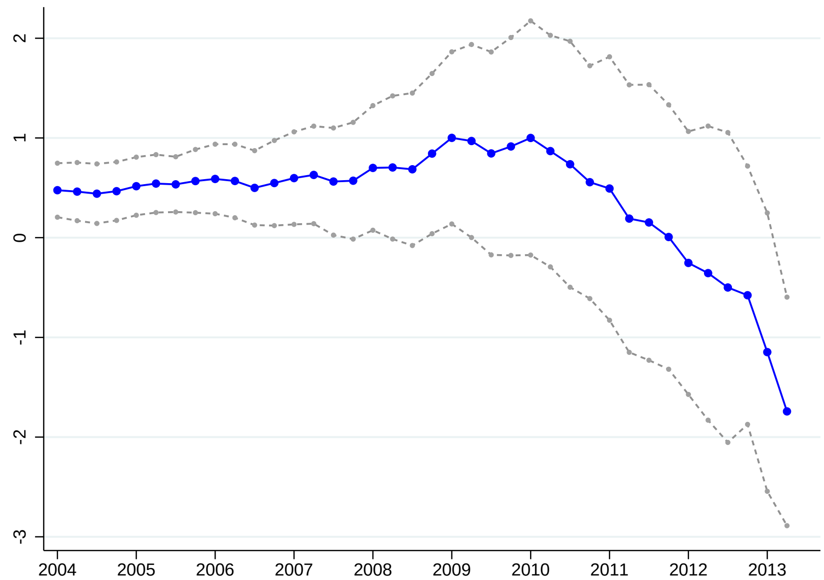


Notes: Point estimates and confidence interval for the elasticity of theft with respect to the Live Register. Results are based on a seven quarter moving window.

A more sophisticated method is to use a moving window and obtain point estimates for each time period. I employ this approach with a seven quarter moving window, i.e. by generating results for quarter t with data from that particular quarter and the three quarters ($t-3$, $t-2$, $t-1$) before and three quarters after ($t+1$, $t+2$, $t+3$). Figure 1 shows the estimated elasticity relating theft to the labour market and the corresponding 95% confidence interval using this approach.

There are three features to note in Figure 1. Firstly, both the point estimate and confidence interval are consistent in the first half the data, with an elasticity of 0.5 that is quite precisely estimated. Secondly, the point estimates begin a downward trend in 2009 and are negative from 2010 onwards. Thirdly, the confidence interval of these estimates increases approximately fourfold at about the same time. The increase in variance is large enough that the results lose statistical significance from mid-2009 onwards. Figure 2 shows a comparable figure for burglary.

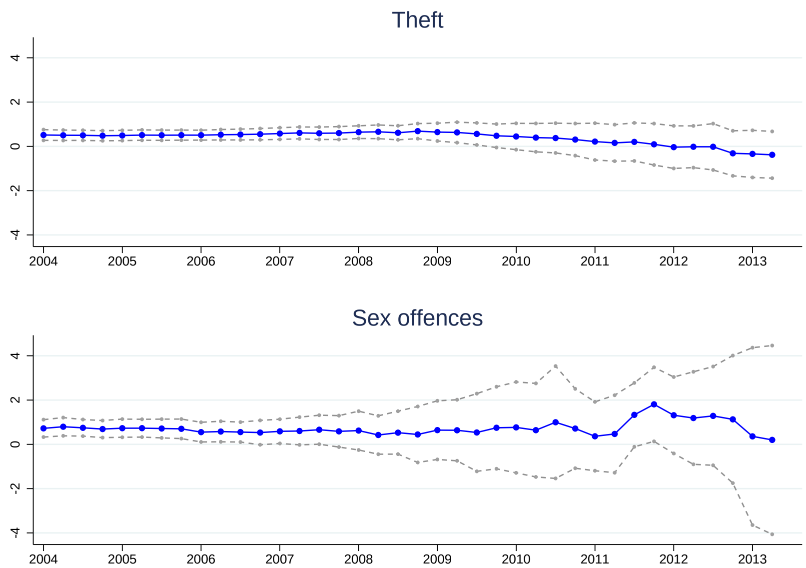
Figure 3.2: Moving Average-Based Estimated Elasticities: Burglary



The three findings emerge again. Firstly, there is a reasonably precisely estimated positive elasticity in the pre-2009 era; secondly, a downward trend thereafter; and thirdly, an enormous reduction in the precision of the estimates after the boom.

The pattern of increased variance from 2009 is particularly evident for sexual offences. The increased variance for thefts was immediately apparent in Figure 1. In Figure 3, I plot the results for sexual offences with the results for theft included for scale of reference.

Figure 3.3: Moving Average-Based Estimated Elasticities



Notes: Point estimates and confidence intervals for the elasticities for theft and all sexual offences. Notice that the increase in confidence intervals is much larger for sex offences than for theft.

Although not reproduced here, the pattern of substantially larger confidence intervals during the recession can be observed for other types of crime, and when analyzing relationships in levels rather than logs. It is not possible to determine from this analysis whether the decreased precision was caused by the increased economic volatility or from a decrease in data quality. Nonetheless, the evidence suggests a clear reduction in the precision of estimates and, if anything, a tendency for point estimates to decrease in the post-Celtic Tiger data. Both of these effects will serve to reduce the statistical significance of my results, tending to create attenuation bias in Tables 1–6. As any mis-reporting in the years immediately prior to the CSO's investigation will tend to dampen the magnitude and significance of my estimates, I conclude that there is no evidence that the results are driven by mis-reporting.

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