Essays on Gender and Political Economy

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Abstract

This thesis examines how gender mediates the relationship between individuals and public policy across three domains: local government spending, domestic abuse, and labour markets. The first two chapters draw on English local government data, while the third examines a policy reform in China; each employs a different strategy to investigate how political representation and policy design shape gendered outcomes.

The first chapter provides causal evidence that female political representation shifts local government spending priorities in England. Using a novel instrumental variable strategy based on the alphabetical ordering of candidates on ballot papers, the analysis shows that increasing the share of female councillors raises spending on children's social care and public health, with offsetting reductions in infrastructure. These effects are concentrated in councils where women hold about 30 per cent of seats, consistent with the critical mass hypothesis. The second chapter investigates female political representation and domestic abuse in England. Combining police-recorded data with confidential survey evidence from the Crime Survey for England and Wales, the analysis reveals that areas with higher female representation record more domestic abuse cases, yet show no increase in underlying prevalence. This divergence suggests that higher recording reflects improved reporting behaviour rather than worsening conditions, driven by victim-initiated police contact and higher public health spending. The third chapter examines the labour market consequences of China's Two-Child Policy using longitudinal data and a difference-in-differences design. Women newly eligible for a second child experienced significant wage penalties and reduced employment probability, with adverse effects disproportionately borne by younger, less-educated, and rural women.

Together, the three chapters demonstrate that gendered outcomes depend critically on the distribution of political power, the interpretation of administrative data, and the institutional environment in which policies operate. The findings underscore the importance of embedding gender-aware analysis into policy design and of combining multiple data sources when studying sensitive and underreported outcomes.

To all women, everywhere.

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Declaration

I declare that, except where explicit reference is made to the contribution of others, that this dissertation is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution.

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Introduction

The persistence of gender gaps across labour markets, political participation, and exposure to violence remains a central concern in economics. A substantial body of research documents systematic differences between women and men in earnings and employment (Goldin, 1990, 2014), in political representation and policy preferences (Chattopadhyay and Duflo, 2004; Edlund and Pande, 2002), and in experiences of violence and access to protection (Aizer, 2010; Anderberg et al., 2016; Iyer et al., 2012). These outcomes are shaped not only by individual characteristics or choices, but by how policies are designed, how public resources are allocated, and how social problems are recorded and addressed by the state. Understanding the role of public policy and political processes in shaping gendered outcomes is therefore central to the study of gender and political economy.

This thesis examines how gender mediates interactions between individuals and public policy across economic, social, and political domains. I approach these questions from three angles. First, how political representation shapes spending priorities. I test whether women in political office influence how public resources are allocated, exploiting features of English local elections and detailed financial data that allow causal identification of the effect of female representation on spending priorities. Second, how political representation relates to domestic abuse. I investigate how female political representation relates to experiences of domestic abuse, combining police-recorded data with confidential survey evidence to distinguish changes in prevalence from changes in reporting. Third, how population policy affects women in the labour market. I ask how policies designed to manage population growth can generate unintended consequences for women's labour market outcomes, drawing on a nationwide fertility policy reform in China that altered fertility options without directly targeting employment. The thesis demonstrates that understanding gendered outcomes requires rigorous identification strategies, careful attention to policy design and political representation, and cautious interpretation of administrative data.

The first chapter examines whether female political representation affects local government

spending priorities in England. English local councils have substantial discretion over how public resources are allocated across services such as social care, public health, transport, and environmental services. I ask whether an increase in the share of female councillors leads to systematic changes in how these budgets are distributed. To address endogeneity in political representation, I exploit quasi-random variation generated by the alphabetical ordering of candidates' surnames on ballot papers, which affects electoral outcomes but is plausibly unrelated to local policy preferences or candidate characteristics. Using a panel dataset I constructed combining local election results with detailed financial accounts, I find that higher female representation increases spending on care-related services, particularly children's social care and public health, with offsetting reductions in transport and some traditional services. These effects are concentrated in councils where women hold a sufficiently large share of seats, consistent with the idea that a critical mass of representation is required for policy influence to emerge. The chapter provides causal evidence that descriptive representation affects budgetary priorities in a high-income democracy without gender quotas, and develops a novel instrumental variable strategy that forms the methodological core of the thesis.

The second chapter studies domestic abuse in England, focusing on how female political representation relates to both police-recorded and self-reported outcomes. Domestic abuse is highly gendered and underreported, making it difficult to assess changes in prevalence using administrative data alone. I combine police-recorded domestic abuse data with individual-level survey data from the Crime Survey for England and Wales (CSEW), allowing a direct comparison between recorded cases and women's self-reported experiences. The analysis shows that areas with higher female representation in local councils record more domestic abuse cases, while confidential survey evidence suggests that women in these areas are less likely to experience abuse, particularly non-physical forms. A central contribution of this chapter is interpreting these patterns jointly. The results suggest that increases in recorded cases are not driven by higher underlying prevalence, but are consistent with changes in reporting behaviour and the visibility of abuse. I apply the ballot-order instrumental variable strategy developed in the first chapter and explore potential channels, including local public health spending and engagement with specialist support organisations. The chapter demonstrates the importance of combining administrative and survey data when studying sensitive and underreported outcomes.

The third chapter studies the labour market consequences of China's relaxation of its fam-

ily planning policy. In early 2014, China implemented the Two-Child Policy, relaxing the one-child family planning regulations that had been enforced since the 1980s. Whilst this pro-natalist policy has not substantially increased fertility rates, its broader socioeconomic implications, particularly for women, warrant thorough investigation. I examine how this reform affected women's labour market outcomes, focusing on those who became newly eligible to have an additional child. Using longitudinal data from the China Family Panel Studies (CFPS) and a difference-in-differences design that exploits the timing of the reform and eligibility rules, I compare eligible and ineligible women before and after the policy change. The results demonstrate that eligible women experienced statistically significant adverse labour market effects operating through two channels: a wage penalty and decreased employment probability. Further analysis reveals heterogeneous effects across socioeconomic dimensions, with women in prime childbearing ages, less-educated women, and rural eligible women disproportionately bearing the adverse consequences of the policy. The chapter shows how fertility policy can generate unintended labour market consequences for women, even when employment is not an explicit policy target, and highlights how population policy interacts with existing labour market structures in ways that can disadvantage women.

In sum, the thesis shows that gendered outcomes are shaped not only by individual circumstances, but also by how political power is distributed, how social problems are measured and interpreted, and how policies operate in practice. By studying public spending, domestic abuse, and labour markets using complementary empirical approaches, the thesis advances our understanding of how public policy and political representation affect women's lives, and the role of women's political voice. These findings carry three implications: the value of women's descriptive representation in shaping policy priorities, the need for research approaches that can separate real changes from changes in measurement, and the importance of designing policies that account for potential gendered impacts.

Chapter 1

Do Female Councillors Change Spending Priorities? Evidence from UK Local Elections

Abstract

This chapter examines whether female councillors influence local government spending priorities in England, a question central to understanding how gender representation shapes policy decisions. Using electoral data on about 95,000 candidates between 2010 and 2019, merged with financial accounts of 211 local authorities, I estimate the causal impact of female representation on spending patterns. To address endogeneity, I exploit the alphabetical ordering of candidates on ballots, which generates quasi-random variation in electoral success and thus in female representation. The results show that greater female representation increases spending on care-related services, with offsetting reductions in infrastructure and traditional services. These effects are concentrated in councils where women hold more than 30% of seats, consistent with critical mass theory. The findings provide new causal evidence that descriptive representation affects budgetary priorities, with critical mass thresholds determining when this influence materialises.

1.1 Introduction

Gender gaps persist across countries in labour market participation, occupational roles, caregiving responsibilities, and time use, continually reshaped by social norms, institutional constraints, and policy choices rather than being mere historical legacies (Goldin, 1990, 2014). For instance, in the UK, the gender pay gap declines slowly, a trend linked to women working part-time more often, concentrating in care-related sectors, and performing a larger share of unpaid care and household work (ONS, 2023*a*,*b*). These structural and social differences shape how women engage with the state and what they expect from it. Evidence shows that women tend to prioritise social protection, healthcare, and education, while men more often support physical infrastructure, defence, and other traditionally male-coded public goods (Edlund and Pande, 2002; Gottlieb et al., 2018; Persson et al., 2024). This raises a central question: when women hold political office, do they make different policy decisions, and under what conditions do those differences translate into measurable spending shifts?

This chapter examines whether female councillors influence local government spending priorities in England, particularly whether they increase investment in services relevant to women and families. Although evidence from various contexts shows that female politicians often prioritise different issues than men, whether these preferences translate into actual spending changes depends on the political environment, institutional structures, and fiscal constraints. Understanding when and how female representation affects policy outcomes requires analysing settings in which councillors have genuine budgetary discretion and where variation in representation can be credibly separated from other factors that influence both elections and spending decisions.

The English context offers several advantages for examining these questions. Most causal evidence on female representation comes from settings with gender quotas or proportional representation systems. England provides a candidate-centred, first-past-the-post system without formal gender quota.[1] This makes it possible to study how representation affects policy when it emerges from standard electoral competition rather than institutional requirements. English local councils also have substantial spending responsibilities and real discretion across a wide range of services, including social care, transport, and environmental and regulatory services. While national government provides much of the funding, councils de-

[1] In a first-past-the-post (FPTP) voting system, the candidate receiving the most votes wins, even if they do not obtain an absolute majority. See UK Parliament (n.d.).

cide how resources are distributed across these competing priorities. Councillors are elected at the ward level using first-past-the-post voting, and candidate names appear in alphabetical order on the ballot paper. This alphabetical ordering creates a source of quasi-random variation in female electoral success unique to this institutional setting. Understanding whether effects documented in quota and proportional representation contexts extend to plurality systems like England's has implications for a broader set of democracies.

To estimate the effect of female representation on spending outcomes, I construct a novel panel dataset combining local election results with detailed financial data from local authority budget returns. I begin by estimating reduced-form correlations using OLS. The main analysis then uses a two-stage least squares (2SLS) strategy, exploiting exogenous variation in electoral success created by alphabetical ballot ordering. In English local elections, candidates are listed strictly in alphabetical order by surname, and earlier-listed candidates receive systematically more votes (Webber et al., 2014). I construct the instrument as the share of top ballot positions occupied by female candidates, aggregated across wards to the council level. Since surnames are inherited and historically determined, alphabetical positioning is plausibly unrelated to candidate quality, policy preferences, or local demand for services. A potential concern is that changes in female candidate supply could coincide with shifts in spending preferences; I address this by showing results hold when restricting to councils with stable candidacy rates over time. The findings also remain consistent across alternative instrument constructions, strengthening confidence in the causal interpretation.

The findings show that increasing the share of female councillors leads to shifts in budget priorities toward care-related services, particularly children's social care. These effects are concentrated in core protection and support services, such as safeguarding and looked-after children. At the same time, spending on highways and transport decreases significantly, indicating a trade-off. However, not all traditional services experience offsetting reductions; cultural services and planning show no significant effects. Notably, councils with at least 30% female representation exhibit stronger and more consistent effects, pointing to the presence of a critical mass threshold. Below this threshold, female councillors' influence on spending appears limited, suggesting that institutional context – not just the presence of women – determines when descriptive representation translates into substantive policy change. These effects persist over time, reflecting longer-term shifts in spending priorities rather than temporary adjustments.

This study contributes to three strands of literature. First, it adds new causal evidence to research on female political representation, using local governments in a high-income setting with no formal gender quotas. Second, it complements studies on gendered preferences and public spending by showing how these preferences translate into observable fiscal outcomes at the local level. Third, it advances methodological approaches in political economy by developing a novel identification strategy based on ballot-order effects in English elections. This strategy addresses endogeneity concerns that have limited inference in previous work on representation and policy outcomes.

The remainder of the chapter proceeds as follows. Section 1.2 reviews the relevant literature on female political representation, gendered preferences, and identification strategies in political economy. Section 1.3 outlines the institutional background and describes the data. Section 1.4 presents the empirical strategy, including the instrumental variable design based on ballot position. Section 1.5 reports the results, exploring baseline estimates, heterogeneous effects, and instrument robustness checks. Section 1.6 presents additional robustness checks. Section 1.7 concludes.

1.2 Literature Review

1.2.1 Female Representation in Politics

Although there has been some progress in recent decades, women remain under-represented in political institutions at both national and local levels. In the UK, women were first permitted to stand for election in 1918, and since then, 693 women have been elected as Members of Parliament (MPs), making up only 12% of all individuals who have ever served (Buchanan et al., 2025). Despite these historic gains, progress remains slow. Following the general election in 2024, women make up 35% of all parliamentarians in the UK, including both the House of Commons and the House of Lords. At the global level, the share of women in national parliaments increased modestly from about 19% in 2010 to roughly 26.8% in 2024.[2] Representation at the local level in the UK has followed a similarly slow trend: women made up 30.6% of councillors in England in 2010 (Evans and Aston, 2011), increasing to around 36% across the UK by 2019. These figures remain far from numerical parity, highlighting a

[2]Figures are drawn from the Inter-Parliamentary Union (IPU) Parline database on women in national parliaments (Inter-Parliamentary Union, 2019).

persistent gender gap in political power.

The persistent underrepresentation of women in politics has raised important questions about when and how female representation leads to meaningful political change. Two concepts are central to this debate: *descriptive representation*, which refers to the number of women in elected positions, and *substantive representation*, which concerns the extent to which elected women promote policies that reflect women's interests (Phillips, 1995; Pitkin, 1967). Some studies find that although increasing the number of women in office doesn't always result in immediate policy shifts, it can broaden political agendas, influence institutional cultures, and inspire more women to enter politics (Beaman et al., 2012; Gilardi, 2015). However, scholars have also questioned whether simply adding more women to the office is enough to make a difference. For example, the "critical mass" theory suggests that women are more likely to exert real influence once they reach a certain threshold (Childs and Krook, 2006; Dahlerup, 1988; Kanter, 1977; Svaleryd, 2009). More recent research suggests that outcomes also depend on whether women hold leadership roles, or at least whether decision-making spaces include more than just men (Kern et al., 2025; Reinsberg et al., 2024). These perspectives point to the importance of context in shaping the relationship between women's presence in politics and actual policy outcomes, supporting the need for empirical studies that can capture this variation.

1.2.2 Gender Differences in Policy Priorities

Evidence from multiple contexts shows that women in elected office often prioritise different policy areas than their male counterparts. In the United States, women legislators are more likely to advance health, social services, family and childcare, and environmental issues. They also promote child-support enforcement and related family policy (Besley and Case, 2000; Jones, 1997; Little et al., 2001; Schwindt-Bayer, 2006). Cross-national research aligns with this pattern: higher female representation is associated with greater health and social-welfare spending and lower defence budgets, consistent with preference differences between women and men (Chen, 2010; Edlund and Pande, 2002; Hicks et al., 2016; Koch and Fulton, 2011; Lott et al., 1999). These findings suggest that effects are likely in care-related categories, while recognising that local context can influence how such priorities appear in budgets.

However, these patterns are not uniform across settings or policy areas. Survey evidence

from 33 African countries shows small average gender gaps but clear differences on particular goods: women place higher priority on clean water, while men place greater weight on roads, but where women's labour-force participation is higher, these gaps narrow (Gottlieb et al., 2018). Other studies highlight the role of socialisation and economic autonomy in shaping women's priorities (Di Landro, 2025; Persson et al., 2024). Intersectional evidence from India shows that the effects of female representation can vary by caste (Clots-Figueras, 2011). In sum, there is no single "women's agenda": priorities often tilt toward care-related services but depend on the policy domain and local conditions. This suggests that empirical tests should examine specific budget lines and allow for heterogeneity.

Turning to policy outcomes, research finds that when women win office, spending often shifts toward care-oriented goods. In India's village councils, randomly reserved leadership seats for women increased investment in drinking water and narrowed education gaps (Chattopadhyay and Duflo, 2004). Female politicians were more likely to invest in public-health infrastructure, which reduced infant mortality (Bhalotra and Clots-figueras, 2014). Girls' time on household chores fell and school attendance improved in villages led by women (Beaman et al., 2007, 2012). Constituencies that narrowly elected a woman subsequently grew faster than those that narrowly elected a man (Baskaran and Hessami, 2018). Other work links higher female representation to lower corruption in India and Brazil (Beaman et al., 2007; Brollo and Troiano, 2016; Chatterjee et al., 2025).

Research from developed countries examining the effects of female representation in local governments shows mixed findings. In Swedish municipalities, higher female representation shifted spending toward childcare, with stronger effects once women reached a threshold of around 30% representation (Svaleryd, 2009). In Japan, municipalities with more female councillors exhibited lower borrowing and reduced investment in public enterprises, consistent with greater fiscal caution (Suzuki and Avellaneda, 2018). In German councils, each additional woman sped up public childcare expansion by about 40% (Baskaran and Hessami, 2025). However, other studies find more limited effects. For example, research from the United States, Spain, and Norway reports little to no change in overall spending as a result of greater female representation (Bagues and Campa, 2021; Ferreira and Gyourko, 2014; Geys and Sørensen, 2019).

Among this body of work, the study by Baskaran and Hessami (2025) stands out for demonstrating a clear causal link between an additional female councillor and the provision of a

public good. While Bagues and Campa (2021) show that gender quotas in Spanish municipalities increased female representation without affecting policy outcomes, Baskaran and Hessami identify a meaningful effect using close mixed-gender races in open-list elections in Bavaria. My study contributes to this literature by examining the UK context, where local elections use a first-past-the-post (FPTP) voting system. Unlike the proportional representation (PR) system in Bavaria, where parties rank their candidates and seats are allocated based on vote share, the UK's local electoral system is candidate-centred. Voters choose individuals rather than party lists, and seats are awarded to the top vote-getters in each ward, irrespective of party. I exploit the ballot order effect as a source of exogenous variation in the election of female councillors. This approach not only adds to the limited causal evidence from electoral systems without quotas or proportional representation, but also helps assess the generalisability of existing findings across different institutional settings.

1.2.3 Identification Challenges and Strategies

Establishing a causal link between gender representation and policy outcomes faces a fundamental challenge of selection: areas that elect more women may differ systematically from those that elect fewer, and the type of women who win in such areas may also differ. This makes it hard to separate the effect of representation from underlying voter preferences, the candidate pool parties put forward, or local conditions. To address this challenge, researchers have used several identification strategies.

First, some studies exploit reservations or quotas that exogenously increase women's representation. In India, one third of Village Council head positions have been randomly reserved for a woman, creating as-good-as-random variation in who holds office (Chattopadhyay and Duflo, 2004). Cross-national studies often use difference-in-differences (DID) designs around when countries adopt quotas to track how policies change (Chen, 2010; Clayton and Zetterberg, 2018). At the municipal level in Europe, researchers have used discontinuities in quota rules. Bagues and Campa (2021) study Spanish population thresholds that trigger list requirements. Baltrunaite et al. (2019) examine Italy's double-preference voting with gender constraints. Coman and Shair-Rosenfield (2025) analyse Italy's 2012 one-third candidate rule using both regression discontinuity (RD) and difference-in-differences (DID). In these cases, institutional rules rather than voter preferences create the variation used for identification.

Second, close-election designs compare places where a candidate barely wins with those where they barely lose. The idea is that chance determines outcomes near the margin, creating quasi-random variation (Lee, 2008). Studies first check that this comparison is credible in their setting (e.g., Pettersson-Lidbom (2008)) and then estimate effects at that cutoff. Baskaran and Hessami (2025) use this logic for mixed-gender council contests. When elections happen at the ward level but outcomes are measured at the council level, researchers add up the share of seats narrowly won by women and use this as an instrument for the council's female representation (Clots-Figueras, 2011). For mayoral races, however, close-election studies often find little change in total budgets or spending. This suggests that council composition and mayoral gender may have different effects (Carozzi and Gago, 2023; Ferreira and Gyourko, 2014).

Third, ballot-order effects can provide another source of variation. When voters have limited information about candidates, they often use ballot position as a shortcut, a phenomenon sometimes called "donkey voting" (Orr, 2002). Evidence from multiple countries shows that this leads to real electoral advantages. First-position effects have been documented in Australia (King and Leigh, 2009; Orr, 2002), Colombia (Gulzar et al., 2022), Korea (Jun and Min, 2017), Spain (Lijphart and Pintor, 1988), and the United States (Edwards, 2015; Koppell and Steen, 2004; Meredith and Salant, 2013). These consistent findings across different electoral systems suggest that ballot order creates quasi-random variation in who gets elected.

In England, the law requires candidates to be listed alphabetically by surname, regardless of party or incumbency. Webber et al. (2014) find strong evidence of positional advantage in English local elections, estimating that roughly 2,050 councillors won their seats because of ballot position. Since surnames are inherited and historically determined, alphabetical ordering is unlikely to be related to candidate quality, policy preferences, or campaign strength. This makes it suitable as an instrument. To the best of my knowledge, no one has used this variation to study how female representation affects local spending. In this thesis, I construct an instrumental variable based on how many female candidates appear in top ballot positions. Section 1.3 describes the institutional context and data, followed by full details of the instrument construction and validation in Section 1.4.

1.3 Institutional Context and Data

1.3.1 English Local Government System

Local government in England comprises five main types of principal authorities: unitary authorities, metropolitan districts, and London boroughs (single-tier); and county councils and district councils (in two-tier areas). As of 1 April 2023, there are 317 local authorities in England (Sandford, 2022). Responsibilities are tiered. Upper-tier and single-tier councils hold statutory duties for education, social care, highways and transport, planning services, and public health (since 2013). District councils do not run social care; they are typically responsible for waste collection, recycling, billing and collecting council tax. Single-tier councils provide the full set of local services.[3] Figure 1.1 illustrates the administrative hierarchy in England.

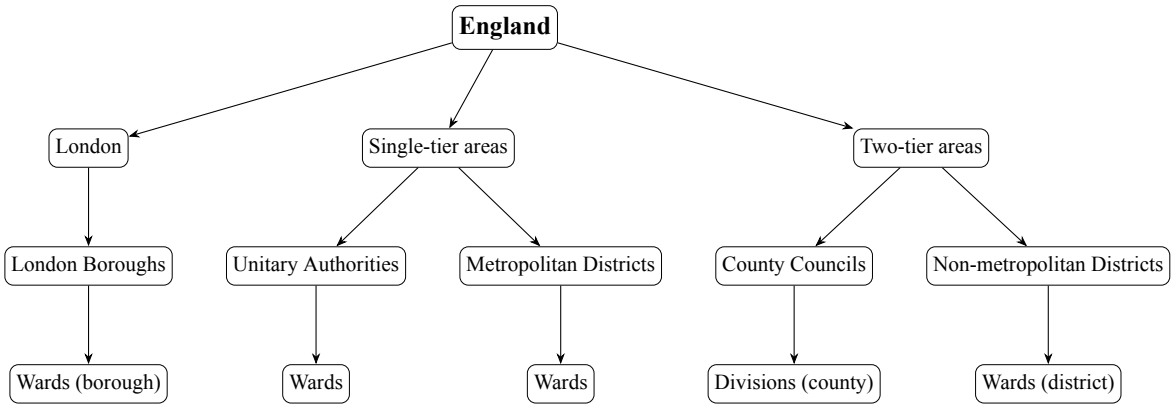

Figure 1.1: Administrative Units in England

Notes: Principal authorities comprise London boroughs, unitary authorities, metropolitan districts, and non-metropolitan districts (within two-tier counties). Councillors are elected by wards (and county divisions for county councils). Combined authorities, police/fire bodies, and the City of London and Isles of Scilly are omitted for brevity. *Source:* Author's illustration based on ONS administrative geographies and House of Commons Library briefings (Sandford, 2022).

Councils are funded from council tax, retained business rates, fees and charges, and central government grants.[4] And they are also subject to numerous statutory duties. For instance, councils must provide children's services under the *Children Act 1989*, adult social care under the *Care Act 2014*, and household waste collection under the *Environmental Protection Act 1990*. Whilst these duties constrain spending, councils retain discretion over service levels and modes of delivery – for example, determining the balance between preventive and crisis interventions in social care, or deciding the frequency and scope of waste collection services.

[3] See UK Government (n.d.*b*) for details on how council works.

[4] See Institute for Government (n.d.) for an overview of local government funding in England.

Most councils are run by a majority group, but the annual budget is adopted by the full council, following proposals from the executive. All councillors therefore vote on the budget.[5] In this governance set-up, the composition of the *whole* council – not only the executive – matters for budget choices.

Since 2010, English local government has operated under sustained austerity. Real-terms service spending fell across the decade, and a broader measure of children's social care that includes Sure Start and youth services declined by about 9% between 2010-11 and 2019-20 (Ogden and Phillips, 2024). Against this aggregate decline, there is substantial heterogeneity across councils. The main scope for change is how shrinking budgets are allocated across services. Therefore, I focus on the *composition* of spending: conditional on common austerity trends, I examine whether councils with different gender compositions allocate their constrained budgets differently across service categories.

To study spending composition, I use the UK government's Local Authority Revenue Expenditure and Financing (RO) returns, which report outturn expenditure: the actual amounts spent during the financial year rather than budgeted allocations.[6] The analysis uses service-level *net current expenditure* (NCE), which deducts income from fees and charges. NCE is preferred over total revenue expenditure because, according to Ministry of Housing, Communities & Local Government (MHCLG) guidance, it reflects "the cost of running local authority services within the financial year," including staffing and operational costs. Revenue expenditure also covers debt repayment, capital financing, and other non-operational items (MHCLG, 2019), which are largely outside councillors' direct control in any given year.

The timing of local elections aligns closely with the budget cycle, enabling newly elected councillors to influence spending in the same financial year. English local elections are held on the first Thursday in May, while the local authority financial year runs from 1 April to 31 March.[7] Although the annual budget is typically set before the May elections, councils retain discretion over in-year spending adjustments and service delivery decisions throughout the financial year. Using outturn expenditure data, rather than budgeted figures, is therefore

[5] See the Explanatory Notes to the *Local Government Act 2000*, para. 30 (UK Parliament, 2000); see also Chartered Institute of Public Finance and Accountancy (2021) for guidance on local authority financial governance and budget approval.

[6] Data are drawn from the local authority revenue expenditure and financing datasets (Department for Levelling Up, Housing and Communities, 2025*b*).

[7] See Department for Levelling Up, Housing and Communities (2024) for details on the local authority financial year and budget reporting framework.

critical for capturing the actual spending decisions made by councillors during their term, reflecting real policy priorities rather than initial budget proposals. Moreover, the composition of the council directly affects budget priorities in subsequent years, as the full council votes on budget proposals each year regardless of executive arrangements.

Table 1.1: Summary Statistics (Analysis Sample, 2010–2019)

	Mean	Std Dev	Min	Max	N
Panel A: Dependent Variables (% of Total Expenditure)					
Children's Social Care Services	6.65	6.12	0.00	25.50	2300
Adult Social Care Services	12.04	10.30	0.00	33.69	2282
Public Health Services	2.63	2.41	0.00	13.27	1784
Education Services	27.32	23.93	0.00	66.08	2557
Highways and Transport Services	3.33	1.87	0.00	16.68	1742
Cultural Services	8.79	8.46	0.01	45.69	2543
Environmental Services	17.88	15.69	1.12	83.66	2557
Planning Services	6.26	6.62	0.00	37.97	2478
Panel B: Key Variables					
% Female Elected	32.35	7.64	0.00	60.87	2299
Total Expenditure (log)	11.47	1.80	8.52	14.57	2557
Total Services Expenditure (£'000)	292853.76	345552.48	5026	2132014	2557
Council Size (# of councillors)	53.33	15.60	3	126	2299
Population (log 1000s)	5.23	0.79	0.80	7.37	2590
Sex Ratio	0.97	0.03	0.90	1.26	2594
Panel C: Political Control					
Conservative Control	0.62	0.48	0	1	2045
Labour Control	0.20	0.40	0	1	2045
Liberal Democrat Control	0.03	0.17	0	1	2045
No Overall Control	0.13	0.34	0	1	2045
Seat Share of Largest Party	67.10	15.86	15.79	100	2045

Notes. All dependent variables in Panel A are expressed as percentages of total net current expenditure. The number of observations varies across service categories due to data availability and missing values in local authority financial records.

The RO returns classify expenditure into the following categories: children's social care, adult social care, public health, education, highways and transport, cultural services, environmental services, and planning and development.[8] To illustrate, children's social care covers services such as child protection and looked-after children; highways and transport includes items such as road maintenance and public transport subsidies; and environmental services encompasses activities such as waste collection and street cleansing. The dependent variables are the shares of total NCE allocated to each category (per cent of total). Panel A of Table 1.1 reports summary statistics of outcome variables in this study.

[8]Categories follow the service lines used in the Revenue Outturn (RO) returns; public health expenditure is included from 2013-14 following the transfer of public health responsibilities to local authorities (Department of Health, 2012).

1.3.2 Local Elections

English local councillors are elected using the first-past-the-post (FPTP) system from wards (or county divisions for county councils, i.e., the lowest level shown in Figure 1.1).[9] Election cycles differ by authority. County councils and London boroughs use whole-council elections, in which all seats are contested once every four years. Metropolitan districts mainly elect by thirds, where one third of seats is up for election in each of three consecutive years, with no scheduled elections in the fourth year; under this staggered pattern, individual councillors still serve four-year terms. District councils and unitary authorities either hold whole-council elections or elect by thirds, and a small number of districts elect by halves, with half the seats up for election every two years. Boundary reviews can also trigger a one-off whole-council election even where the usual cycle is by thirds.[10]

A distinctive feature of English local elections is the statutory requirement for alphabetical ballot ordering. The Representation of the People Act 1983 mandates that candidates appear strictly in surname order, regardless of party affiliation or incumbency (UK Parliament, 1983). Earlier-listed candidates systematically receive more votes, particularly in low-information settings (Freeder et al., 2025; Koppell and Steen, 2004; Miller and Krosnick, 1998; Webber et al., 2014). This exogenous ordering supports my empirical strategy, which exploits the number of women appearing in advantageous positions as a source of identifying variation (see Section 1.4). A sample ballot paper is reproduced in Appendix Figure A.1 to illustrate the format prescribed in legislation.[11]

For this study, I collected election data from the Local Elections Archive Project (LEAP), which covers all English local elections from 2002 to 2023.[12] I restrict the sample to whole-council elections to ensure complete coverage of council composition. The dataset includes over 170,000 candidacies across 216 local authorities, recording both successful and unsuccessful candidates with their names, party affiliations, vote totals, and ward-level results. Because the election records do not report candidate gender, I infer it from first names using the R package `gender`.[13] This generates an estimate $P(female|firstname)$, where 0

[9] See the House of Commons Library briefing on voting systems in the UK (House of Commons Library, 2025).

[10] See Department for Levelling Up, Housing and Communities (2025*a*) for details on local authority election cycles in England.

[11] See the *Representation of the People (Ballot Paper) Regulations 2015* (UK Government, 2015).

[12] Election results are compiled from the Local Elections Archive Project (Teale, n.d.).

[13] The package draws on historical datasets from the U.S. Social Security Administration, the U.S. Census Bureau (via IPUMS USA), and the North Atlantic Population Project to estimate, for specified countries and

denotes male and 1 denotes female. Ambiguous cases with probabilities between 0.1 and 0.9, about 5-6% of names are reviewed manually using council websites and public social media (LinkedIn, Facebook, Twitter). To avoid bias from partial identification within a ward, I exclude the entire ward whenever any candidate's gender cannot be identified. This rule applies mainly to records with only first-name initials (fewer than 2% candidates).

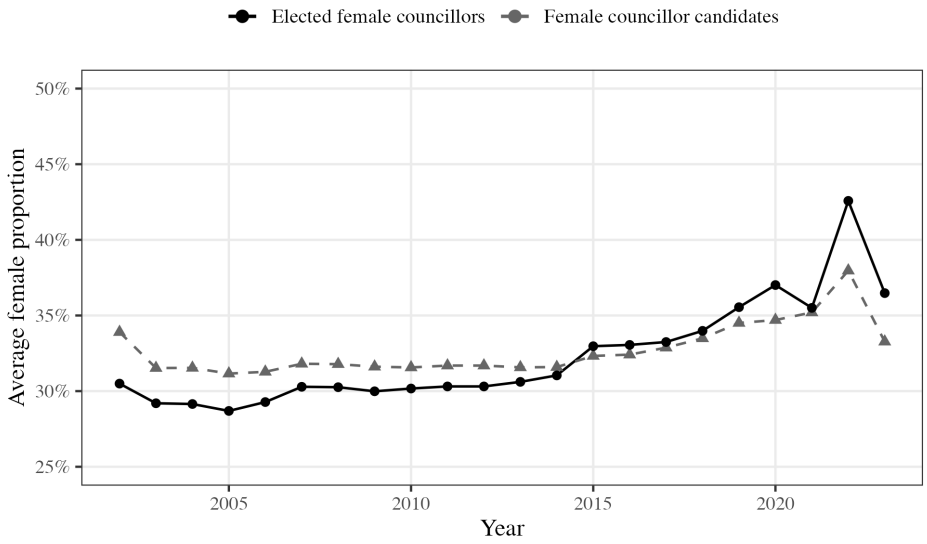

Figure 1.2: Female Representation in Local Elections Over Time

Figure 1.2 plots the share of female candidates and elected councillors in English local elections over time. Female representation was stable at around 30% until the mid-2010s, after which it began to rise steadily. By the early 2020s, the proportion of women among elected councillors exceeded 35%, with a temporary spike above 40% in 2022. These descriptive trends mirror national debates on gender balance in politics and the gradual diffusion of party-level gender initiatives at the local level. These figures also highlight that, despite recent gains, women remain underrepresented in local government relative to their share of the population.

Table 1.1 provides further descriptive statistics for the councils in my dataset. The average council has 53 councillors and serves a population of about 175,000 residents ($exp(5.23) \times 1,000$). Total annual service expenditure averages £292 million, with wide variation across authorities, reflecting differences in population size, geographic scale, and service responsibilities. Expenditure is concentrated in a few categories: education (26% of the total), adult and children's social care (19%), and environmental services (19%). Other functions such as highways, cultural services, and planning account for smaller but still meaningful shares.

periods, the probability that a given first name is female (Mullen, 2021).

These spending patterns underscore the fiscal significance of local government, with councils responsible for delivering services that directly affect residents' daily lives.

After merging with finance data and applying sample restrictions, the analysis sample covers 211 local authorities across all nine English regions from 2010 to 2019. The restriction to this period avoids potential distortions from the COVID-19 years, when both elections and local government finances were atypical. On average, 31.8% of elected councillors in the sample are female (Table 1.1, Panel B). This level of representation is broadly consistent with the descriptive trend in Figure 1.2 but reflects the restricted 2010-2019 sample that forms the basis of the empirical analysis. Some parts of the analysis further use subsamples defined by local election years, which are introduced in Section 1.4.

1.4 Empirical Strategy

1.4.1 Baseline Specification

I begin by establishing the baseline relationship between female representation and local spending priorities using ordinary least squares (OLS) estimation. This approach allows me to examine whether there is a correlation between the share of female councillors and how councils allocate their budgets. While these estimates cannot establish causality, they provide a useful foundation for understanding the basic patterns in the data and serve as a point of comparison for the causal estimates I develop later using instrumental variables. The OLS specification takes the form:

$$SpendingCategory_{it} = \alpha_0 + \alpha_1 FemaleCllr_{it} + \alpha_2 Controls_{it} + \gamma_t + \delta_r + \theta_p + \varepsilon_{it} \tag{1.1}$$

where $SpendingCategory_{it}$ represents spending on category k as a percentage of total service expenditure for local authority i in year t. $FemaleCllr_{it}$ measures the percentage of elected councillors who are female. $Controls_{it}$ includes the logarithm of total service expenditure, total number of elected councillors, logarithm of population, sex ratio in local authority area, and seat share of the largest political party.

The specification includes three sets of fixed effects. Year fixed effects (γ_t) absorb com-

mon temporal shocks such as national policy changes or economic conditions affecting all councils. Regional fixed effects (δ_r) account for systematic geographic differences in spending patterns across England's nine government office regions. Crucially, I include political party control fixed effects (θ_p), which account for systematic differences in spending priorities across Conservative-controlled, Labour-controlled, Liberal Democrat-controlled, other party-controlled councils, and councils with no overall control. These party controls are important because parties differ in both their propensity to field female candidates and their broader policy manifestos, making it essential to compare councils within the same party control category rather than across them.

Notably, I do not include local authority fixed effects in this specification. The analytical sample spans 2010-2019, a period during which most local authorities experience only one or two elections due to the typical four-year electoral cycle. This limited within-authority variation would render local authority fixed effects impractical and potentially problematic for identification. Instead, the cross-sectional variation across authorities provides the primary source of identification, with standard errors clustered at the local authority level to account for potential serial correlation within authorities over time.

1.4.2 Identification Challenge

The primary empirical challenge in estimating the causal effect of female political representation on local government spending priorities lies in the potential endogeneity of female councillor representation. Female representation is not randomly assigned across local authorities. Instead, the areas that elect more women may differ systematically from those that elect fewer women in ways that also affect spending decisions.

Several sources of bias could confound the correlations observed in the OLS estimates. First, omitted variable bias may arise if unobserved local characteristics simultaneously influence both the propensity to elect female councillors and preferences over public service provision. For instance, areas with more progressive social attitudes might both favour female candidates and independently support higher spending on social services, creating a spurious positive correlation between female representation and care spending. Similarly, urban areas tend to elect more women and face different service demands than rural areas, such as greater needs for social care relative to highways infrastructure. These confounding factors mean that any observed relationship between female representation and spending could re-

flect underlying differences in local preferences rather than the causal effect of having more women in office.

Second, reverse causality presents a concern if existing spending patterns influence the political landscape. Areas already investing heavily in care services may attract female candidates who wish to protect or expand these policies, or may have voters who specifically seek out female candidates perceived as supportive of such services. In this scenario, spending priorities would drive female representation rather than the reverse.

These endogeneity concerns mean that OLS estimates are potentially biased and inconsistent. If progressive areas both elect more women and spend more on care independently of female representation, OLS would overstate the causal effect of having more women in office. Without a credible identification strategy, we cannot determine whether female representation causally affects spending priorities or whether the observed correlations merely reflect selection and sorting patterns.

1.4.3 Instrumental Variable Approach

To address these challenges, I employ an instrumental variables (IV) approach based on alphabetical ballot ordering in English local elections. While close-election designs offer an alternative identification strategy (Baskaran and Hessami, 2025), they require sufficient numbers of competitive mixed-gender races with narrow margins. In the English local government context, many wards are not very closely contested, and aggregating ward-level close elections to the council level can introduce additional complications when the outcome of interest is council-wide spending. The ballot-order approach, by contrast, generates variation across all wards regardless of competitiveness, making it better suited to this setting.

This identification strategy leverages the institutional feature that candidate names are listed in alphabetical order by surname, independent of party affiliation or individual characteristics, creating quasi-random variation in ballot position. The empirical literature has documented substantial ballot position effects in various electoral contexts, with candidates listed earlier on ballots typically receiving more votes due to voter behaviour patterns. These effects tend to be stronger in local elections, where limited candidate information leads voters to rely on cues like alphabetical position when casting their votes (Miller and Krosnick, 1998).

The alphabetical ordering system in English local elections thus creates an exogenous source of variation in female electoral success. Because candidate surnames are mechanically ordered, the share of top ballot positions occupied by female candidates varies quasi-randomly across wards and years. I exploit this quasi-random variation to construct instrumental variables for female representation. The logic is as follows: if ballot position matters for electoral outcomes, as prior research suggests, then wards where female candidates happen to occupy top positions should, on average, elect more women to the council. This approach would allow me to isolate variation in female representation that is plausibly unrelated to candidate ideology, local policy preferences, or the candidate selection process. I construct and test whether this instrument is empirically relevant in the first-stage analysis presented below.

Instrument Construction

Main Approach. The construction of my primary instrumental variable proceeds through several steps, aggregating ward-level female alphabetical advantages to the local authority level. First, for each ward-year election, I define "advantageous" ballot positions as the top N positions, where N equals the number of council seats contested in that ward. This definition reflects the institutional reality that voters typically select as many candidates as there are seats available, making early ballot positions systematically advantageous.[14] For example, in a ward electing three councillors, the top three alphabetical positions constitute advantageous positions; in single-member wards, only the first position is advantageous. This approach differs from using only the top position and better reflects the multi-member structure of English local elections, where 69% of wards elect two or three councillors, and 31% elect just one.

Second, I calculate the proportion of these advantageous positions occupied by female candidates in each ward-year:

$$FemaleAdv_{wt} = \frac{\text{Female candidates in advantageous positions}_{wt}}{\text{Total advantageous positions}_{wt}},$$

where w refers electoral wards and t refers years.

Then, I aggregate these ward-level measures to the local authority level using simple aver-

[14] Rule 47(5) of the *Local Elections (Parishes and Communities) Rules 2006* (UK Government, 2006).

ages:

$$AlphaAdv_{jt} = \frac{1}{W_{it}} \sum_{w=1}^{W_{it}} FemaleAdv_{wt},$$

where W_{it} represents the number of wards in local authority i during year t.

This aggregation yields my primary instrumental variable: the average share of advantageous ballot positions occupied by female candidates within each local authority-year (hereafter 'female alphabetical advantage', denoted $AlphaAdv$). The instrument captures plausibly exogenous variation in female representation arising from the alphabetical ordering of surnames, which influences electoral outcomes when female candidates happen to appear early on the ballot. Because alphabetical position is exogenous to the individual candidate, and unrelated to candidate quality, policy preferences, or campaign strength, the instrument is unlikely to be correlated with unobserved determinants of spending. It isolates variation in female representation that is driven by ballot design rather than by voter ideology, local political conditions, or strategic candidate placement. This provides a credible basis for identifying the causal effect of female representation on local spending outcomes.

Alternative Specifications. To assess the robustness of the main instrument, I construct several alternative specifications that vary along two dimensions: the definition of advantageous positions and the aggregation weights. First, I account for the possibility that voters may not cast all available votes in multi-member wards. While each voter in a ward electing three councillors may cast up to three votes, they may also cast fewer (e.g., one or two votes), and such ballots remain valid.[15] If voters systematically cast fewer votes than the number of seats available, the ballot-order advantage may be concentrated in fewer positions. To test this, I construct variants of the instrument using fixed position thresholds: defining advantageous positions as only the top 1, top 2, or top 3 positions, regardless of the number of seats in the ward.

Second, rather than relying solely on unweighted averages when aggregating to the local authority level, I create instruments that weight ward-level measures by (i) the number of seats, (ii) the total number of candidates, or (iii) the number of female candidates per ward. These alternative weighting schemes account for potential heterogeneity in electoral competitive-

[15] See the Electoral Commission's *Combined Guidance for Returning Officers at Local Government and Police and Crime Commissioner Elections in England*, section "Doubtful ballot papers" (Electoral Commission, 2024).

ness and ward size, ensuring that larger or more contested wards do not disproportionately influence the aggregate measure.

As an additional robustness check, I construct an alternative instrumental variable that operates directly at the local authority (LA) level, rather than aggregating ward-level measures. Specifically, I rank all candidates within each local authority-year alphabetically by surname (and first name to break ties), regardless of the ward in which they stand. I then define advantageous positions as the top N ranks, where N equals the total number of councillors elected across all wards in that authority-year. The LA-level instrument is then calculated as:

$$AlphaAdv_{it}^{LA} = \frac{\text{Female candidates in top } N \text{ LA-wide ranks}_{it}}{N_{it}},$$

where N_{it} is the total number of councillors elected in local authority i in year t.

This specification tests whether female candidates with early-alphabet surnames across the entire authority benefit from ballot-order effects, independent of how they are distributed across wards. While this measure abstracts from the ward-level electoral structure where ballot-order effects actually occur, it provides a complementary test: if results hold using this alternative source of variation, it strengthens confidence that the main findings are not driven by ward-level confounders. This specification is presented as a robustness check alongside the main ward-aggregated instrument.

By varying both the definition of advantageous positions and the level of aggregation, these alternative specifications provide robustness tests of whether the results depend on specific modelling choices or reflect the causal effect of female representation on spending priorities. Figure 1.3 displays the distributions of all instrumental variables constructed using these alternative specifications. The distributions are remarkably consistent across weighting schemes and position definitions, with means clustering around 0.33 and standard deviations between 0.07-0.14 (detailed summary statistics in Appendix Table A.1). This consistency demonstrates that the instruments exhibit meaningful variation across local authorities while maintaining similar distributional properties regardless of construction method. The LA-wide instrument, which ranks candidates across the entire authority rather than aggregating ward-level measures, displays a distribution comparable to the ward-aggregated measures, supporting its use as a complementary specification.

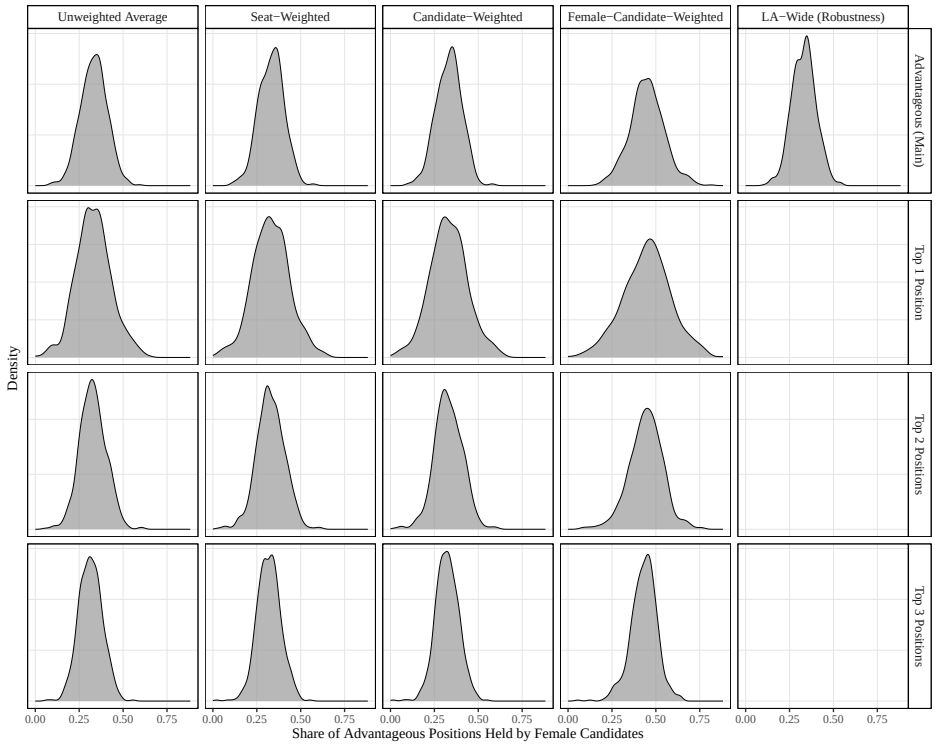

Figure 1.3: Distribution of Instrumental Variables

Notes: Density distributions of instrumental variables measuring the share of advantageous ballot positions held by female candidates. Columns vary by weighting scheme (unweighted average, seat-weighted, candidate-weighted, female-candidate weighted, and LA-wide ranking). Rows vary by position definition (advantageous positions where N equals seats, or fixed top 1, 2, or 3 positions). Sample: 211 local authorities, election years 2010-2019.

Instrument Validity

The validity of the instrumental variable approach relies on two key assumptions: instrument relevance and exclusion restriction. The relevance condition requires that alphabetical ballot positioning significantly affects female representation. Prior research documents substantial ballot position effects in elections (Freeder et al., 2025; Miller and Krosnick, 1998). If female candidates are neither systematically advantaged nor disadvantaged by the alphabetical distribution of surnames, then quasi-random variation in the gender composition of advantageous positions should predict variation in female electoral success. This assumption is directly testable through first-stage F-statistics.

The conventional rule-of-thumb that F-statistics above 10 indicate sufficient instrument strength, Staiger and Stock (1997); Stock and Yogo (2005) provide a useful benchmark. However, recent research has highlighted important limitations of this threshold. Lee et al. (2022) show that standard 2SLS inference can remain distorted even when F-statistics exceed conventional thresholds, while Keane and Neal (2022) demonstrate that 2SLS exhibits power asymmetry,

particularly limited power to detect true negative effects when OLS bias is positive, which can persist even with relatively strong instruments. To complement standard 2SLS inference, I also report Anderson-Rubin (AR) test results where appropriate. The AR test provides valid inference regardless of instrument strength and is optimal for exactly identified models (Keane and Neal, 2022).

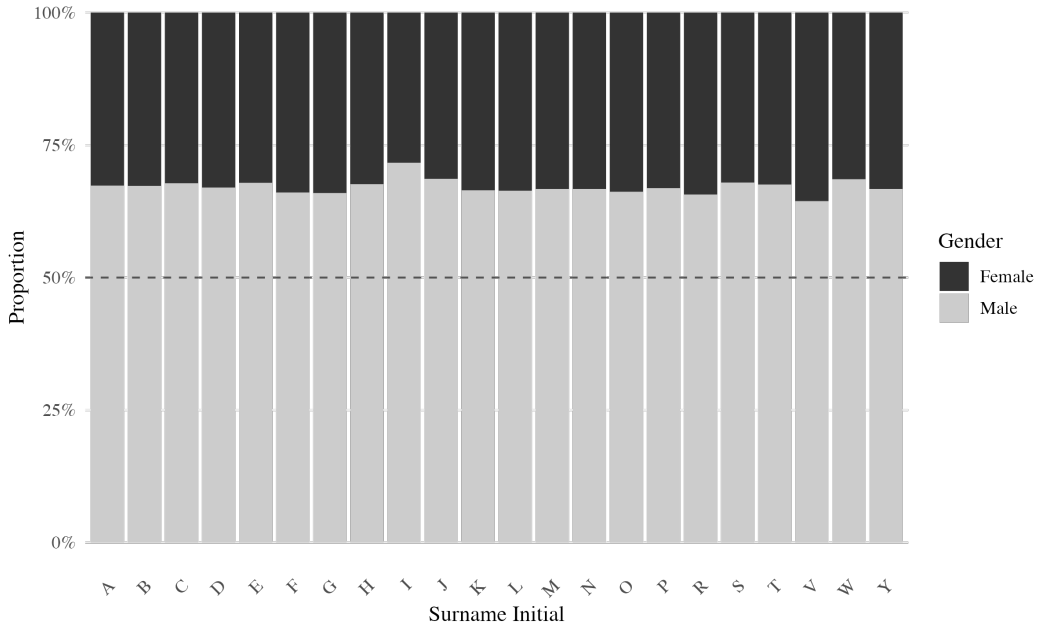

Figure 1.4: Gender Composition by Surname Initial

Notes: Gender composition by surname initial for all candidates ($N = 169,555$) from 2002 to 2023, excluding surname initials with fewer than 500 candidates. Female candidates constitute 32.8% of the sample, and this proportion is highly stable across surname initials (mean absolute deviation = 0.98 percentage points). These results support the assumption that alphabetical ballot order is uncorrelated with candidate gender.

The exclusion restriction requires that alphabetical ballot positioning affects spending priorities only through its impact on female representation, not through other channels. This assumption is inherently untestable but can be justified based on economic reasoning and institutional knowledge. The plausibility of the exclusion restriction rests on the argument that surname alphabetical ordering is unlikely to be systematically correlated with spending preferences or other characteristics that affect policy outcomes. Figure 1.4 provides support for this assumption by displaying the gender composition across surname initials for all candidates. The figure demonstrates that gender proportions are consistent across the alphabet, indicating that alphabetical positioning is randomly distributed with respect to candidate gender. This pattern supports the core identifying assumption that surnames, being largely inherited and historically determined, are plausibly exogenous to contemporary political preferences and candidate characteristics.

To further assess the validity of the instrument, I compare pre-determined characteristics

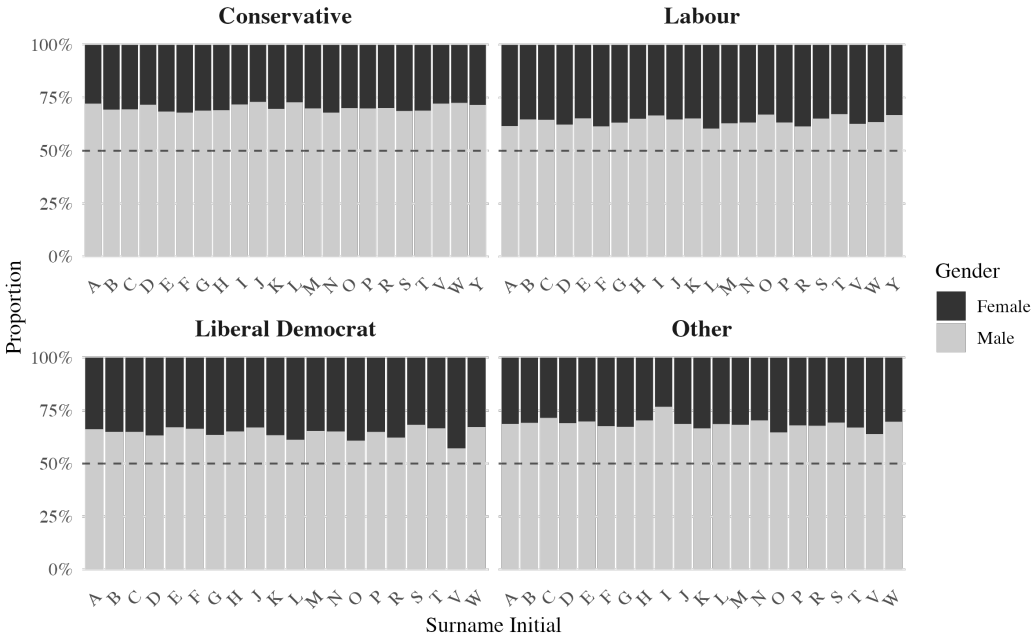

Figure 1.5: Gender Composition by Surname Initial and Party Group

Notes: Includes 51,070 Conservative, 42,590 Labour, 35,184 Liberal Democrat, and 41,686 Other candidates from 2002–2023. To reduce visual noise, surname initials occurring fewer than 200 times per party group were excluded: Q, U, X, Z (Conservative); Q, U, Z (Labour); I, Q, U, Y, Z (Liberal Democrat); Q, U, X, Y, Z (Other). Proportions are calculated within party groups by surname initial. The dashed line marks gender parity (50%).

across local authorities with varying levels of female alphabetical advantage. The balance tests, reported in Appendix Table A.2, show no statistically significant differences in structural characteristics such as total expenditure, council size, or population. However, some differences emerge in political party control. These political party differences reflect an important underlying feature of English local politics: parties vary in their propensity to field female candidates. Figure 1.5 illustrates this heterogeneity, showing that Labour generally fields more female candidates than Conservatives. Importantly, this party-level variation does not compromise the identification strategy. All regression specifications include political party control fixed effects, which absorb systematic differences in spending preferences across party lines. Additionally, I include the seat share of the largest party as a control variable in all specifications to control for the strength of party control beyond the binary fixed effects.

Additionally, Section 1.5.5 provides further evidence supporting instrument validity. I conduct subsample analysis restricted to local authorities with relatively stable female candidacy rates, ensuring that the results are not driven by changes in the number of female candidates over time. These additional checks reinforce the credibility of the instrument for identifying the causal effect of female representation on local spending priorities.

1.4.4 Two-Stage Least Squares Specification

Having established the instrument, I now present the formal two-stage least squares (2SLS) model that identifies the causal effect of female representation on spending priorities. The first stage estimates the relationship between female alphabetical advantage and female representation:

$$FemaleCllr_{it} = \pi_0 + \pi_1 AlphaAdv_{it} + \pi_2 Controls_{it} + \gamma_t + \delta_r + \theta_p + \nu_{it} \tag{1.2}$$

The second stage estimates the causal effect of female representation on spending priorities using predicted values from the first stage:

$$SpendingCategory_{it} = \beta_0 + \beta_1 \widehat{FemaleCllr}_{it} + \beta_2 Controls_{it} + \gamma_t + \delta_r + \theta_p + \varepsilon_{it} \tag{1.3}$$

where $\widehat{FemaleCllr}_{it}$ represents the fitted values from the first stage regression, and the controls and fixed effects structure mirrors that of the OLS specification in equation 1.1. The coefficient of primary interest is β_1, which measures the causal effect of increasing female councillor representation by one percentage point on the share of total expenditure allocated to each spending category. Given that spending shares must sum to 100%, negative effects on some categories are mechanically necessary when others increase, making the pattern of effects across categories as important as individual coefficients. Standard errors are clustered at the local authority level to account for potential serial correlation within councils over time.

1.5 Results

1.5.1 First-Stage Results

Before examining the effects on spending priorities, I establish the relevance of the female alphabetical advantage instrument for predicting female representation. The first-stage results, reported in Tables 1.2 and 1.4, demonstrate a consistently strong relationship between female alphabetical advantage and female councillor representation across all specifications. The first-stage coefficients range from 28.071 to 28.529, all statistically significant at the 1% level. The associated F-statistics consistently exceed 37, with most above 40, well above conventional thresholds for instrument relevance (Staiger and Stock, 1997; Stock and Yogo,

2005). These strong first-stage relationships confirm that alphabetical ballot positioning significantly affects female electoral success in English local elections.

Following Keane and Neal (2022), I also report Anderson-Rubin (AR) test results, which provide valid inference regardless of instrument strength. For children's social care, the AR test yields a Chi-square statistic of 9.79 ($p = 0.002$), rejecting the null hypothesis that female representation has no effect on spending. Similarly, for public health, the AR Chi-square statistic is 4.30 ($p = 0.038$), also rejecting the null. These results confirm that the IV estimates are robust to potential weak instrument concerns.

A one standard deviation increase in female alphabetical advantage (SD = 0.076) translates to roughly 2.2 percentage points increase in female representation ($28.4 \times 0.076 \approx 2.2$). This magnitude represents a substantial shift in the gender composition of councils; in a standard 50-member council, a 2.2 percentage point increase effectively means one additional female councillor, demonstrating that ballot positioning can have meaningful real-world consequences for political representation.

The consistency of first-stage relationships across different outcome samples provides additional confidence in instrument validity. Despite varying sample sizes due to data availability across spending categories, the first-stage coefficient remains remarkably stable (ranging only from 28.071 to 28.529), suggesting that the instrument captures a systematic feature of electoral competition rather than spurious correlations with particular subsets of authorities.

1.5.2 Care-Related Services Spending

Care-Related Main Categories

Table 1.2 presents the main results on the relationship between female representation and spending on care-related services: children's social care, adult social care, public health, and education. Comparing OLS and 2SLS estimates highlights both the direction of selection bias and the magnitude of causal effects. The difference in sample size between OLS and 2SLS reflects the fact that the instrumental variable is only observed in election years; thus, the 2SLS estimates use the election-year subsample. For comparison, Appendix Table A.3 reports OLS estimates restricted to the same election-year sample used in the 2SLS analysis.

Children's social care provides the clearest evidence of gendered spending priorities. A 10 percentage point increase in female representation leads to a 1.43 percentage point increase in children's social care spending share (10×0.143=1.43), significant at the 1% level. This is about three times the OLS estimate (0.55 percentage points), suggesting that omitted variable bias reduces the observed association. The IV estimate captures the causal policy influence in settings where alphabetical ballot ordering affects election outcomes.

Table 1.2: Female Representation and Care-Related Spending

	Children's Social Care		Adult Social Care		Public Health		Education	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Main Results *(2SLS)*								
%FemCllr	0.055***	0.143***	0.030	0.121	0.022**	0.055**	-0.031	-0.121
	(0.016)	(0.054)	(0.024)	(0.081)	(0.009)	(0.028)	(0.044)	(0.129)
First-Stage Results *(Dependent Variable: % Female Councillors)*								
AlphaAdv		28.328***		28.071***		28.529***		28.328***
		(4.304)		(4.323)		(4.346)		(4.304)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓	✓	✓
Obs	1802	541	1784	534	1404	407	1997	573
R-squared	0.876	0.869	0.920	0.918	0.808	0.820	0.944	0.947
First-stage F		40.88		39.79		37.73		43.32
AR Chi-sq		9.790		2.535		4.300		0.905
AR p-val		0.002		0.111		0.038		0.341

Notes: The table presents OLS and IV estimates for the effect of female councillor representation on local government spending priorities. The dependent variables are spending categories as percentages of total service expenditure. The main independent variable is the percentage of female elected councillors in the local authority. The instrumental variable is the female share in advantageous alphabetical ballot positions (simple average across wards). Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Given that children's social care accounts for roughly 6-7% of total local authority expenditure, a 1.43 percentage point increase represents approximately a 20% rise relative to the baseline level of spending (1.43/7≈0.20). There are several ways to interpret the economic magnitude of this effect. One approach uses the instrument's strength: a one standard deviation increase in female alphabetical advantage (SD = 0.076) increases female representation by approximately 2.15 percentage points (0.076×28.328 = 2.15), which implies a 0.31 percentage point rise in children's social care spending share (2.15×0.143 ≈ 0.31). For a typical authority with a £300 million annual budget, this corresponds to around £0.93 million in additional spending on children's services (0.0031×£300m).

However, the most intuitive interpretation focuses on the councillor level, which I adopt as the preferred explanation throughout this analysis. In a typical local authority with 50 councillors, one additional female councillor increases female representation by 2 percentage points, which in turn raises children's social care spending by approximately 0.29 percentage points (2×0.143). This translates to an estimated £0.87 million in additional spending ($0.0029 \times$ £300m). This interpretation is preferred because it provides a concrete, policy-relevant unit of analysis: the effect of electing one more woman, which is easily understood and directly relevant to discussions about political systems and election rules.

Public health shows a similar but more modest pattern. The 2SLS estimate indicates that a 10 percentage point increase in female representation raises public health spending by 0.55 percentage points (10×0.055), significant at the 5% level. Using the preferred councillor-level interpretation, one additional female councillor in a 50-member council increases public health spending by approximately 0.11 percentage points (2×0.055), corresponding to roughly £0.33 million for a typical £300 million budget ($0.0011 \times$ £300m). The estimated effect is meaningful given that public health accounts for only approximately 2.7% of total expenditure.

The effects on adult social care and education are imprecisely estimated. For adult social care, both OLS and 2SLS estimates are positive but not statistically significant. In the case of education, the coefficients are negative but also insignificant. These null results may reflect institutional constraints: education is the largest spending category (averaging 27.32% of total expenditure), leaving limited room for short-term reallocation. Adult social care, while substantial at approximately 12% of expenditure, may be heavily constrained by statutory duties and demographic pressures, limiting councillors' discretion to reallocate resources.

Care-Related Subcategories

To identify which components drive the aggregate effects in care-related spending, I disaggregate total net current expenditure into subcategories defined by the General Fund Revenue Account Outturn guidance issued by the Department for Levelling Up, Housing and Communities. Although the dataset includes additional care-related items, this section reports only those subcategories with sufficient data coverage and statistical precision to support meaningful inference. Results for all other items are provided in Appendix Table A.4 for completeness. The six subcategories presented in Table 1.3 include two under children's social

care (looked-after children and safeguarding children), two under adult social care (mental health support for ages 18-64 and 65+), and two under public health (sexual health STI testing and children's health ages 0-5). Each outcome is measured as the share of total net current expenditure and estimated using the same 2SLS model as in the main analysis.

Table 1.3: Female Representation and Care-Related Subcategories Spending

	(1) Looked-After	(2) Safeguard	(3) Mental Health 18-64	(4) Mental Health 65+	(5) STI	(6) Chd Health 0-5
%FemCllr	0.0649***	0.0649***	0.0144**	0.0140**	0.0103**	0.00751*
	(0.0251)	(0.0238)	(0.00679)	(0.00579)	(0.00412)	(0.00436)
Obs	541	540	376	375	410	345
First-stage F	40.88	41.50	38.33	38.03	37.85	33.41

Notes: The table reports IV estimates of the effect of female councillor representation on the share of total net current expenditure (NCE) allocated to care-related subcategories. Definitions follow the General Fund Revenue Account Outturn guidance: "Looked-After Children" includes spending on residential and fostering services for children in care (including unaccompanied asylum-seeking children); "Safeguarding Children" covers child protection and prevention services; "Adult Mental Health (18–64)" and "Adult Mental Health (65+)" cover support for working-age and older adults, respectively; "Sexual Health (STI Testing)" includes testing and treatment services; "Children's Health (0–5)" includes health visiting and early years health checks. Specifications mirror the main models: the instrument is the female share in advantageous alphabetical ballot positions, controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The largest effects are observed in children's services. A 10 percentage point increase in female representation leads to a 0.65 percentage point increase in spending on looked-after children and a 0.65 percentage point increase in safeguarding children, both significant at the 1% level. Using the councillor-level interpretation, one additional female councillor in a 50-member council increases spending on looked-after children by approximately 0.13 percentage points (2×0.0649), corresponding to roughly £390 thousand for a typical £300 million budget. Similarly, safeguarding spending increases by 0.13 percentage points, or approximately £0.39 million.

Spending on looked-after children covers the provision of formal care placements for children in local authority care, including residential homes, foster care, and placements for unaccompanied asylum-seeking children. Safeguarding children includes child protection services delivered by social workers, such as assessment, case management, and multi-agency coordination, as well as the administrative functions that support commissioning and safeguarding strategy. These results suggest that the aggregate increase in children's social care spending is primarily directed toward core protection and care responsibilities.

For adult social care, female representation increases spending on mental health services by 0.14 percentage points for both working-age (18-64) and older (65+) populations, following a 10 percentage point increase in female representation. At the councillor level, one additional female councillor increases mental health spending by approximately 0.03 percentage points for each age group (2×0.014), corresponding to roughly £90 thousand per age category. These categories include a range of long- and short-term support for adults with mental health needs, including residential care, supported living arrangements, and community-based services.

In public health, a 10 percentage point increase in female representation is associated with a 0.10 percentage point increase in spending on sexual health services and a 0.08 percentage point increase in early years health provision. At the councillor level, one additional female councillor increases sexual health spending by approximately 0.02 percentage points (2×0.0103), or roughly £60 thousand, and children's health spending by approximately 0.015 percentage points (2×0.00751), or roughly £45 thousand. Sexual health services include local STI and HIV testing and treatment programmes, often delivered through clinics, pharmacies, or contracted GP services. Early years health provision refers to preventive health support for children aged 0-5, such as developmental reviews, health visiting, and targeted early intervention for families with additional needs.

Overall, the subcategory results identify the specific domains where female representation significantly affects spending priorities. Within children's social care, the aggregate effects documented in Table 1.2 are driven primarily by increased investment in child protection and looked-after children services. For adult social care, although the aggregate effect was not statistically significant, female councillors do reallocate resources toward mental health services. These findings indicate that female councillors systematically redirect funding toward these specific care-related services, even when broader category-level effects are not detected.

1.5.3 Infrastructure and Traditional Services Spending

Table 1.4 assesses the effects of female representation on service categories typically associated with physical infrastructure and local built-environment responsibilities: highways and transport, cultural services, environmental services, and planning. These results contrast with the patterns observed for care-related services and provide further evidence of a systematic

reallocation of expenditure priorities.

The most substantial negative effect is found in highways and transport. The 2SLS estimate indicates that a 10 percentage point increase in female representation reduces transport spending by 1.57 percentage points (10×0.157=1.57), significant at the 1% level. Using the preferred councillor-level interpretation, one additional female councillor in a 50-member council reduces highways and transport spending by approximately 0.31 percentage points (2×0.157), corresponding to roughly £0.93 million less spending for a typical £300 million budget. This effect is substantially larger than the OLS estimate, reinforcing the interpretation of a Local Average Treatment Effect (LATE) concentrated in local authorities where electoral outcomes are influenced by alphabetical ballot order. The Anderson-Rubin test (Chi-sq = 7.551, $p = 0.006$) confirms this result is robust to potential weak instrument concerns.

Table 1.4: Female Representation and Infrastructure-Related Spending

	Highways and Transport		Cultural		Environmental		Planning Services	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Main Results *(2SLS)*								
%FemCllr	-0.031**	-0.157***	-0.020	-0.181	-0.078*	-0.302*	-0.010	0.001
	(0.015)	(0.055)	(0.040)	(0.156)	(0.044)	(0.169)	(0.028)	(0.089)
First-Stage Results *(Dependent Variable: % Female Councillors)*								
AlphaAdv		26.038***		28.444***		28.328***		27.955***
		(6.192)		(4.326)		(4.304)		(4.358)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Party FE	✓	✓	✓	✓	✓	✓	✓	✓
Obs	1191	315	1984	568	1997	573	1937	555
R-squared	0.350	0.332	0.654	0.618	0.871	0.859	0.706	0.671
First-stage F		17.69		43.23		43.32		41.14
AR Chi-sq		7.551		1.425		3.438		0.000
AR p-val		0.006		0.233		0.064		0.988

Notes: The table presents OLS and IV estimates for the effect of female councillor representation on local government spending priorities. The dependent variables are spending categories as percentages of total service expenditure. The main independent variable is the percentage of female elected councillors in the local authority. The instrumental variable is the female share in advantageous alphabetical ballot positions (simple average across wards). Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Environmental services also exhibit a negative association. The 2SLS estimate indicates that a 10 percentage point increase in female representation reduces environmental spending by 3.02 percentage points (10×0.302), significant at the 10% level, though the result is

less precisely estimated. At the councillor level, one additional female councillor reduces environmental spending by approximately 0.60 percentage points, or roughly £1.8 million. However, this estimate should be interpreted with caution given the broad scope of environmental and regulatory services, which includes not only waste collection and street cleansing but also pollution control, pest management, food and water safety, trading standards, CCTV operations, and environmental enforcement. Since environmental services encompass a wide range of functions, and local authorities differ in how they allocate resources within this category, the observed reduction in spending cannot be confidently attributed to any one specific component.

Cultural and planning services show no statistically significant point estimates in both OLS and 2SLS specifications. Neither the standard t-tests nor the Anderson-Rubin tests rejected the null hypothesis of no effect. These null results indicate that female councillors do not uniformly reduce funding across all infrastructure-related categories. Rather than withdrawing support systematically, spending adjustments appear concentrated in specific categories, primarily highways and transport and to a lesser extent environmental services. The results suggest that when female councillors increase spending on care-related services, the primary trade-off comes from reduced investment in transport infrastructure, while other traditional service areas do not systematically experience trade-offs.

1.5.4 Heterogeneous Effects by Institutional Context

This subsection examines whether the effects of female representation vary by institutional context. Specifically, I test whether female councillors have stronger influence in councils with relatively high female representation compared to councils where women comprise a smaller minority. To classify councils, I use the share of female councillors in each council's first observed election. This baseline value determines the classification, which remains fixed throughout the analysis. This approach avoids endogeneity: using average representation across all years could create reverse causality if spending changes themselves influence future female representation. By using only the first observed election (before any spending changes in my sample occur), the classification is predetermined and cannot be affected by subsequent policy choices.

Critical Mass Threshold. To test whether a critical mass threshold exists, I systematically vary the threshold used to classify councils, identifying the point at which female representation begins to influence spending priorities. This approach tests whether the policy influence of women requires reaching a critical mass, and thus whether gendered policy preferences are more likely to shape spending outcomes when female representation is sufficiently high.

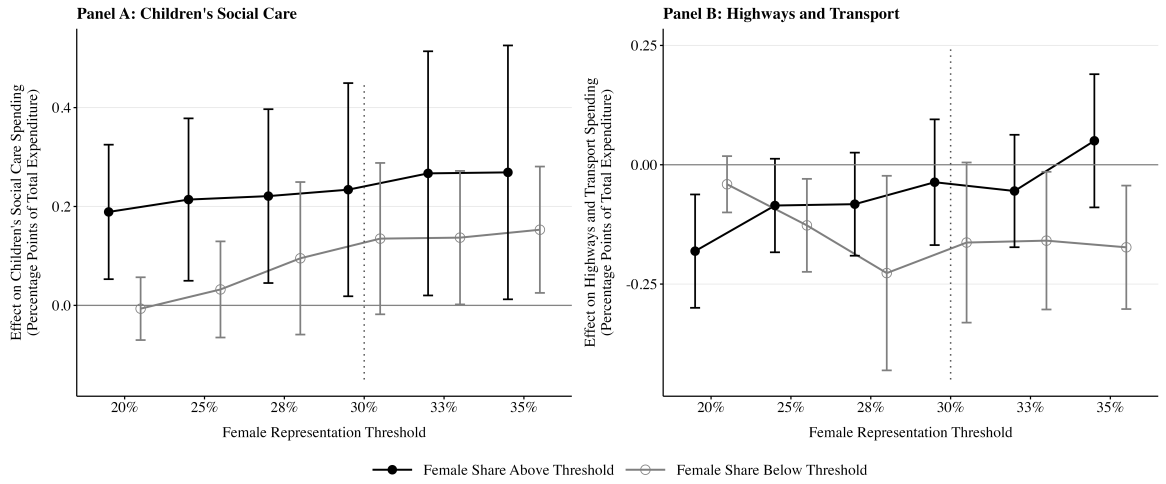

Figure 1.6: Gender Composition Thresholds and Spending Priorities

I test thresholds ranging from 20% to 35% and report complete results in Appendix Table A.5. Figure 1.6 displays the pattern for two spending categories: children's social care (Panel A) and highways and transport (Panel B). The figure reveals a clear discontinuity in effect sizes around 28-30% in Panel A. For councils where baseline female representation falls below 28%, the effect of female representation on children's social care spending is positive but not statistically significant. However, once baseline representation exceeds 30%, the effect becomes substantially larger and statistically significant. This pattern suggests that female councillors need to reach roughly 30% representation before they can meaningfully shift spending toward children's social care. Below this threshold, their influence appears limited, supporting the idea that women need to reach a certain level of representation to counter institutional barriers and exert meaningful policy influence.

The 30% threshold is consistent with critical mass theory, which posits that minority groups need to reach approximately one-third of an organization's membership before they can effectively influence outcomes (Dahlerup, 1988). Empirical evidence from Swedish municipalities also supports this threshold, showing that female representation effects on childcare spending strengthened substantially once women exceeded roughly 30% of councillors (Svaleryd, 2009).

Threshold-Based Heterogeneity. Based on the threshold analysis above, I classify councils using a 28% cutoff, set just below the 30% critical mass threshold. Councils above this cutoff are "above-threshold," and those at or below it are "below-threshold." This classification allows me to compare councils where female councillors are approaching or have reached critical mass with those where they remain a smaller minority. The 28% cutoff provides the clearest distinction between these two groups, as evidenced by the statistical significance patterns in Table A.5.

The critical mass pattern is most pronounced for children's social care spending. In above-threshold councils, female representation has a substantial and statistically significant positive effect on children's social care spending. In below-threshold councils, there is no statistically significant effect. This stark contrast demonstrates that female councillors require sufficient institutional presence before they can meaningfully redirect spending toward children's social care.

For highways and transport spending, both above-threshold and below-threshold councils show some negative effects, but the patterns are less consistent and precisely estimated than for children's social care. This suggests that while female councillors in above-threshold councils systematically increase spending on children's social care, the specific trade-offs (that is, which other spending categories are reduced to fund these increases) are not clearly identified in this heterogeneous analysis. The reallocation away from highways and transport appears in some specifications, but it is not the sole or clearly dominant offset.

Timing and Persistence. To further examine the dynamics of these effects and test for potential reverse causality, Figure 1.7 displays the estimated effects of female representation on spending at three time points: the year before the election ($t-1$), the election year (t), and the year after ($t+1$). This analysis serves two purposes: it provides a placebo test to validate the identification strategy and examines whether spending changes persist beyond the immediate election year. The detailed results are presented in Appendix Table A.6.

The placebo test supports the causal interpretation. Panel A shows that in the year before the election ($t-1$), there is no significant relationship between female representation and children's social care spending in either above-threshold or below-threshold councils. Since current female representation cannot causally affect past spending decisions, this null result

confirms that the observed effects are not driven by pre-existing trends or reverse causality.

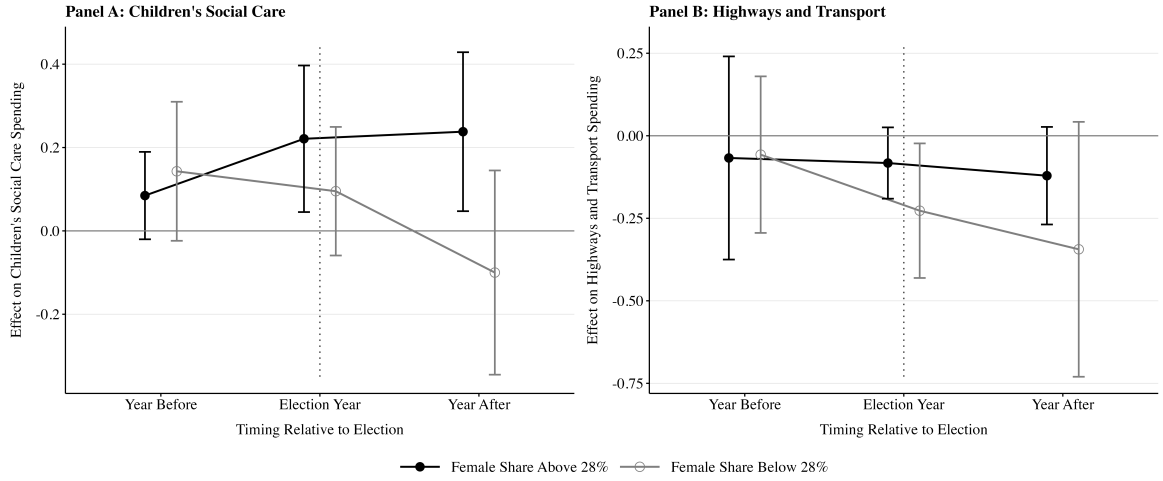

Figure 1.7: Timing Dynamics of Female Representation Effects on Spending

The effects appear clearly in the election year (t) and persist into the following year ($t + 1$). In above-threshold councils, the positive effect on children's social care spending remains substantial and statistically significant in both the election year and the year after. This persistence indicates that the spending changes reflect lasting policy shifts rather than temporary adjustments. The pattern is consistent with budget implementation cycles: decisions made during or shortly after an election are reflected in spending that year and carry forward into subsequent budget periods. In contrast, below-threshold councils show no significant effects at any time period for children's social care, reinforcing the critical mass interpretation. Overall, the timing analysis strengthens the conclusion that female representation causally affects spending priorities when women approach or exceed the critical mass threshold, with effects on children's social care that persist beyond a single budget cycle.

1.5.5 Instrument Robustness

Alternative IV Specifications

To assess the validity and robustness of the main results, I test whether the effects hold across alternative instrumental variable constructions. As described in Section 1.4.3, I vary both the definition of advantageous ballot positions and the aggregation weights used to construct the instruments. This analysis addresses two main questions: first, whether the results depend on how advantageous ballot positions are defined; and second, whether alternative weighting schemes that account for ward size, electoral competition, or the number of candidates influence the estimated effects.

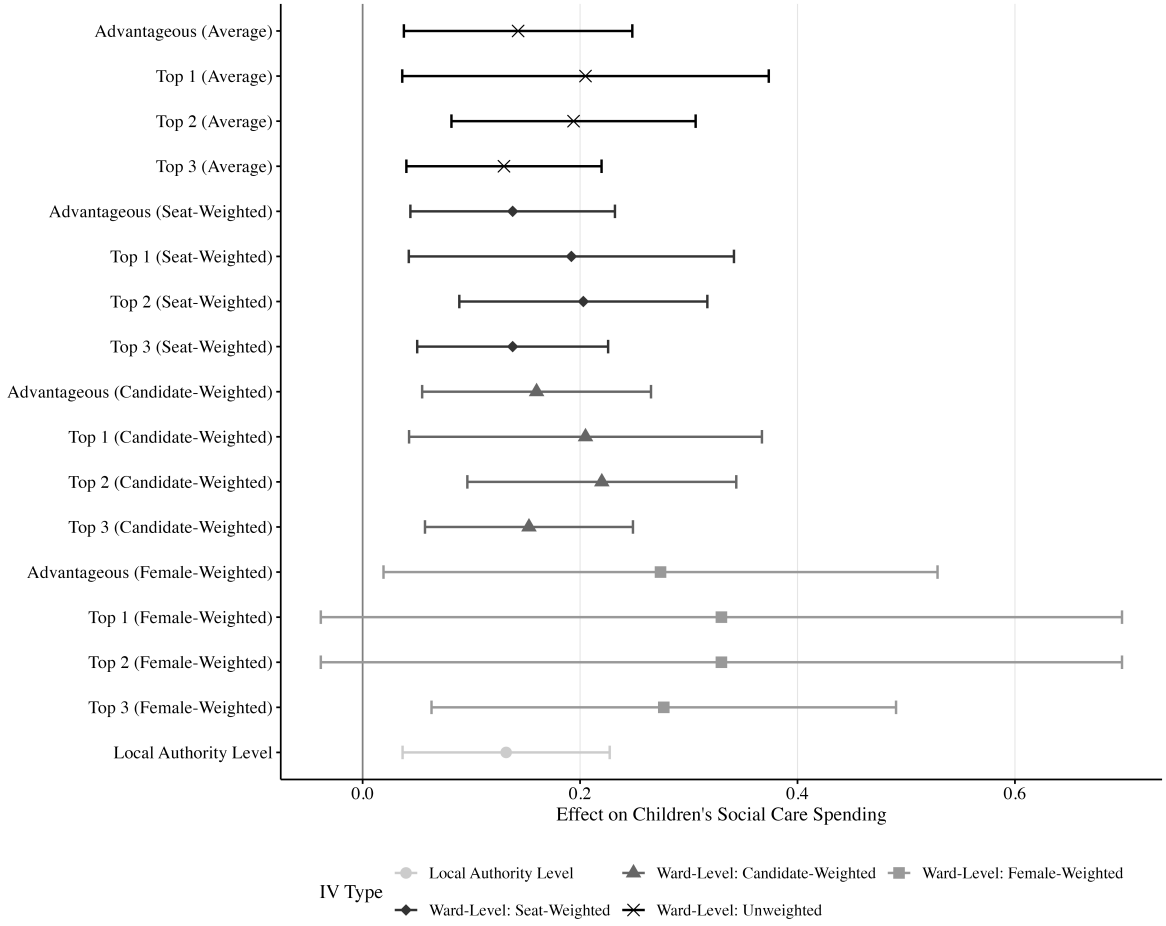

Figure 1.8: Robustness to Alternative IV Specifications

Figure 1.8 displays the estimated effects of female representation on children's social care spending across 17 different IV specifications. The figure organises results into five groups based on the type of instrument: ward-aggregated measures using unweighted averages, seat-weighted averages, candidate-weighted averages, female-candidate-weighted averages, and the local authority-level instrument. Within each ward-aggregated group, I test four position definitions: advantageous positions (where N equals ward seats), and fixed thresholds using only the top 1, top 2, or top 3 positions regardless of ward size.

The results demonstrate consistency across most specifications. The main specification uses advantageous positions with unweighted averages (the first cross in Figure 1.8), while alternative definitions and weighting schemes produce estimates with slight variation around the main estimate. These differences suggest that the core finding, female representation increases children's social care spending, is robust to reasonable variations in how advantageous positions are defined and how ward-level variation is aggregated to the local authority level.

The female-candidate-weighted instruments produce larger point estimates with wider confidence intervals, reflecting weaker first-stage relationships. As shown in the detailed tables (Appendix Tables A.7–A.10), these specifications yield first-stage F-statistics below 10, suggesting potential weak instrument concerns. However, even these specifications produce positive and statistically significant effects for children's social care, reinforcing the main conclusion. The local authority-level instrument, which ranks candidates across the entire authority rather than within wards, also produces a positive and statistically significant effect, though this specification abstracts from the ward-level structure where ballot-order effects actually occur.

The pattern extends to other spending examined in this study. Appendix Tables A.7 through A.10 report the full set of results for children's social care, public health, highways and transport, and environmental services across all IV specifications. Overall, this comprehensive robustness analysis demonstrates that the main findings are not sensitive to specific IV construction. The positive effects on children's social care and public health, and the negative effects on highways and transport, hold across a wide range of IV constructions.

Stable Female Candidacy

A potential threat to the identification is that alphabetical ballot positioning could be correlated with supply-side changes in female candidacy. If parties strategically increase female recruitment in areas where voters demand more care services, or if unobserved area characteristics drive both the number of women running for office and spending preferences, the instrument might violate the exclusion restriction. In such cases, the estimated effects would capture confounded variation in candidate supply rather than the causal effect of ballot positioning on policy outcomes. To address this concern, Table 1.5 restricts the sample to local authorities with stable female candidate pools over time. I construct three complementary stability measures, each designed to isolate variation in electoral outcomes (who wins) from variation in candidate supply (who runs). Each measure is calculated from the share of female candidates standing for election in each local authority-year.

Table 1.5: Female Representation Effects Under Stable Candidacy Conditions

Panel A: Range of Female Candidacy Across Years				
Restriction:	None	$< 10pp$	$< 8pp$	$< 5pp$
% Female Cllr	0.143***	0.195***	0.173***	0.188*
	(0.0536)	(0.0722)	(0.0612)	(0.105)
Observations	541	422	364	156
First-stage F-stat	40.88	16.98	21.84	10.48
Panel B: Within-Council Standard Deviation of Female Candidacy				
Restriction:	$< 4pp$	$< 3.5pp$	$< 3pp$	$< 2pp$
% Female Cllr	0.139**	0.146**	0.206***	0.267***
	(0.0602)	(0.0625)	(0.0773)	(0.100)
Observations	474	424	361	184
First-stage F-stat	22.91	22.88	15.89	10.14
Panel C: Maximum Year-to-Year Change in Female Candidacy				
Restriction:	$< 10pp$	$< 8pp$	$< 5pp$	$< 3pp$
% Female Cllr	0.114**	0.128**	0.240***	0.307**
	(0.0551)	(0.0628)	(0.0806)	(0.152)
Observations	491	436	226	82
First-stage F-stat	25.45	20.19	19.55	7.68

Notes: This table tests whether the IV effect varies with supply-side changes in female candidacy by restricting the sample to local authorities with stable female candidate pools. Panel A restricts on the intertemporal range of female candidacy, Panel B on the within-council standard deviation, and Panel C on the maximum year-to-year change. The dependent variable is children's social care spending as a share of total net current expenditure. The instrument is the female share in advantageous alphabetical ballot positions (ward-level average). Controls include log total expenditure, number of councillors, log population, sex ratio, largest party seat share, and year, region, and party-control fixed effects. Standard errors are clustered at the local authority level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The first measure uses the range of female candidacy across all observed elections within each

local authority (Panel A of Table 1.5). For each authority, I calculate the difference between the maximum and minimum female candidacy rates observed across election years, then restrict the sample to authorities where this range falls below specified thresholds: less than 10, 8, or 5 percentage points.[16] This criterion excludes councils that experienced large swings in female candidate supply between any two elections, retaining only those with consistently similar candidate pools throughout the study period.

The second measure uses the within-council standard deviation of female candidacy rates (Panel B of Table 1.5). For each local authority, I calculate the standard deviation across all observed election years, capturing the typical year-to-year variability in candidate supply. I then restrict the sample to councils with standard deviations below 4, 3.5, 3, or 2 percentage points. Unlike the range, which is sensitive to extreme values, the standard deviation provides a more robust measure of overall stability across all elections.

The third measure focuses on the maximum year-to-year change in female candidacy (Panel C of Table 1.5). For each local authority, I calculate the absolute change in female candidacy rate between each pair of consecutive elections, then identify the maximum such change within that authority. The sample is restricted to councils where this maximum change falls below 10, 8, 5, or 3 percentage points. This measure is designed to exclude councils that experienced sudden spikes in female recruitment, such as those resulting from party policy changes or local recruitment campaigns, while retaining those with gradual or minimal changes.

These increasingly strict criteria ensure that the remaining councils show little change in the gender composition of their candidate pools, allowing the analysis to better isolate the effect of alphabetical ballot position on electoral success while holding candidate supply approximately constant. The results (shown in Table 1.5) remain stable in both magnitude and direction across all specifications.

These findings support the validity of the exclusion restriction in the instrumental variable (IV) strategy. The fact that alphabetical ballot positioning continues to influence spending priorities, even when the supply of female candidates remains effectively constant, sug-

[16] I test thresholds down to these levels as they represent substantive restrictions on sample composition while maintaining sufficient sample size for reliable inference. More restrictive thresholds (e.g., range $< 3pp$) reduce the sample substantially and weaken the first-stage relationship, making inference less reliable. The key finding is that coefficient estimates remain stable across all reported thresholds, suggesting results are not sensitive to the specific cutoff chosen.

gests that the instrument affects outcomes through its impact on electoral success rather than through changes in candidate recruitment or other supply-side factors. In councils where the proportion of female candidates remains relatively stable from one election to the next, as measured by range, standard deviation, or maximum year-to-year change, the quasi-random assignment of female candidates to top ballot positions still produces meaningful variation in female representation.

This robustness check also addresses a potential concern raised by the female-candidate-weighted instruments discussed earlier in this section, which resulted in wider confidence intervals and weaker first-stage relationships. Those specifications gave more weight to wards with larger numbers of female candidates, potentially increasing the influence of areas with a higher or more fluctuating supply of female candidates. The stable candidacy analysis shows that even when this variation is entirely removed, the main results still hold. This supports the conclusion that the alphabetical ballot advantage influences spending priorities through its effect on the gender composition of elected councils, not through correlated shifts in female candidate supply or other confounding factors.

1.6 Additional Robustness Checks

This section presents additional robustness checks that complement the core identification strategy validated in Section 1.5.5. While the main results rely on variation in election years where the instrument is directly observed, I examine whether the findings extend to the full panel of years between elections. I also test sensitivity to alternative outcome definitions and explore heterogeneity across political and institutional contexts. These checks confirm that the main findings are not driven by specific sample restrictions or measurement choices.

Election Years vs. Full Panel. The main 2SLS analysis restricts the sample to election years, which aligns with the construction of the instrumental variable. Alphabetical ballot positions are only observed and assigned when elections occur, making election years the natural setting for the identification strategy. However, this restriction raises a practical question: do the effects of female representation persist beyond the immediate election year, or are they limited to the specific years when councils are elected?

Table 1.6: Election-Year (Main) vs. Full Sample

	Children Care		Public health		Highways & Transport		Environmental	
	(1) Main	(2) Full	(3) Main	(4) Full	(5) Main	(6) Full	(7) Main	(8) Full
%FemCllr	0.143***	0.145***	0.055**	0.069**	-0.157***	-0.191***	-0.302*	-0.389**
	(0.054)	(0.056)	(0.028)	(0.029)	(0.055)	(0.065)	(0.169)	(0.196)
Obs	541	1736	407	1404	315	987	573	1770
First-stage F	40.88	32.25	37.73	32.00	17.69	12.07	43.32	32.94

Notes: This table reports 2SLS estimates of the effect of female councillor representation on the category's share of total service expenditure. Columns labelled "Main" restrict the sample to election years, consistent with the construction of the instrument based on ballot paper positions. Columns labelled "Full" extend the instrument to non-election years by carrying forward values between elections. Dependent variables are spending shares in Children's Social Care, Public Health, Highways & Transport, and Environmental Services. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

To address this question, Table 1.6 compares estimates from the election-year sample used in the main analysis ("Main") to those from an extended sample that includes all years between elections ("Full"). For the full-panel specification, I carry forward the instrumental variable from each election year to cover the entire inter-election period. This approach assumes that the female alphabetical advantage obtained in an election continues to influence council composition throughout councillors' terms, which typically span four years in English local government.

The results demonstrate strong consistency across both sample definitions. The positive effects on children's social care and public health remain statistically significant in both samples, as do the negative effects on highways and transport and environmental services. The direction and magnitude of coefficients are stable across specifications, with the full sample producing slightly larger point estimates in most cases. These findings indicate that the main results are not restricting to election years, and indicate the effects of female representation persist throughout the electoral cycle.

Alternative Outcome Definition. The main analysis uses net current expenditure (NCE) as the outcome measure and denominator. NCE reflects the discretionary operational costs of running local services within the financial year, excluding income from fees and charges. As discussed in Section 1.3.1, this measure best captures the spending decisions under councillors' direct control. To test the sensitivity of results to this choice, Table 1.7 reports estimates using total spending shares as the alternative outcome definition. Unlike NCE, total spend-

ing includes capital financing, debt service, and other largely non-discretionary components that are typically determined by long-term commitments and statutory obligations rather than annual budgetary decisions.

Table 1.7: Robustness to Alternative Outcome Definition: Total Spending

Dependent Var:	Children Care	Adult Care	Public Health	Education
% Female Cllr	0.0924**	0.0385	0.0191	-0.182
	(0.0390)	(0.0800)	(0.0165)	(0.122)
Observations	541	540	410	573
First-stage F	40.88	40.81	37.85	43.32
Dependent Var:	Highways & Transport	Cultural	Environmental	Planning Services
% Female Cllr	-0.0647	-0.128	-0.0780	-0.0737
	(0.0475)	(0.121)	(0.122)	(0.0707)
Observations	573	573	573	573
First-stage F	43.32	43.32	43.32	43.32

Note. This table reports 2SLS estimates of the effect of female councillor representation on spending as a percentage of total spending, rather than net current expenditure as in the main analysis. The endogenous regressor is the share of female councillors, instrumented by the female share in advantageous alphabetical ballot positions (average across wards). Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The results show that the effect on children's social care remains positive and statistically significant (0.092), although smaller than the main NCE-based estimate (0.143). Effects on other care-related areas, including adult social care, public health, and education, are also positive but not statistically significant. For infrastructure categories, the coefficients remain negative but are not statistically significant in any specification. The smaller effects observed when using total spending as the outcome are consistent with the idea that female councillors primarily influence the discretionary parts of the budget. Total spending includes many fixed expenditure items that councillors have limited control over in the short term, which reduces the observable impact of changes in council composition. These findings support using NCE as the primary outcome measure for identifying how female representation affects spending priorities. Estimating the model with log spending levels rather than expenditure shares produces smaller and mostly insignificant effects, while the coefficient on total services is close to zero, confirming that the main results reflect a reallocation across categories rather than changes in overall spending levels (Appendix Table A.12).

Party Affiliation. A potential remaining concern is that voters might sort into parties based on both gender attitudes and service preferences. The main analysis includes party control fixed effects to account for systematic differences in both female candidate recruitment and spending platforms across parties. However, one might be concerned that the effects are driven entirely by party ideology rather than gender composition per se, or that voters sort into parties based on preferences over both gender representation and service provision.

Table 1.8: Female Councillors and Children's Care Spending by Party

	All Councils	Subsample			
	Interactions	Conservative	Labour	NOC	Other
%FemCllr		0.0604	0.156	0.311*	0.0537
		(0.0421)	(0.123)	(0.163)	(0.109)
Conservative×%FemCllr	0.152**				
	(0.067)				
Labour×%FemCllr	0.065**				
	(0.033)				
NOC×%FemCllr	0.108***				
	(0.038)				
Other×%FemCllr	0.170**				
	(0.068)				
Observations	1453	964	242	187	60
R-squared	0.874	0.912	0.851	0.828	0.942
First-stage F-stat	22.93	22.36	5.46	7.84	1.60

Note. Column (1) reports pooled 2SLS estimates interacting % female councillors with party control; coefficients represent marginal effects within each party group. Remaining columns report subsample regressions by leading party (NOC = no overall control). The dependent variable is children's social care spending as a share of total net current expenditure. The instrument is the female share in advantageous alphabetical ballot positions (ward-level average). Controls include log total expenditure, number of councillors, log population, sex ratio, largest party seat share, and year and region fixed effects. Standard errors are clustered at the local authority level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

As a robustness check, I examine whether the effect of female councillors varies across political contexts. The major UK parties have longstanding differences in their approach to public spending, with Conservatives generally emphasising fiscal discipline and Labour emphasising redistribution (Kollias et al., 2014). To test whether gender effects are confounded with partisan priorities, I estimate specifications that allow the impact of female representation to differ by leading party. Column (1) of Table 1.8 reports a pooled model with interactions between female representation and party control, presented as marginal effects by party. Columns (2) to (5) estimate the model separately for each party subsample.

The results indicate a positive effect of female councillors on children's social care spending across all major party groups. In the pooled interaction model, the effect in Conservative-led councils is about 0.15 percentage points, with Labour-led and no overall control councils

showing estimates in a similar range. Subsample estimates are less precise, with some statistically insignificant, especially in smaller samples. Overall, the findings suggest that the effect on children's social care spending is not explained by longstanding party differences. Instead, the influence of female councillors appears consistently across political contexts, supporting the interpretation that the effect reflects gender composition rather than party control.

Council Size. As a further robustness check, I examine whether the effects of female representation vary by council size. Larger councils may exhibit different decision-making dynamics than smaller ones: they typically have more formalised committee structures, more specialised councillors, and greater administrative capacity. Conversely, smaller councils may allow individual councillors to exert more direct influence over spending decisions.

To test whether council size moderates the relationship between female representation and spending priorities, I split the sample at various thresholds and estimate separate effects for councils above and below each cutoff. Table A.11 in the Appendix reports estimates using five different cutoffs: 40, 45, 50, 55, and 60 councillors. For each cutoff, councils are classified as "Below Cutoff" if their size is less than or equal to the threshold, and "Above Cutoff" if their size exceeds it. The results show that effects vary somewhat across council sizes and spending categories, with no single clear pattern emerging.

For children's social care, the positive effects are generally stronger and more consistently significant in smaller councils (below the cutoffs), while effects in larger councils become less precise and lose statistical significance at higher thresholds. For public health, effects appear across both small and large councils, though patterns are inconsistent across cutoffs. The negative effects on highways and transport are most pronounced in larger councils, appearing significant in the "Above Cutoff" columns across multiple thresholds. Effects on environmental services show mixed patterns with no clear relationship to council size.

Overall, while the effects of female representation on spending priorities are observed across councils of different sizes, their strength and precision vary. Despite these variations, the core findings remain robust across all specifications: female representation leads to increased spending on care-related services, particularly children's social care, with offsetting reductions mainly in highways and transport. The consistency of results across sample definitions, outcome measures, political contexts, and institutional settings reinforces confidence in the

main results.

1.7 Conclusion

This chapter examines whether female representation affects local government spending priorities in England. Using newly constructed data on local elections and a novel identification strategy based on alphabetical ballot ordering, the analysis provides causal evidence that increasing the share of female councillors shifts local budgets toward care-related services. A 10 percentage point increase in female representation raises children's social care spending by 1.43 percentage points, approximately a 20% increase relative to baseline levels. Looking at more detailed budget categories reveals that children's social care increases are concentrated in core protection and care roles, including looked-after children and safeguarding services. These increases are accompanied by reductions in infrastructure spending, most notably highways and transport. However, the evidence does not suggest that female councillors systematically cut back on all traditional services. Other categories such as cultural services and planning show no significant effects.

Institutional context moderates these effects substantially. Councils where women hold at least 30% of seats show significantly stronger increases in children's social care spending than councils below this threshold, where effects are small and not statistically significant. This critical mass pattern is consistent with the theory that women need to reach approximately one-third of membership before they can effectively influence policy outcomes. The timing of effects supports a causal interpretation: effects appear clearly in election years and persist in subsequent years, while tests using pre-election outcomes show no significant relationships, ruling out reverse causality.

The results prove robust to extensive checks. The core findings hold across alternative instrument constructions that vary how advantageous ballot positions are defined (e.g., top 1, 2, or 3 positions) and how ward-level variation is aggregated to the council level (e.g., unweighted versus seat-weighted averages). Restricting the sample to councils where the supply of female candidates remains stable over time produces consistent results, supporting the validity of the instrument. This check isolates variation in electoral success from variation in who parties choose to run, confirming that the instrument affects outcomes through its impact on election results rather than through changes in candidate supply. The positive effects on

children's social care persist when extending the analysis to all years between elections, hold when using alternative outcome measures, and appear consistently across different types of party control.

This chapter makes several contributions to the literature on female political representation. First, it provides new causal evidence from local government in a high-income setting without formal gender quotas, using England's first-past-the-post electoral system. Second, it introduces a novel identification strategy based on alphabetical ballot ordering, which addresses the endogeneity concerns that often limit inference in studies of representation and policy outcomes. The consistency of results across alternative ways of constructing the instrument and under stable candidacy conditions strengthens confidence in this approach for future research. Third, it demonstrates that descriptive representation translates into substantive representation. Increasing the share of women in elected office leads to observable and meaningful changes in how public resources are allocated, particularly toward care-related services.

The findings also speak to broader questions about how gendered experiences shape engagement with the state. Women continue to bear a disproportionate share of caregiving responsibilities and rely more heavily on social infrastructure. Their political representation offers a channel through which these priorities can be reflected in public budgets. This chapter shows that when women gain sufficient voice in local government, particularly when reaching approximately 30% representation, they are able to shift spending toward services most aligned with their lived experiences and policy preferences. In doing so, they not only participate in decision-making but also help to reshape how governments respond to social needs. The results highlight that institutional features matter for translating women's presence into policy influence. Both the electoral rules that shape who gets elected and the gender balance within councils affect whether women's policy preferences are reflected in spending decisions.

Chapter 2

English Female Councillors and Domestic Abuse

Abstract

This chapter examines the relationship between the gender composition of local councils and domestic abuse in England. Using administrative data on police-reported incidents, we find a positive association between female representation in local government and reported domestic abuse rates. However, individual-level survey data indicate that a higher proportion of female councillors is associated with a lower probability that a woman has experienced domestic abuse in the past year. This divergence suggests that female representation increases the visibility of abuse in official statistics through enhanced reporting rather than increasing actual prevalence. We explore two channels consistent with this interpretation: women in areas with greater female representation are more likely to self-report incidents to police, and councils with more female representation allocate more resources to public health services supporting abuse victims. Moreover, the positive association between female representation and police-recorded abuse is concentrated in councils without statutory partnerships with specialist domestic abuse support organisations, suggesting that such partnerships may provide alternative reporting pathways that reduce reliance on police recording.

2.1 Introduction

Domestic abuse is a major public health challenge with profound social and economic consequences. It is among the most widespread forms of gender-based violence: globally, an estimated one in three women has experienced physical or sexual violence by an intimate partner (WHO, 2021). In England and Wales, the prevalence is roughly one in four, with 1.7 million women affected in 2021.[1] Beyond its human costs, domestic abuse also imposes significant economic burdens. The total cost in England and Wales was estimated at £66 billion in 2016, with emotional harms accounting for the largest share (Oliver et al., 2019). Despite recent legal reforms, including the Domestic Abuse Act 2021, abuse remains underreported, difficult to monitor, and often misunderstood.[2]

A central challenge in studying domestic abuse is measuring its true prevalence. Administrative data, such as police recorded crime (PRC), provide a reliable but partial view, as they capture only cases that come to official attention. Victims may choose not to report abuse due to fear, stigma, or lack of trust in authorities (Felson and Paré, 2005; Tjaden and Thoennes, 2000). Survey data help fill this gap. For example, the Crime Survey for England and Wales (CSEW) includes a self-completion module that allows respondents to report abuse confidentially. These two sources – police data and survey data – often tell different stories. As shown in Figure 2.1, they differ both in levels and in trends over time, highlighting the need to use multiple data sources to get a more accurate picture of domestic abuse.

The growing visibility of domestic abuse in official statistics raises important questions. Why has the gap between police and survey data narrowed over time? Are victims more willing to come forward? Have public attitudes shifted? Are institutions responding more effectively? And more broadly, how can domestic abuse be better addressed? One possible answer lies in the political representation and empowerment of women. A growing body of research finds that female political leadership can shape policy agendas, expand access to public services, and shift norms around gender and violence (e.g., Anukriti et al., 2022; Iyer et al., 2012; Kuipers, 2020). In lower-income settings, exposure to female leaders has been shown to

[1] The data are drawn from Office for National Statistics (2022) and National Centre for Domestic Violence (2023).

[2] The Domestic Abuse Act 2021 establishes a statutory definition of domestic abuse in England and Wales, encompassing physical, emotional, economic, and coercive behaviours, and introduces a range of legal measures to enhance protection for victims and improve institutional responses. It also creates new protective orders, restricts abusers' rights in court, recognises children as victims, and strengthens the role of the Domestic Abuse Commissioner (UK Parliament, 2021).

increase reporting of gender-based violence and reduce tolerance for abuse. Evidence from high-income democracies remains limited.

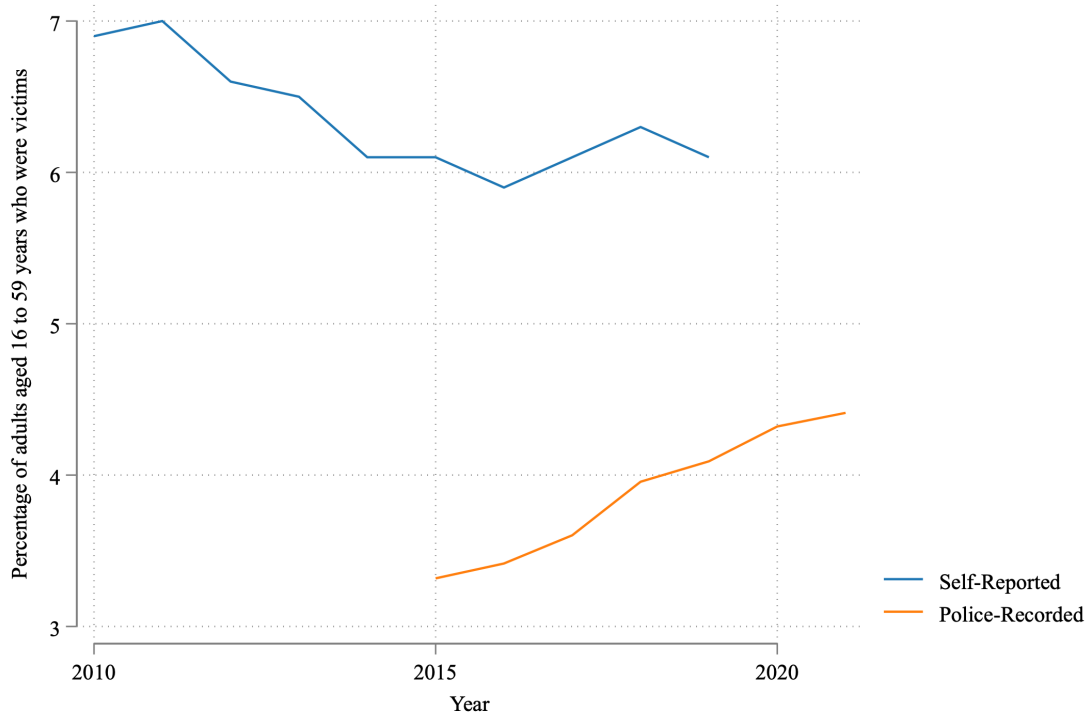

Figure 2.1: Prevalence of Domestic Abuse in England and Wales

Notes: The figure plots two measures of domestic abuse: (1) Self-reported prevalence, based on confidential responses to the Crime Survey for England and Wales (CSEW); and (2) Police-recorded cases, based on administrative data on domestic abuse-related incidents and crimes from the Home Office. Both are expressed as the percentage of adults aged 16 to 59 who reported or experienced abuse. *Sources:* Office for National Statistics (Crime Survey for England and Wales), Home Office (Police Recorded Crime).

Building on the literature, this chapter examines the relationship between women's political representation in local government and domestic abuse in England. We construct a novel dataset covering all English councillors who served between 2016 and 2020, including information on their gender and party affiliation. To account for the limitations of single data sources, we combine administrative police-recorded crime data with individual-level survey data from the CSEW. This approach allows us to capture both the reporting and prevalence of domestic abuse. In doing so, the study provides new evidence on how local-level gender representation may influence both victim reporting behaviour and experiences of abuse.

The empirical analysis reveals a striking divergence between administrative and survey measures of domestic abuse. Police-recorded cases show a positive association with female representation in local government, whilst individual-level survey data indicate that women in areas with more female councillors are less likely to report experiencing non-physical abuse. This pattern suggests that female representation does not increase the underlying prevalence

of abuse, but rather is associated with increased reporting and recording of incidents in official statistics.

We explore two channels consistent with this interpretation. First, analysis of victim behaviour shows that women in areas with greater female representation are more likely to self-report incidents to police, suggesting enhanced willingness to engage with authorities. Second, mediation analysis demonstrates that female councillors allocate significantly more resources to public health services, including programmes for domestic abuse victims, and this funding is positively associated with police-recorded cases. In addition, we find that the positive association between female representation and police-recorded abuse is concentrated in councils without statutory partnerships with specialist domestic abuse support organisations. In councils with such partnerships, the association reverses, consistent with specialist services providing alternative support pathways that reduce the share of cases reaching police records.

To probe whether these associations reflect causal relationships, we employ instrumental variable analysis exploiting quasi-random variation in female representation induced by ballot ordering. The IV analysis proves inconclusive: the estimation sample is substantially smaller than the main analysis and the OLS association is already insignificant in this restricted subsample. The two-stage least squares estimates remain negative in direction but are imprecise across all specifications, preventing definitive causal claims. These results highlight the difficulty of establishing causality in this setting given the available instruments, and we discuss the limitations and possible direction for future research in Section 2.7.

Our work contributes to several strands of research. First, we add to the literature on the political determinants of gender-based violence. Whilst most existing studies focus on low- and middle-income countries, we provide new evidence from a high-income democracy where elected women may influence local funding priorities and victim reporting. Second, we separate changes in victimisation from changes in reporting, which is an important distinction in the study of domestic abuse, by combining evidence from both administrative and survey data. Prior studies examining the relationship between female political representation and violence against women have typically relied on a single data source – most commonly police or administrative records – and inferred reporting effects indirectly (e.g., Iyer et al., 2012). By showing that police-recorded abuse increases whilst self-reported experiences decrease in areas with more female councillors, we provide direct evidence for the reporting mechanism

rather than relying on auxiliary assumptions. Third, we attempt to probe the causal interpretation of these findings using instrumental variable analysis based on surname ordering on local election ballots (validated in Chapter 1). Whilst the IV estimates are imprecise due to a substantially reduced estimation sample and generally weak first-stage relationships, the exercise demonstrates the challenges of causal identification in this context and motivates future work with stronger instruments or larger samples.

The rest of the chapter proceeds as follows. Section 2.2 reviews the related literature. Section 2.3 outlines the institutional context. Section 2.4 describes the data. Section 2.5 presents the empirical analysis and main results. Section 2.6 examines evidence for increased reporting through funding allocation, institutional partnerships, and victim behaviour. Section 2.7 presents instrumental variable analysis probing the causal interpretation of the main findings. Section 2.8 concludes.

2.2 Literature Review

Economists have long studied domestic abuse as a function of intra-household bargaining and broader economic shocks. A key insight is that women's economic opportunities affect their bargaining power and, in turn, their vulnerability to violence. Aizer (2010) shows that higher female wages reduce the likelihood of abuse, consistent with outside options strengthening women's positions within the household. Anderberg et al. (2016) model marriage as a form of insurance against unemployment and predict that male unemployment reduces abuse, while female unemployment increases it. Similarly, in the United States, Schneider et al. (2016) finds that rising unemployment during the Great Recession was associated with increases in intimate partner violence. Social context also matters. Mookerjee et al. (2022) find that neighbourhood-level domestic violence has a causal spillover effect on household-level abuse in India: using exogenous variation in neighbouring women's exposure to parental violence as an instrument, they show that an increase in neighbourhood domestic violence significantly raises the probability of abuse within a household, highlighting the role of local social norms and peer effects. These findings suggest that abuse is shaped not only by household-level factors but also by the wider community and potentially by political institutions.

A related literature investigates how female political representation affects gender-related outcomes, including violence against women. In India, Iyer et al. (2012) find that exposure

to female politicians increases reporting of crimes against women, and Anukriti et al. (2022) show that women's political participation raises reported experiences of spousal abuse. Iyer et al. (2012) interpret their findings as reflecting improved reporting rather than increased violence, though this interpretation rests on indirect evidence – specifically, the absence of effects on crimes against men. Anukriti et al. (2022), by contrast, argue that female leaders increase reported spousal abuse through a real behavioural channel: improved public provision of contraceptive services leads to marital conflict, particularly in households with strong son preference. Neither study directly measures both reporting and prevalence, leaving open the question of whether observed increases in officially recorded violence reflect genuine changes in incidence or in victims' willingness to come forward. However, Banerjee (2018) find limited effects of female chief ministers on crime rates, suggesting that local presence may matter more than symbolic leadership.

Beyond violence, female political leaders have been shown to affect welfare relevant to women and children. Chattopadhyay and Duflo (2004) find that women village leaders in India allocate more resources to public goods valued by women. Bhalotra and Clots-figueras (2014) show that women's representation in Indian state legislatures reduces neonatal mortality through improved access to antenatal and child health services. Bhalotra et al. (2023) extend this globally, finding that gender quotas in national parliaments reduce maternal mortality by increasing skilled birth attendance, prenatal care, and girls' education. In other contexts, female politicians have been shown to increase social spending and investment in education and health (Baskaran and Hessami, 2025; Chen, 2010; Clots-Figueras, 2012). While Hessami and da Fonseca (2020) note that effects on spending are stronger in developing countries, they also highlight evidence from developed democracies suggesting that female representation influences parliamentary deliberations, childcare policy choices, and institutional quality.

Political representation may also shift gender norms and public attitudes. Beaman et al. (2012) find that exposure to female leaders improves aspirations and educational outcomes for girls, while Campbell and Wolbrecht (2006) show that female politicians in the United States increase political engagement among teenage girls. Burnet (2011) describes how gender quotas in Rwanda altered public perceptions of women's social roles. Importantly, Kuipers (2020) shows that electing female candidates in Indonesian local elections reduces support for intimate partner violence among constituents, suggesting that descriptive repre-

sentation can directly influence social attitudes toward abuse.

In high-income settings, most research has focused on law enforcement rather than political institutions. Studies show that female police officers are more likely to take domestic abuse seriously, increasing both reporting and follow-up (Andrews and Johnston Miller, 2013; Miller and Segal, 2019; Wen, 2024). This highlights how the gender composition of institutions can shape both responsiveness and reporting. However, little is known about whether similar effects extend to elected officials – especially at the local level, where many public services are delivered and where local authorities control funding for domestic abuse support, including victim services, housing, and partnerships with external providers. These responsibilities give local governments significant discretion in shaping how abuse is addressed in practice, yet their role in this area remains underexamined.

We contribute to this literature by studying whether female representation in English local government influences domestic abuse. Unlike earlier work focused on developing countries or law enforcement, we examine elected politicians in a high-income setting with decentralised service delivery. Our findings provide evidence on how women's political representation can shape institutional behaviour, affect reporting patterns, and possibly shift community norms in ways that reduce domestic abuse.

2.3 Institutional Background

Local government in England plays a central role in delivering public services, many of which are directly or indirectly related to domestic abuse prevention and response. These include housing, social care, health promotion, and community safety. Councillors, as elected local representatives, influence the development of local policies and the allocation of resources. Understanding the institutional framework within which councillors operate is essential for interpreting how local political representation may affect domestic abuse. In what follows, the terms *local authority*, *local government*, and *local council* are used interchangeably, depending on context.

2.3.1 Local Authorities and Councillor Elections

England's local governance is based on either a two-tier or a unitary (single-tier) system. In the two-tier structure, county councils (upper tier) are responsible for services that cover broader categories, such as education, social care, transport, and environmental and regulatory planning. District, borough, or city councils (lower tier) manage more localised roles, including waste collection, Council Tax, housing, and planning applications. In contrast, unitary authorities combine both levels of responsibility into a single tier of government.[3] London boroughs, metropolitan boroughs, and some shire areas operate under this unitary system, aiming to streamline local service delivery.

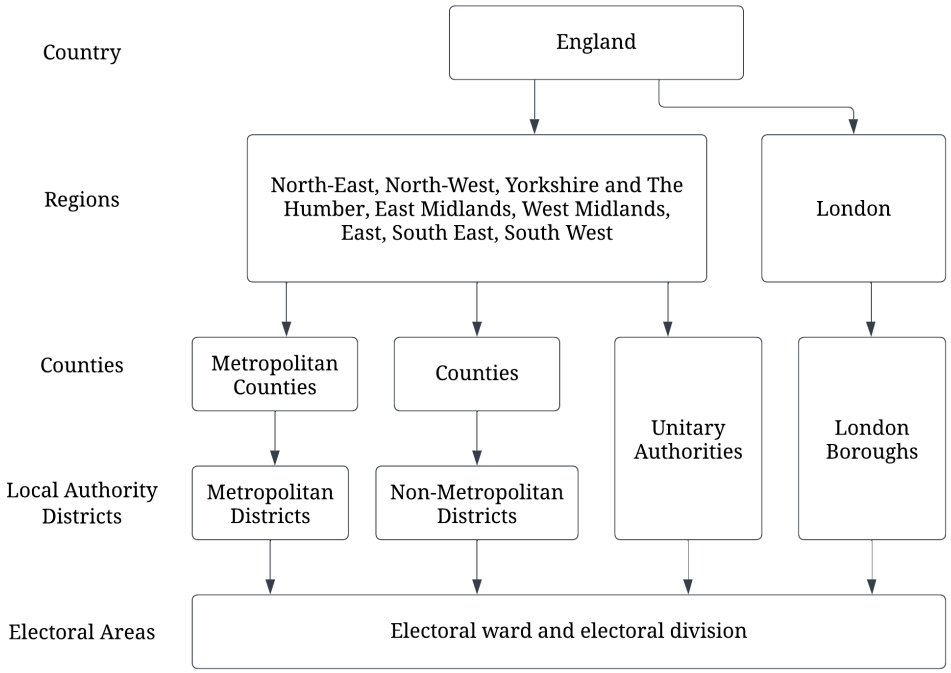

Figure 2.2: Administrative Geography Hierarchy for England
Source: Author's illustration based on the hierarchy of areas relating to national and local government in England. See ons.gov.uk

Figure 2.2 illustrates the administrative geography hierarchy for England, showing how local authorities relate to broader territorial divisions such as regions and counties. Local authorities are further subdivided into electoral areas known as *wards* or *divisions*, which serve as the basic unit for local elections. Depending on its size and local arrangements, each ward normally elects between one and three councillors to represent its constituents.[4] Councillors are elected through a first-past-the-post (FPTP) voting system and typically serve four-year terms. Election cycles vary across councils: some elect all councillors every four years,

[3] See UK Government (n.d.*b*) for details on how council works.
[4] See UK Government (n.d.*a*) for details on local government structure and elections.

others elect half the council every two years, and others follow a staggered model where one-third of councillors are elected each year over a three-year cycle, followed by a fallow year without elections.[5] Additionally, by-elections are held when councillor seats become vacant due to resignation, death, or other reasons.[6]

A specific institutional feature relevant to this study is the alphabetical ordering of candidates' names on local election ballots. According to Schedule 1, Rule 14(3) of the Representation of the People Act 1983, names must appear in alphabetical order by surname (UK Parliament, 1983). Prior research shows that this ordering can influence election outcomes, particularly in low-information elections where voters are less familiar with candidates (Miller and Krosnick, 1998). In later sections, this rule forms the basis for an instrumental variable strategy to probe the causal interpretation of the observed associations between female political representation and domestic abuse.

2.3.2 Police Forces and Crime Reporting Geography

Although domestic abuse often remains under-reported, police recorded crime (PRC) is a key source of information on how abuse is recognised, responded to, and recorded by the criminal justice system. Understanding patterns in what gets reported to the police is therefore crucial, not only for assessing the visibility of abuse, but also for analysing how institutional factors, such as political representation, may influence reporting behaviour. This study incorporates both self-reported and PRC domestic abuse data. Police forces are relevant to this study as they serve as the administrative unit through which domestic abuse incidents are officially recorded, yet their jurisdictions do not always align with local authority boundaries.

There are 39 police constables and territorial police forces in England, each responsible for law enforcement across a specific geographic area.[7] Since 2015, police forces have been required by the Home Office to flag whether a recorded offence is domestic abuse-related, in line with recommendations from the 2014 report by Her Majesty's Inspectorate of Constabulary.[8] These flags form the basis of the Police Recorded Crime (PRC) data used in this study.

[5] See The Electoral Commission (n.d.*b*) for further information.
[6] See The Electoral Commission (n.d.*a*) for details on local council by-elections.
[7] A full list of police forces is available at Police.uk (n.d.).
[8] See HMICFRS (2014) for details on improvements in the police response to domestic abuse.

However, a complication arises from the fact that police force boundaries do not align with local authority areas. As shown in Figure 2.3, the Thames Valley Police Force Area spans nine distinct local councils: Bracknell Forest, Buckinghamshire, Milton Keynes, Oxfordshire, Reading, Slough, West Berkshire, Windsor and Maidenhead, and Wokingham. Because political representation is organised at the local authority level, while domestic abuse is recorded at the police force level, careful alignment is needed when using administrative data to study political effects on reported abuse.

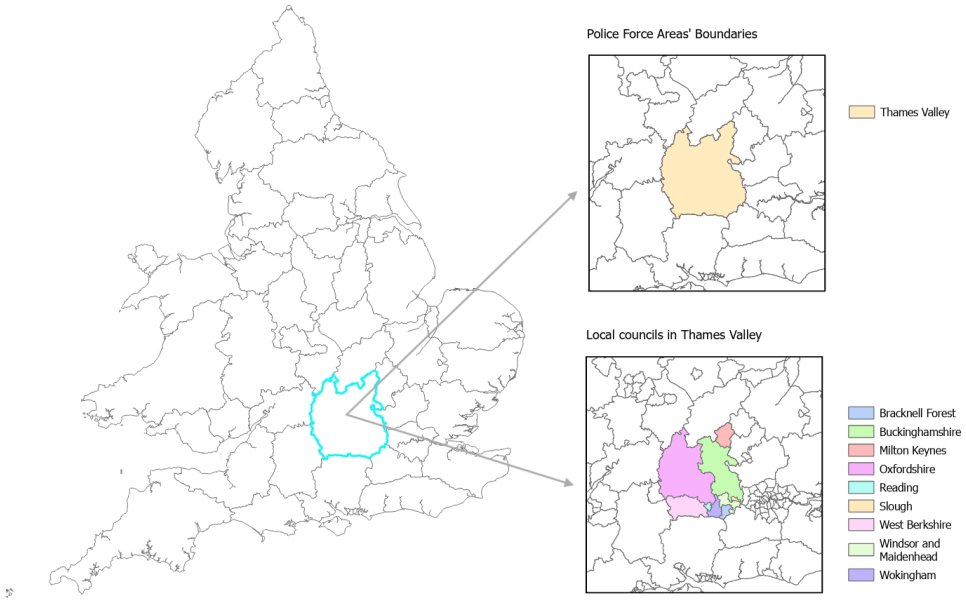

Figure 2.3: Comparative Maps of Police Force Areas and Local Councils in Thames Valley, England
Notes: Left graph: Thames Valley Police Force Area (PFA) within England. Top right: close-up of Thames Valley PFA. Bottom right: nine local councils within Thames Valley, illustrating distinct local councils' boundaries from the PFA.

To enable analysis at the local authority level, we rely on PRC domestic abuse-related data that have been disaggregated by local authority area. In cases where the boundaries of police forces and local authorities do not overlap, this requires additional effort to match datasets appropriately. Further details on data sources and the construction of local-level measures are provided in the following section.

2.4 Data

This section outlines the data sources used in our analysis. We begin by describing the construction of a novel dataset on local councillors in England, which allows us to measure the

gender composition of local government. We then introduce two complementary measures of domestic abuse: administrative records from police forces and self-reported experiences from the Crime Survey for England and Wales (CSEW). Finally, we describe additional data sources that provide contextual or suggestive information used to explore potential mechanisms and control for confounding factors.

2.4.1 Councillors Data

Our councillor dataset spans from 2016 to 2022 and provides a panel of gender and party composition for each of the 152 upper-tier local authorities in England.[9] This dataset includes all councillors elected through regular elections and by-elections, amounting to a total of 64,502 individual observations. Based on this, we construct a key explanatory variable for the analysis: the proportion of female councillors in each local authority in each year. This variable captures both cross-sectional variation across councils and temporal variation within each council over time, allowing for a rich exploration of the relationship between female representation and domestic abuse prevalence and reporting.

To construct this dataset, we combined information from two open-access sources: Open Council Data UK and the Local Elections Archive Project (LEAP). Open Council Data UK scrapes local council websites annually, shortly after the May election cycle, to retrieve councillors' names, party affiliations, and wards.[10] LEAP offers historical election results for all local elections in UK.[11] Using both sources allowed us to cross-validate the information and fill in missing or inconsistent data. Because Open Council Data UK is updated annually and captures both regular and interim by-election results, it ensures our panel is sensitive to short-term changes in council composition.

To infer the gender of councillors, we used the `gender` R package, which estimates the probability that a first name is female based on historical name-gender distributions drawn from sources such as the U.S. Social Security Administration, the US Census, and the North Atlantic Population Project (Mullen, 2021). Each first name receives a probability score from 0 to 1, with 0 indicating male and 1 indicating female. For cases where the gender probability was between 0.1 and 0.9 – accounting for 6.23% of all councillor observations – we manually verified the individual's gender by checking council websites and social media profiles

[9] For a list of the 152 local authorities, see Table B.1 in the Appendix.
[10] The data are drawn from Open Council Data UK (n.d.).
[11] Election results are compiled from the Local Elections Archive Project (Teale, n.d.).

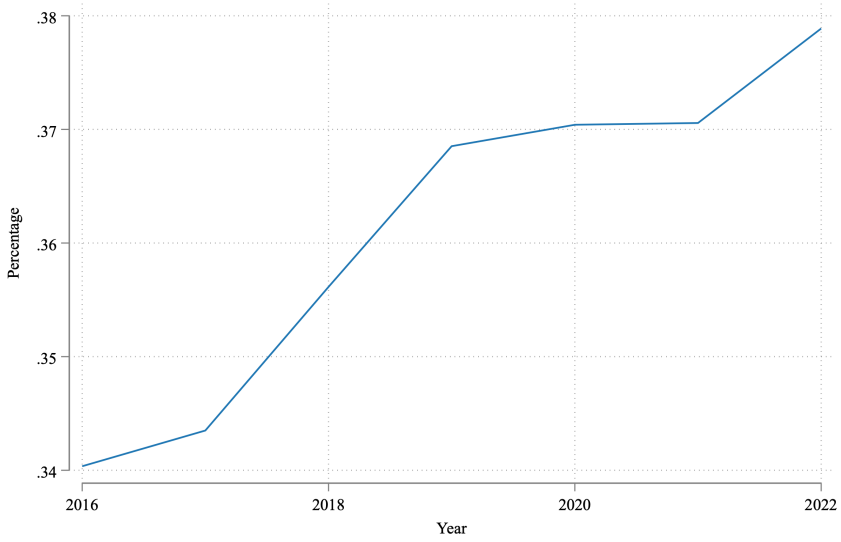

Figure 2.4: Percentage of Female Councillors Nationally Over Time

including LinkedIn, Facebook, and Twitter. This process ensures a high degree of accuracy in our gender classification.

Based on these gender classifications, we calculated the proportion of councillors who are female in each local authority and year. Figure 2.4 shows the national trend from 2016 to 2022, illustrating a steady increase in female representation over time. Despite this upward trend, women remain underrepresented relative to men in most local governments. Figure 2.5 illustrates variation within the Thames Valley police force area, plotting the trend of female representation in its nine local authorities (left panel) alongside a map of their geographic boundaries (right panel). This example demonstrates both the diversity of gender composition across localities and its change over time.

To further illustrate spatial and temporal variation, Figure 2.6 presents average female representation and growth rates across councils between 2016 and 2020. The left panel displays the average share of female councillors per council during this period, while the right panel plots the growth rate, with negative values indicating declines in female representation. Although national trends point toward increasing gender parity, these figures reveal that some councils have moved in the opposite direction, underscoring the importance of analysing local-level variation. Political affiliation also varies across councils: more than 90% of councillors are affiliated with the Labour, Conservative, or Liberal Democrat parties (Copus, 2004), with Figure B.1 in Appendix showing an increase in Conservative control between 2016 and 2021.

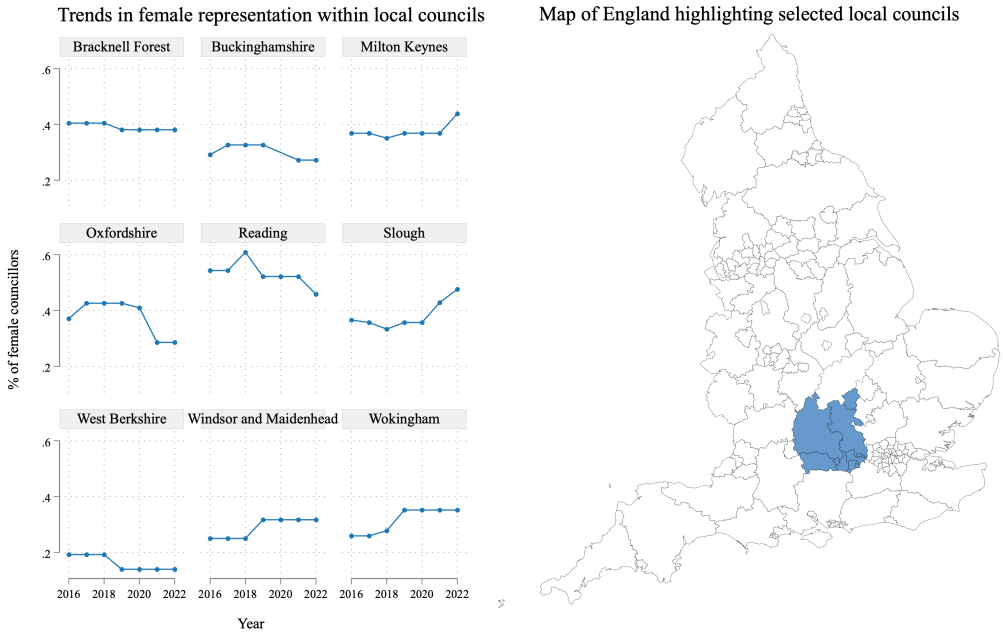

Figure 2.5: Time Trends of Female Councillor Representation Across Selected Local Councils
Notes: The line graphs on the left correspond to local councils highlighted in blue on the right.

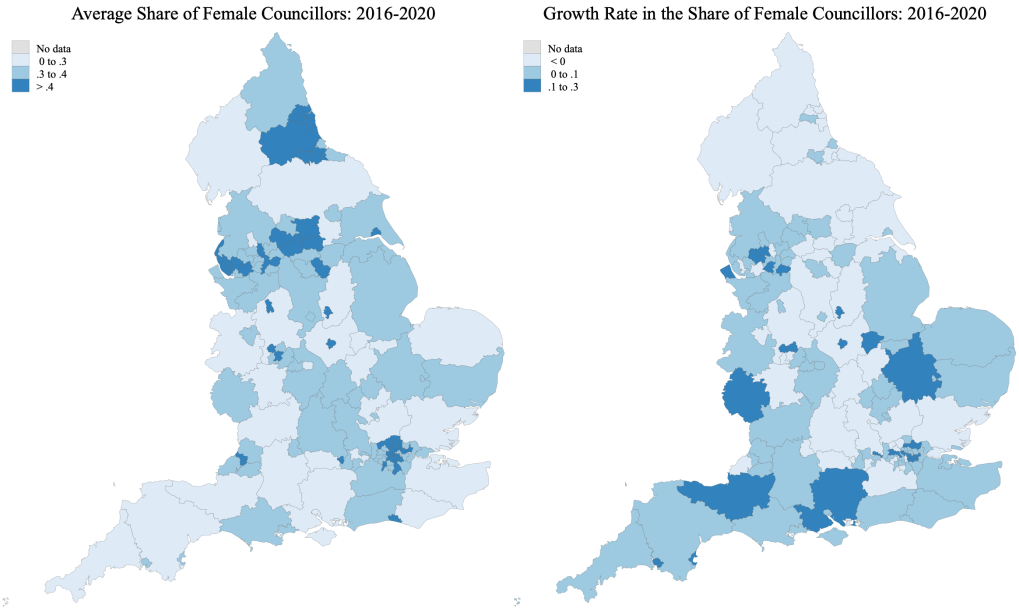

Figure 2.6: Growth and Average Percentage of Female Councillors Over Time

Overall, this councillors' dataset forms the basis for measuring female representation in local government. In subsequent sections, we examine whether and how variation in this representation relates to the prevalence and reporting of domestic abuse.

2.4.2 Domestic Abuse Measure

This section introduces the two key sources of domestic abuse data used in this study: police-recorded incidents and individual self-reports. These datasets offer complementary insights. Police recorded crime (PRC) data reflect reported and documented cases of abuse as processed through the criminal justice system. In contrast, self-reported data from the Crime Survey for England and Wales (CSEW) allow for the analysis of under-reported experiences of abuse, providing a broader picture of prevalence. While each dataset has limitations, taken together, they offer a more complete understanding of how domestic abuse is experienced, recognised, and reported in England.

Police Recorded Crime (PRC) Data

Police recorded domestic abuse data are drawn from administrative sources and reflect incidents and crimes that have been formally reported to the police. Since April 2015, following recommendations in the 2014 *HMICFRS* report on improving the police response to domestic abuse, police forces in England have been required to flag crimes as domestic abuse-related if they meet the relevant statutory definition.[12] These data are publicly available at the level of police force areas, which do not always align with local government boundaries.[13]

To enable consistent comparison with local authority-level political representation, we requested disaggregated statistics on domestic abuse-related incidents and crimes from individual police forces. For ten police forces whose jurisdiction coincides with a single local authority, this data was directly usable.[14] For the remaining 29 forces, we submitted Freedom of Information (FOI) requests seeking domestic abuse data disaggregated to the local authority level. Appendix Table B.2 summarises the responses and coverage obtained. These efforts allow us to analyse the prevalence of police recorded domestic abuse in a way that corresponds with the boundaries of elected local governments.

[12] In response to the report *Everyone's Business: Improving the Police Response to Domestic Abuse* (2014), the Home Office directed police forces to systematically flag domestic abuse cases. See HMICFRS (2014).

[13] Data are drawn from the ONS domestic abuse data tool (Office for National Statistics, 2025*a*).

[14] The ten police forces that coincide with local authority boundaries include Cumbria, Warwickshire, Lincolnshire, Northamptonshire, Norfolk, Suffolk, Hertfordshire, Surrey, City of London, and Gloucestershire.

Domestic abuse-related data reported by police are typically categorised into two types: incidents and crimes. Incidents include all reported behaviours falling under the broad definition of domestic abuse – such as threatening behaviour or emotional abuse – even if they do not meet the legal threshold for a criminal offence. Crimes, on the other hand, refer to cases where a notifiable offence has been recorded following investigation.[15] Both measures are flagged as "domestic abuse-related" under the National Crime Recording Standard (NCRS) and National Standard for Incident Recording (NSIR) protocols.[16]

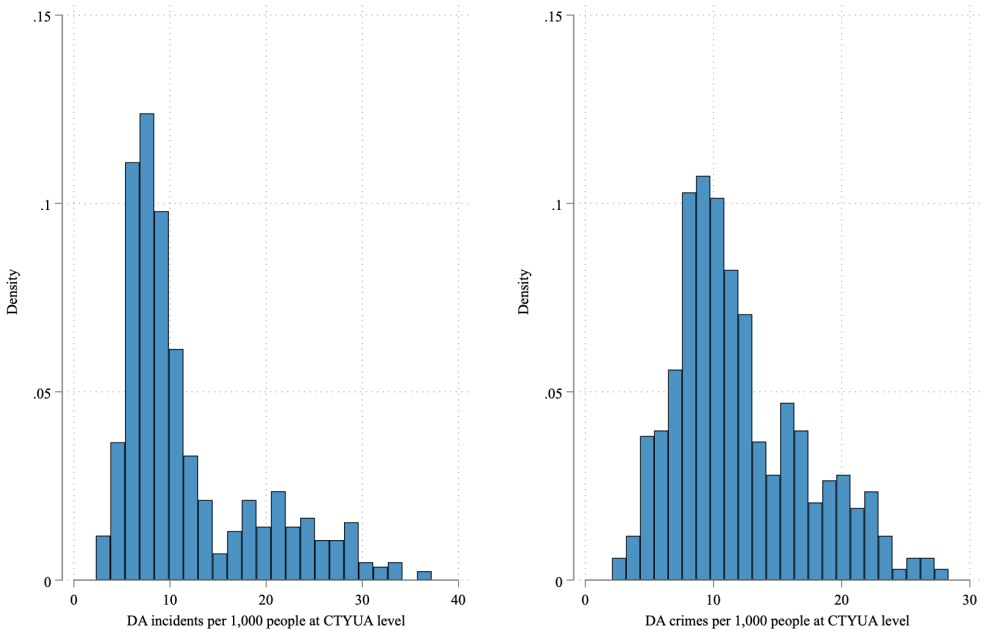

Figure 2.7: Police Recorded Domestic Abuse-related Incidents and Crimes at Local Authority Level

We calculate the rate of police recorded domestic abuse-related incidents and crimes per 1,000 residents using mid-year population estimates for each local authority. Figure 2.7 presents histograms illustrating the distributions of these rates. Both distributions are right-skewed, indicating that while most local authorities report relatively low rates, a few areas show markedly higher levels of recorded domestic abuse. The distribution for crimes extends further to the right, reflecting greater variability in recorded criminal offences compared to incidents. Considering both types of data is essential to understanding variation in reporting

[15] An example of a domestic abuse incident that does not amount to a crime would be two family members having a loud argument, a third party calls the police, the police attend and calm the situation down, but no crime has taken place. More information on domestic abuse-related incidents and crimes can be found in Office for National Statistics (2019).

[16] The ACPO defines Domestic Abuse as follows: "Any incident of threatening behaviour, violence or abuse (psychological, physical, sexual, financial or emotional) between adults, aged 18 or over, who are or have been intimate partners or family members, regardless of gender and sexuality." Family members are defined as mother, father, son, daughter, brother, sister, and grandparents, whether directly related, in-laws, or stepfamily. This definition is operationalised through standardised flagging procedures in police databases. See Home Office (2011) for further information.

behaviour and the administrative response to domestic abuse across England.

Crime Survey for England and Wales (CSEW) Self-Reported Data

While police-recorded crime data reflect reported and documented abuse, they do not capture the full extent of domestic abuse in England. Prior research has shown that many incidents go unreported due to fear, stigma, or mistrust of law enforcement authorities (Felson and Paré, 2005; Voce and Boxxall, 2018). To account for this underreporting, we complement administrative data with self-reported experiences of domestic abuse from the Crime Survey for England and Wales (CSEW), one of the most comprehensive and reliable sources of crime victimisation data in the UK.

The CSEW is a large-scale, nationally representative household survey conducted annually, which gathers data on victims' experiences of crime, including those not reported to the police. One adult from each sampled household is randomly selected to complete the survey, and responses concerning intimate partner violence (IPV) and other sensitive topics are gathered through a self-completion module on a laptop. This approach helps ensure respondent confidentiality and encourages more candid reporting.[17]

For the purpose of this study, we focus on four years of data spanning 2016 to 2019, which are the most recent years for which both the IPV module and corresponding geographic identifiers are available. Our analysis draws primarily from the intimate personal violence (IPV) module, which asks detailed questions about emotional, physical, and sexual abuse, as well as coercive and controlling behaviours by partners or ex-partners. Respondents are asked whether they have experienced any of these forms of abuse since the age of 16 and within the past 12 months. We construct three key outcome measures from this module: Any Abuse (physical or non-physical); Physical Abuse (e.g., hitting, slapping, or forced sexual activity); Non-Physical Abuse (e.g., controlling behaviour, emotional manipulation). Appendix Table B.3 outlines the full set of survey questions used to construct these variables. These distinctions are important, as our results suggest differential patterns across abuse types when analysed against changes in local political representation.

From 2016 to 2019, approximately 55,000 individuals completed the IPV module. Among these, about 5% reported experiencing abuse from a partner or ex-partner in the past 12

[17] These data are accessed under Secure Access conditions. See ONS (2021).

months, and around 21% reported having experienced abuse at some point since age 16. These figures mask important gender disparities: nearly 28% of female respondents reported abuse since age 16, compared to just 12% of male respondents. This disparity is particularly pronounced in emotional and controlling forms of abuse.

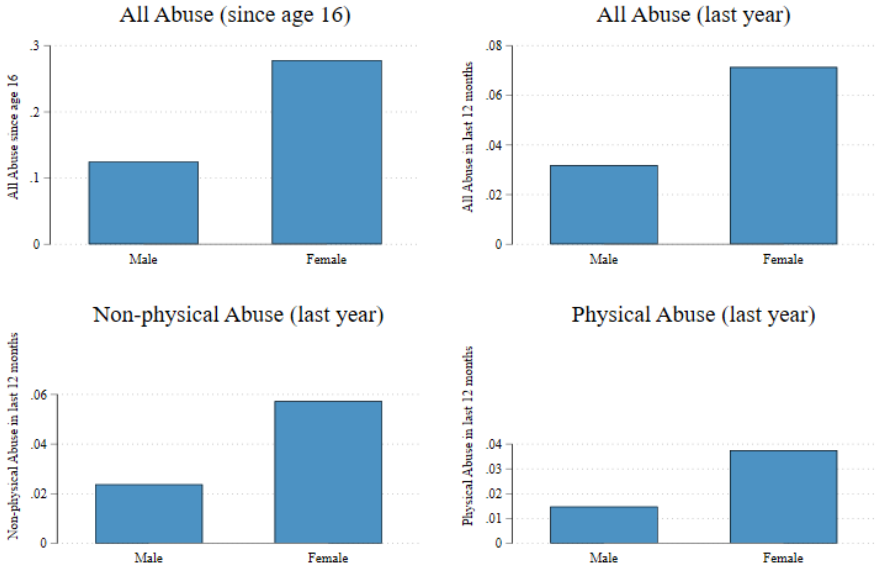

Figure 2.8: Gender Differences in Domestic Abuse Experiences

Notes: Data derived from Office for National Statistics. 2021. *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *11th Edition.* UK Data Service. SN: 7280, http://doi.org/10.5255/UKDA-SN-7280-11

Figure 2.8 illustrates the gendered nature of domestic abuse across several measures: experiences of any abuse since age 16, any abuse in the past year, and breakdowns into physical and non-physical abuse. The differences between male and female responses are stark, highlighting the importance of a gender-specific analytical approach. For this reason, our empirical analysis of self-reported data focuses primarily on female respondents, who constitute the majority of domestic abuse victims and exhibit the clearest variation in relation to political representation.

In addition to the IPV module, the CSEW provides rich socio-demographic information on respondents, including age, education, employment, household composition, ethnicity, and other characteristics. This allows us to control for a range of potential confounders in our empirical models. Summary statistics for the full sample are provided in Table B.6, with breakdowns by gender in Tables B.7 and B.8 in the Appendix. These descriptive statistics offer important context for interpreting the relationship between domestic abuse prevalence and the gender composition of local government.

2.4.3 Other Complementary Data

To support a comprehensive analysis of the relationship between female representation in local government and domestic abuse, this study incorporates several complementary datasets. These data sources serve two key purposes: (i) they control for demographic, socio-economic, and institutional differences across areas, and (ii) they provide contextual variables used in exploring potential channels behind the observed associations.

Police Workforce Data. We use data from the Police Workforce Open Data tables, which provide detailed demographic breakdowns for each of the 43 territorial police forces in England and Wales, plus the British Transport Police.[18] This includes counts and percentages by gender and ethnicity across various roles – such as officers, civilian staff, community support officers, designated officers, special constables, and volunteers. Given the central role of policing in domestic abuse response, the gender composition of local police forces may be relevant both as a control and as a contextual variable in understanding variation in reporting behaviour or institutional support for victims.

Population Data. We use mid-year population estimates from the Office for National Statistics (ONS) to account for the demographic composition of local areas.[19] These include total population, gender breakdowns, age structure, and population density. We also include area-level ethnic composition. These indicators are essential for controlling for heterogeneity in local population profiles, which may affect both domestic abuse prevalence and local governance dynamics.

Annual Population Survey (APS) Data. The Annual Population Survey provides detailed local labour market indicators, disaggregated by gender and ethnicity.[20] We include variables such as economic activity rates, employment and unemployment rates, the share of self-employment, and levels of economic inactivity. These data are important for capturing the socio-economic environment at the local level, which may influence both victim vulnerability and the broader institutional capacity to respond to domestic abuse.

[18] Data are drawn from the Home Office police workforce open data tables (Home Office, 2016).
[19] Population estimates are obtained from Office for National Statistics (2025*b*).
[20] Labour market statistics are obtained from the Nomis database (Office for National Statistics (n.d.)).

Local Authority Financing Data. We include data on local government revenue and expenditure to explore potential channels through which female representation may influence domestic abuse outcomes. These data are drawn from the Ministry of Housing, Communities and Local Government's revenue expenditure and financing collections.[21] The Revenue Account Budget provides planned income and expenditure for each fiscal year. A key measure is *Net Current Expenditure*, defined as total operational spending minus service-related income (e.g., fees, grants). We collect expenditures across various categories, with particular attention to public health, housing, and social care–all of which may play a role in supporting domestic abuse victims or addressing associated risks. While this financial data is not used in our main causal analysis, it is important in our exploration of possible policy channels. Descriptive statistics are presented separately in Appendix Table B.9.

Together, these complementary datasets allow us to control for area-level confounders and explore the broader institutional and socio-economic context in which local political representation may influence domestic abuse. The next section outlines our empirical strategy and presents the main results.

2.5 Empirical Analysis

This section presents our empirical findings on the relationship between female councillor representation and domestic abuse. We begin with police-recorded data to establish the association between female political representation and reported domestic abuse rates. We then examine individual-level survey data to understand how female councillor presence affects actual experiences of domestic abuse. The two data sources provide complementary perspectives on this relationship, revealing important differences between reporting behaviour and underlying abuse prevalence.

2.5.1 Police Recorded Domestic Abuse

We first examine how female representation in local government relates to police-recorded domestic abuse in the corresponding area. Our empirical strategy exploits variation in the gender composition of local councils both across areas and over time. The baseline specifi-

[21] Local government finance data are drawn from the local authority revenue expenditure and financing datasets (Department for Levelling Up, Housing and Communities, 2025*b*).

cation takes the following form:

$$DA_{jpt} = \beta_0 + \beta_1 \times FemaleCllr_{jpt} + X_{jpt} + W_{pt} + \alpha_p + \gamma_t + \epsilon_{jpt} \tag{2.1}$$

where j denotes local authority areas, p represents police force area (PFA), and t indicates the year. The outcome variable DA_{jpt} measures domestic abuse rates in each local authority j, situated in police force area p, at time t. We express all measures as domestic abuse-related incidents and crimes per 1,000 population. The primary explanatory variable $FemaleCllr_{jpt}$ represents the percentage of female councillors in local authority j within police force area p at time t. The parameter β_1 captures the association between female representation and domestic abuse reporting.

Our specification includes control variables at both the police force level (W_{pt}) and local authority level (X_{jt}). Police force controls include the percentage of female police officers, total police staff, age composition, ethnic composition, absence rate, join rate, leave rate, and promotion rate of police staff. Local authority controls account for the size of the local government, employment rates for both females and males, economic activity rate, population density, sex ratio, and ethnic composition. The model also incorporates police force fixed effects (α_p) to account for time-invariant factors within each police force area. This approach enables us to identify effects from changes in female councillor representation and domestic abuse cases within police force areas over time, while controlling for persistent differences in policing practices or local characteristics. We also include year fixed effects (γ_t) to control for national trends in domestic abuse reporting or policy changes.

Table 2.1 presents the results from this analysis. We find a positive and statistically significant association between female councillor representation and police-recorded domestic abuse incidents and crimes. A one percentage point increase in female councillor share corresponds to approximately 0.14 additional incidents and crimes per 1,000 population when including the full set of controls and fixed effects (Column 3, Panel A). This relationship remains robust across different specifications and when examining incidents and crimes separately (Panels B and C).

These results suggest that victims are more likely to report domestic abuse to police in areas with higher female councillor representation. However, it is important to note that police-recorded data capture only reported cases and cannot inform us about the overall level of

domestic abuse in these communities. To understand whether this increase in reporting reflects changes in actual abuse prevalence or reporting behaviour, we turn to individual-level survey data in the following section.

Table 2.1: Female Councillors and Police Recorded Domestic Abuse

	(1)	(2)	(3)
Panel A: Domestic Abused-Related Incidents and Crimes			
Share of Female Councillors	0.313***	0.267***	0.140**
	(0.107)	(0.065)	(0.058)
Observations	573	573	562
R-squared	0.049	0.787	0.860
Panel B: Domestic Abused-Related Incidents			
Share of Female Councillors	0.154**	0.140***	0.072*
	(0.074)	(0.039)	(0.037)
Observations	577	577	566
R-squared	0.027	0.835	0.881
Panel C: Domestic Abused-Related Crimes			
Share of Female Councillors	0.154***	0.120***	0.062**
	(0.042)	(0.031)	(0.026)
Observations	637	637	626
R-squared	0.059	0.669	0.802
Controls			✓
PFA FE		✓	✓
Year FE		✓	✓

Notes: The primary explanatory variable represents the percentage of female councillors in each local council, ranging from 0 to 100. Control variables include: the size of the local government; the percentage of female police officers, total police staff, age composition, ethnic composition, absence rate, join rate, leave rate, and promotion rate in police forces; Employment rate (and for females), economic active rate (and for females), population density, sex ratio, and ethnic composition in local areas. The model includes regional and year-fixed effects. Standard error adjusted for 136 clusters (140 clusters for Domestic Abuse-related Crimes) in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

2.5.2 Self-Reported Domestic Abuse

The police-recorded data show increased reporting in areas with more female councillors, but this may reflect either higher actual abuse rates or greater willingness to report. To distinguish between these possibilities, we examine individual-level data from the Crime Survey for England and Wales (CSEW), which captures both reported and unreported experiences of domestic abuse.

We estimate the impact of female councillor representation on individuals' experiences of

domestic abuse using the following specification:

$$DA_{ijt} = \beta_0 + \beta_1 \times FemaleCllr_{jt} + X_{ijt} + Z_j + \alpha_j + \gamma_t + \epsilon_{ijt} \tag{2.2}$$

where i denotes individuals, j denotes local authorities, and t indicates year. The dependent variable DA_{ijt} is a binary indicator equal to one if individual i in local authority j experienced domestic abuse in year t, and zero otherwise. The key explanatory variable $FemaleCllr_{jt}$ represents the proportion of female councillors in the local council, ranging from 0 to 1. Since CSEW interviews occur throughout the year, we adjust the female councillor share based on interview timing to ensure alignment with each respondent's 12-month reference period, accounting for council composition changes following May elections.[22] The model includes individual-level controls (X_{ijt}) and local authority characteristics (Z_j), including: age, marital and cohabitation status, household reference person status, urban residency, employment (part- or full-time), education, nationality, birthplace, ethnicity, health condition, and household composition (number of adults and children). We also control for region fixed effects (α_j) and year fixed effects (γ_t).

Given the substantial gender differences in domestic abuse prevalence shown in Figure 2.8, we begin by focusing on female respondents, as women report significantly higher rates of abuse than men, and prior research suggests that female political representation may have differential effects by victim gender (Iyer et al., 2012). Table 2.2 presents results for female respondents across three categories of abuse, with each panel showing progressively saturated specifications. Panel A examines any domestic abuse in the past 12 months, Panel B focuses on physical abuse, and Panel C on non-physical abuse. Across all specifications, female councillor representation is negatively associated with women's experiences of domestic abuse. The effect is strongest for non-physical abuse: in the fully specified model, a ten percentage point increase in female councillor share is associated with a 0.63 percentage point reduction in the probability of experiencing such abuse. This represents an 11% decline relative to the baseline prevalence of 5.73%, indicating an economically meaningful effect.

For completeness and to echo the full population analysis in police-recorded domestic abuse cases, Table B.11 in Appendix presents results for all respondents combined. The negative

[22] For example, for an individual interviewed in July, "the last 12 months" refers to the period from the previous July to the current July. We adjust the female councillor share to reflect the new council composition from May to June of the current year (post-election), and retain the prior composition from July of the previous year to April of the current year (pre-election).

association between female councillor representation and domestic abuse experiences persists when examining all respondents, though the magnitudes are notably smaller than those observed for women alone. In the fully saturated specification, the coefficients lose statistical significance. This pattern aligns with our expectation that female political representation primarily affects outcomes for female victims, while having more modest effects on male victims. Importantly, the negative or insignificant coefficient results in Table B.11 provide a useful comparison with Table 2.1, where we find a positive association, indicating that positive associations do not necessarily imply increases in actual domestic abuse experiences.

Table 2.2: Domestic Abuse Experienced in the Last 12 Months by Female Respondents

	(1)	(2)	(3)	(4)
Panel A: Experienced any abuse by partner in the last 12 months				
% of Female Councillors	-0.0625***	-0.0708***	-0.0740***	-0.0347
	(0.021)	(0.021)	(0.022)	(0.027)
Observations	30,230	23,086	23,086	23,086
Panel B: Experienced physical abuse by partner in the last 12 months				
% of Female Councillors	-0.0177	-0.0208	-0.0239	0.0073
	(0.014)	(0.015)	(0.015)	(0.021)
Observations	30,189	23,062	23,062	23,062
Panel C: Experienced non-physical abuse by partner in the last 12 months				
% of Female Councillors	-0.0601***	-0.0788***	-0.0798***	-0.0633***
	(0.018)	(0.018)	(0.018)	(0.022)
Observations	30,209	23,071	23,071	23,071
Control		✓	✓	✓
Year FE			✓	✓
Regional FE				✓

Notes: In this regression analysis for female respondents, the explanatory variable is the proportion of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. The control variables, along with year and regional-fixed effects, are gradually included in the model. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12

Given the strong effects on non-physical abuse, we next examine which specific behaviours are most affected. Table 2.3 presents disaggregated results using data from 2017 and 2018, when the CSEW included detailed questions on coercive and controlling behaviours.[23] This analysis helps identify the mechanisms that may drive the overall decline in non-physical

[23]These include: unfairly controlling money or spending; isolating the respondent from friends and family; monitoring communication; enforcing humiliating rules or activities; controlling household work or childcare; tracking movements or time use; intimidation such as punching walls or destroying property; forcing sex or specific sexual acts; threatening to harm children; and stalking. See ONS (2021) for further information.

abuse. Monitoring and controlling behaviours show particularly strong negative associations with female representation.

Table 2.3: Detailed Categories of Domestic Abuse Experienced by Women from Partners in the Last 12 Months

	(1) Ctrl money	(2) Isolate	(3) Monitor	(4) Rules	(5) Housework
% of Female Cllr	-0.0095	-0.0554***	-0.0591**	-0.0109	-0.0152
	(0.017)	(0.020)	(0.024)	(0.011)	(0.016)
Observations	7,515	7,515	7,515	7,515	7,515
	(6) Time	(7) Intimidate	(8) Force sex	(9) Children	(10) Stalking
% of Female Cllr	-0.0517*	-0.0199	0.0056	-0.0063	-0.0204**
	(0.030)	(0.025)	(0.016)	(0.006)	(0.009)
Observations	7,515	7,515	7,515	7,515	22,966
Control	✓	✓	✓	✓	✓

Notes: Please note that these detailed categories are only present in the CSEW data for years 2017 and 2018. In this analysis for female respondents, the explanatory variable is the proportion of female councillors in each local council, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

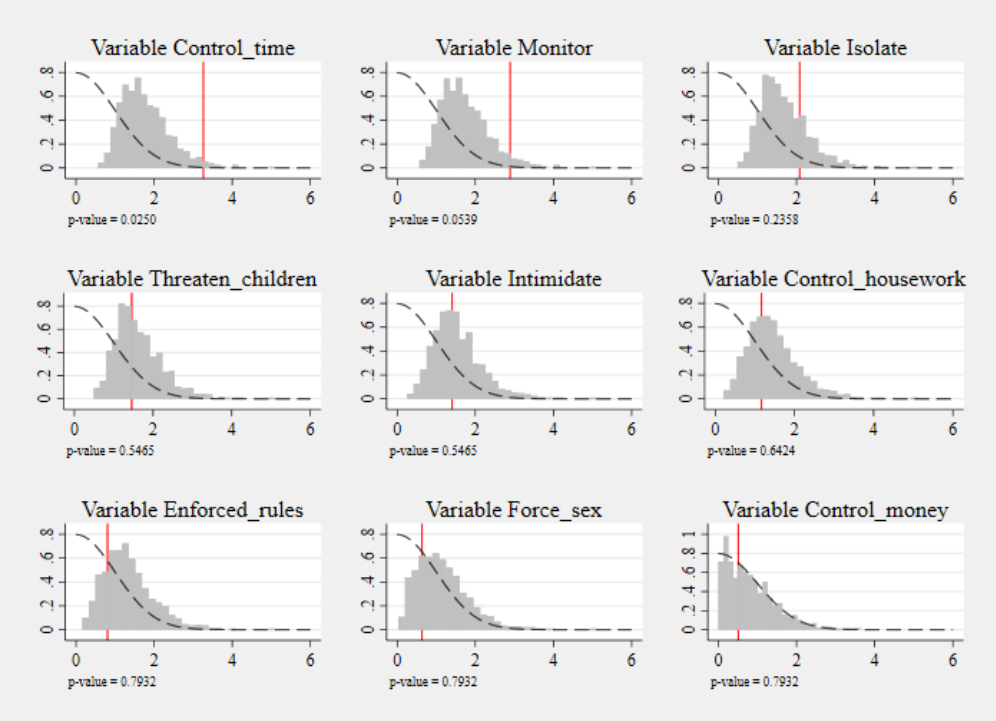

Figure 2.9: Results of Romano-Wolf Multiple-Hypothesis Testing

Notes: The underlying observations in each chart in this figure are 7,515. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12

To address the risk of false positives due to multiple testing, we apply the Romano-Wolf multiple-hypothesis testing to control the Family-Wise Error Rate (FWER) across the nine detailed abuse outcomes (Clarke et al., 2020). Figure 2.9 presents these results. Associations with monitoring and time controlling remain statistically significant after correction, suggesting these are the most robust dimensions of the relationship between female political representation and reduced non-physical abuse.

The contrast between police-recorded and self-reported data provides important insight into the effects of female political representation on domestic abuse. The police data, which cover the full population and include both incidents and crimes, show a positive association between female councillor share and recorded cases. In contrast, the self-reported survey data, based on all respondents, show no significant change overall. Taken together, these patterns suggest that higher recorded cases reflect increased reporting rather than higher actual abuse rates. This interpretation is supported by the negative association between female representation and non-physical abuse among women, indicating a potential decline in actual abuse. Meanwhile, physical abuse – which is more likely to be reported to police – shows no significant change. Since the police data do not distinguish abuse types, the absence of change in self-reported physical abuse reinforces the view that improved reporting, rather than increased prevalence, is driving the rise in police-recorded cases. These findings underscore the value of combining administrative and survey data to assess the broader impact of political representation on sensitive outcomes such as domestic abuse.

2.5.3 Placebo Tests

This section presents placebo tests that assess whether the main findings are driven by unrelated patterns in victimisation or reporting, rather than a true relationship between female representation and domestic abuse. These tests apply the same empirical strategies to alternative outcomes and subpopulations where no effect is theoretically expected. If female councillor representation were correlated with a general reduction in crime, we would expect to observe effects across these placebo outcomes as well. And no such effects will strengthen the interpretation that the observed associations are specific to domestic abuse against women by intimate partners.

Male Victims of Partner Abuse. We first test whether the presence of female councillors affects the incidence of domestic abuse reported by male respondents in the CSEW. This

serves as a placebo because the primary channels through which female political representation might reduce domestic abuse, such as shifts in gender norms or support services, are likely to benefit female victims more directly. Using the same specification as in the main analysis (equation (2.2)), Table B.12 shows no significant effect of female councillor share on partner abuse reported by men. This null result supports the interpretation that the observed reductions are specific to female victims.

Abuse by Family Members. Next, we examine whether female political representation affects reports of domestic abuse perpetrated by family members other than partners. If the effects of female councillors reflect broader violence changes in the family, we might expect to see reductions in these outcomes as well. However, Table B.13 shows no overall statistically significant association. This suggests that the observed effects do not generalise to all forms of domestic abuse, and are more likely driven by mechanisms specific to intimate partner relationships, such as coercion, empowerment, or changes in reporting behaviour.

Police-Recorded Non-Domestic Crimes. We also test whether female councillor representation correlates with police-recorded crime categories unrelated to domestic abuse. Using publicly available street-level crime data from DATA.POLICE.UK, we construct annual crime rates per 1,000 population at the local authority level. Crime types include anti-social behaviour, public order violations, robbery, burglary, criminal damage and arson, vehicle crime, drug-related offences, theft from the person, and other crimes. These crimes are unlikely to be influenced by female representation in local councils, and serve as a placebo for broader shifts in policing or reporting. As shown in Table B.14, we find no significant association between female councillor share and these crimes, reducing concerns that our main results reflect general crime trends.

Self-Reported Non-Domestic Victimisation. Finally, we examine whether female councillor representation affects broader categories of self-reported victimisation using the CSEW Victim Module. This includes experiences such as theft, vehicle-related crime, property damage, and cybercrime. These serve as placebo outcomes to test whether female representation is correlated with perceived safety or victimisation more generally. The regression results, reported in Table B.15, show no significant relationships across these outcomes. A full list of questions in CSEW Victim Module is provided in Table B.4. Again, the absence of effects reinforces the interpretation that the main findings are specific to domestic abuse by partners.

The results of these placebo tests indicate that the relationship between female political representation and domestic abuse is not driven by broader patterns in crime reporting or general victimisation. Across all alternative outcomes, we find no significant associations. This reinforces the interpretation that the observed effects are specific to intimate partner violence against women. The divergence between police-recorded and self-reported measures documented in Sections 2.5.1 and 2.5.2 suggests that female representation increases the visibility of abuse in official statistics rather than the underlying prevalence. In the following sections, we examine evidence consistent with this reporting mechanism (Section 2.6) and probe the causal interpretation of the main findings using instrumental variable analysis (Section 2.7).

2.6 Evidence for Increased Reporting

The empirical findings presented in Section 2.5 reveal a striking divergence between police-recorded and self-reported measures of domestic abuse. Whilst police records show a positive association between female councillor representation and reported domestic abuse incidents, individual-level survey data indicate that women in areas with more female councillors are less likely to report experiencing abuse in the past year. This contrast suggests that female representation does not increase the actual prevalence of domestic abuse, but rather is associated with increased reporting to and recording by authorities. We examine two channels and one institutional moderator consistent with this reporting interpretation. First, we show that women in areas with greater female representation are more likely to self-report incidents to police, suggesting that female councillors may foster environments in which women feel more empowered to come forward (Section 2.6.1). Second, mediation analysis demonstrates that councils with more female councillors allocate greater resources to public health services, including programmes relevant to domestic abuse, and that this funding is positively associated with police-recorded cases (Section 2.6.2). Finally, we examine heterogeneity across councils with and without statutory partnerships with specialist domestic abuse support organisations, finding that such partnerships moderate the relationship between female representation and police recording in a manner consistent with specialist services providing alternative reporting pathways (Section 2.6.3).

2.6.1 Female Empowerment and Victim Reporting

We begin by examining the most direct evidence for the reporting mechanism: whether female political representation is associated with victims' willingness to report incidents to police. One plausible channel is through shifts in community norms and perceived legitimacy of reporting, particularly for women. The presence of women in leadership may signal institutional responsiveness and foster greater trust in authorities. Using data from the Victim Module of the Crime Survey for England and Wales (CSEW), we examine whether individuals in areas with more female councillors are more likely to have incidents known to police. Table 2.4 presents regression results where the dependent variable captures whether the police were notified about an incident.

Table 2.4: Police Notification Regarding Incidents Involving Individuals

	(1) Police Notified	(2) Notification from Victims	(3) Notification from Female Victims	(4) Notification from Male Victims
% of Female Cllr	0.0474	0.1921**	0.2471**	0.1461
	(0.069)	(0.082)	(0.117)	(0.123)
Observations	16,685	6,154	3,257	2,897

Notes: In these models, the dependent variable is whether police know about the incidents that happened to respondents, the explanatory variable is the share of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables are included in these models – the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Column (1) shows no significant effect of female councillor share on whether the police are aware of incidents overall. This is unsurprising, as police may learn about incidents through various channels, including routine patrols, third-party reports, or emergency calls from witnesses or neighbours. However, Column (2) reveals a positive and statistically significant association between female political representation and victim-initiated reporting. A ten percentage point increase in female representation is associated with approximately a 1.9 percentage point increase in the likelihood that victims themselves notify police about incidents. This effect is driven primarily by female victims, as shown in Column (3), where the coefficient is 2.5 percentage points and statistically significant at the 5% level. Column (4) shows no significant effect for male victims, with a coefficient of 1.5 percentage points that is not statistically distinguishable from zero.

Since incidents often occur in private settings and are typically underreported, this increase in self-reporting amongst women is particularly meaningful. It suggests that female councillors may preside over environments in which women feel more empowered to seek help. This pattern is consistent with role model effects (Campbell and Wolbrecht, 2006) and greater perceived legitimacy of local institutions. Importantly, the outcome examined here captures victim reporting of any incidents, not specifically domestic abuse incidents, indicating that the association between female representation and women's willingness to engage with authorities may extend beyond the specific context of intimate partner violence.

2.6.2 Funding Allocation and Policy Priorities

A complementary channel through which female representation may enhance reporting operates through local government funding priorities. In England, local councillors are responsible for key social services, including public health and adult care. These domains contain a range of interventions relevant to domestic abuse, such as specialist victim support services, mental health resources, and alcohol treatment programmes.[24] Motivated by prior work suggesting that female political leaders tend to allocate more resources to social welfare and health-related priorities (Hessami and da Fonseca, 2020), we examine whether increased female representation is associated with shifts in local government funding. We focus on two funding categories: (i) miscellaneous public health services, which include targeted services for victims of domestic abuse, and (ii) total public health expenditure. Both are expressed as spending per 1,000 residents, and data are drawn from local authority revenue accounts (see Section 2.4.3).

To assess whether these funding categories mediate the relationship between female councillor share and reported domestic abuse, we adopt a stepwise regression approach following Baron and Kenny (1986). Figure 2.10 illustrates the underlying framework. First, we estimate the total effect of female representation on domestic abuse-related outcomes (c). Next, we assess whether female councillors are associated with changes in funding allocations (a). Finally, we include the funding variable in the abuse outcome regression to estimate the mediator path (b) and the adjusted direct effect (c'). The product ab captures the indirect (mediation) effect.

[24] Prior research documents the link between alcohol consumption and domestic abuse (Chalfin et al., 2021; Foran and O'Leary, 2008; Klostermann and Fals-Stewart, 2006).

(a) Simple path (b) Path with mediator

Figure 2.10: Path diagrams: (a) Direct path from X to Y, (b) Path from X to Y with mediator M

Table 2.5 presents results from this mediation analysis using police-recorded incidents and crimes as outcomes. Columns (1) and (2) demonstrate that the share of female councillors is positively and significantly associated with both categories of public health spending. A ten percentage point increase in female representation corresponds to an increase of approximately £1.59 per capita in miscellaneous public health services and £4.37 per capita in total public health expenditure, consistent with the hypothesis that female councillors prioritise funding for these services.

Columns (3) and (4) confirm the baseline positive association between female representation and police-recorded domestic abuse when funding variables are excluded. Columns (5) through (8) introduce the public health spending measures as mediators. The coefficients on both funding categories are positive and statistically significant, indicating that areas with higher public health expenditure record more domestic abuse incidents and crimes, conditional on female representation. Importantly, the inclusion of these funding variables attenuates the coefficient on female representation, though it remains positive and significant. The mediation effects quantify the proportion of the total association explained by each funding pathway: miscellaneous public health spending accounts for 13.4% of the effect on abuse-related incidents, whilst total public health spending mediates 20.5% of the effect on incidents and 17.5% on crimes.

These mediation patterns are consistent with a reporting mechanism rather than an increase in actual abuse prevalence. The positive association between public health funding and police-recorded abuse suggests that increased expenditure on services for domestic abuse victims makes incidents more visible in administrative data. Higher spending on public health services may expand the availability of support services, improve outreach to potential victims, and strengthen the capacity of local institutions to identify cases of abuse. These investments, in turn, create pathways through which victims can access help and through which incidents

Table 2.5: Stepwise Regression Results for Mediation Effect: Police Recorded Crime Data

	(1) M1	(2) M2	(3) Incidents	(4) Crimes	(5) Incidents	(6) Crimes	(7) Incidents	(8) Crimes
X	0.159***	0.437**	0.251***	0.204***	0.222***	0.193***	0.211***	0.177***
	(0.056)	(0.201)	(0.083)	(0.046)	(0.081)	(0.045)	(0.080)	(0.044)
M1					0.211**	0.086		
					(0.094)	(0.053)		
M2							0.118***	0.082***
							(0.034)	(0.021)
Obs	726	726	564	624	564	624	564	624
R^2	0.104	0.437	0.178	0.210	0.207	0.220	0.245	0.280
Mediation Effect	N/A	N/A	N/A	N/A	13.37%	N/A	20.54%	17.48%

Notes: X refers to the share of female councillors in each local government. M1 refers to the local government funding category – miscellaneous public health services. M2 refers to the local government funding category – total public health. All regressions control for the size of local councils, local police staff, the logarithm of total services expenses, and the population in the local council area. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

become known to authorities. Improved service provision facilitates victim disclosure and institutional detection, even if the actual incidence of abuse remains unchanged or declines. This interpretation aligns with the divergence observed between police-recorded and self-reported measures in Section 2.5, where female representation is associated with increased police records but decreased self-reported experiences of abuse. The funding patterns documented here suggest one mechanism through which female councillors may contribute to this divergence: by allocating resources toward victim services, they enhance the visibility of abuse in official statistics whilst potentially reducing the underlying prevalence through improved support and intervention.

Together, the evidence from Sections 2.6.1 and 2.6.2 suggests that female representation is associated with both behavioural changes among potential victims and institutional shifts in resource allocation – two complementary pathways through which reporting may increase. We next examine whether the relationship between female representation and police-recorded abuse varies across institutional contexts.

2.6.3 Institutional Partnerships with Specialist Organisations

To further investigate how institutional context shapes the relationship between female representation and police-recorded abuse, we examine whether formal partnerships between local

councils and Refuge moderate this association.[25] Refuge provides a wide range of specialist domestic abuse services, including emergency accommodation, legal advocacy, resettlement support, and community-based outreach to help women stay safe and rebuild their lives.

Table 2.6: Heterogeneous Effects by Statutory Partnership with Refuge

	Statutory Partnership w/ Refuge		No Statutory Partnership	
	(1)	(2)	(3)	(4)
Panel A: Domestic Abused-Related Incidents and Crimes				
% of Female Councillors	-0.351**	-0.196**	0.404***	0.206**
	(0.161)	(0.073)	(0.114)	(0.086)
Controls & FEs		✓		✓
Observations	78	78	495	484
R-squared	0.214	0.887	0.077	0.713
	(5)	(6)	(7)	(8)
Panel B: Domestic Abused-Related Incidents				
% of Female Councillors	-0.214**	-0.099**	0.204**	0.117*
	(0.100)	(0.037)	(0.080)	(0.060)
Controls & FEs		✓		✓
Observations	78	78	499	488
R-squared	0.223	0.916	0.045	0.705
	(9)	(10)	(11)	(12)
Panel C: Domestic Abused-Related Crimes				
% of Female Councillors	-0.118	-0.081**	0.196***	0.096**
	(0.075)	(0.034)	(0.044)	(0.037)
Controls & FEs		✓		✓
Observations	93	93	544	533
R-squared	0.086	0.836	0.089	0.653

Notes: The key explanatory variable is the percentage of female councillors in each local council (0-100). Control variables include local government size; police force characteristics (percentage of female officers, total staff, age composition, ethnic composition, absence rate, entry rate, exit rate, and promotion rate); and local area characteristics (employment rate, female employment rate, economic activity rate, female economic activity rate, population density, sex ratio, and ethnic composition of the local population). All specifications include region and year fixed effects. Standard errors are clustered at the local authority level and reported in parentheses. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

We collect information on statutory partnerships between local councils and Refuge from the organisation's Annual Reports and Financial Statements.[26] We then conduct a heterogeneous effects analysis by splitting local authorities into two groups: those with and without formal partnerships. Additional details are provided in Appendix Table B.5. Table 2.6 presents the results, with each panel examining a different measure of police-recorded domestic abuse. The left columns (1) and (2) show results for councils with Refuge partnerships, whilst the right columns (3) and (4) show results for councils without such partnerships. All specifications include the full set of control variables employed in Section 2.5.1, alongside regional

[25] Refuge (n.d.) is one of the largest specialist domestic abuse organisations in the UK.

[26] Information on statutory partnerships between local authorities and Refuge is compiled from Refuge's Annual Reports and Financial Statements (Refuge, n.d.).

and year fixed effects.

The heterogeneity across these two groups is striking. In councils without Refuge partnerships (columns 3 and 4), the coefficient on female councillor share is consistently positive and statistically significant across all three outcome measures in Panels A, B, and C. This replicates the baseline positive association documented in Section 2.5.1: areas with more female councillors record more domestic abuse-related incidents and crimes. However, in councils with Refuge partnerships (columns 1 and 2), this relationship reverses. Panel A shows that female representation is negatively associated with total domestic abuse-related cases. Panel B, which focuses specifically on domestic abuse-related incidents, similarly reveals a negative coefficient. The magnitude of these negative associations is comparable to the positive associations observed in councils without partnerships, suggesting a substantively meaningful difference in how female representation relates to police-recorded outcomes depending on institutional context. Panel C, which examines domestic abuse-related crimes specifically, shows a statistically insignificant association in councils with partnerships.

This contrast suggests that institutional infrastructure may moderate how female representation relates to police-recorded domestic abuse. In contexts where specialist services are already available through Refuge partnerships, the positive association between female representation and police records is reversed. One interpretation consistent with the reporting narrative is that female councillors in councils with established partnerships may be better able to channel support through non-policing institutions. When specialist services are accessible, victims may be more likely to seek help through these organisations rather than directly through police channels, or incidents may be resolved through support services before escalating to the point of police involvement. This could explain why the association with police-recorded abuse becomes negative in councils with Refuge partnerships, whilst remaining positive in councils without such institutional infrastructure. These findings highlight the importance of institutional capacity in shaping the relationship between female political representation and the visibility of domestic abuse in official statistics.

Since the majority of statutory Refuge partnerships are concentrated among London boroughs (see details in Appendix Table B.5), a natural concern is that the heterogeneity documented above may reflect London-specific characteristics – such as distinctive policing practices, demographic composition, or service provision – rather than the role of Refuge partnerships per se. To address this, we estimate the PRC regressions separately for London and non-London

local authorities (Appendix Table B.16). If the Refuge results were driven by London effects, we would expect the London subsample to exhibit a similar pattern of negative associations.

The results do not support this interpretation. Despite a larger sample size (132–165 observations for London versus 78–93 for Refuge partnerships), the London subsample produces no statistically significant associations in any panel or specification. Coefficients are negative but small in magnitude and imprecisely estimated. The non-London subsample similarly shows insignificant results once all controls are included. By contrast, the Refuge partnership split – based on a smaller sample – yields consistently significant coefficients in both subsamples. This comparison suggests that it is the presence of specialist institutional infrastructure, rather than London-specific factors, that moderates the relationship between female representation and police-recorded domestic abuse. The Refuge heterogeneity thus reflects a meaningful institutional channel rather than a geographic artefact.

Taken together, the evidence presented in this section provides a coherent account of the reporting narrative. Section 2.6.1 demonstrates that women in areas with greater female representation are more likely to self-report incidents to police, pointing to empowerment and shifts in community norms. Section 2.6.2 shows that female councillors allocate more resources to public health services, creating institutional pathways through which incidents become visible in official statistics. Section 2.6.3 reveals that the relationship between female representation and police-recorded abuse depends on the availability of specialist support infrastructure: where Refuge partnerships exist, the positive association with police records reverses, consistent with specialist services providing alternative pathways for victims that reduce reliance on police recording. These complementary mechanisms help explain why police-recorded domestic abuse increases with female representation even as self-reported experiences of abuse decline, as documented in Section 2.5.

2.7 Instrumental Variable Analysis

The associations documented in the preceding sections do not establish causality. Female councillors' share may be correlated with unobserved local characteristics that independently affect domestic abuse reporting or prevalence. To probe whether the main findings reflect a causal relationship, we employ instrumental variable analysis exploiting quasi-random variation in female representation induced by ballot ordering.

As documented and validated in Chapter 1, the alphabetical ordering of candidates' surnames on local election ballots creates exogenous variation in electoral success, particularly in the low-information context of local council elections. Because surname ordering is fixed and independent of candidate quality or voter preferences, this generates plausibly exogenous variation in the share of female councillors elected. We construct multiple specifications of the instrument to ensure robustness, varying the definition of advantageous ballot positions and the weighting scheme applied across wards.[27]

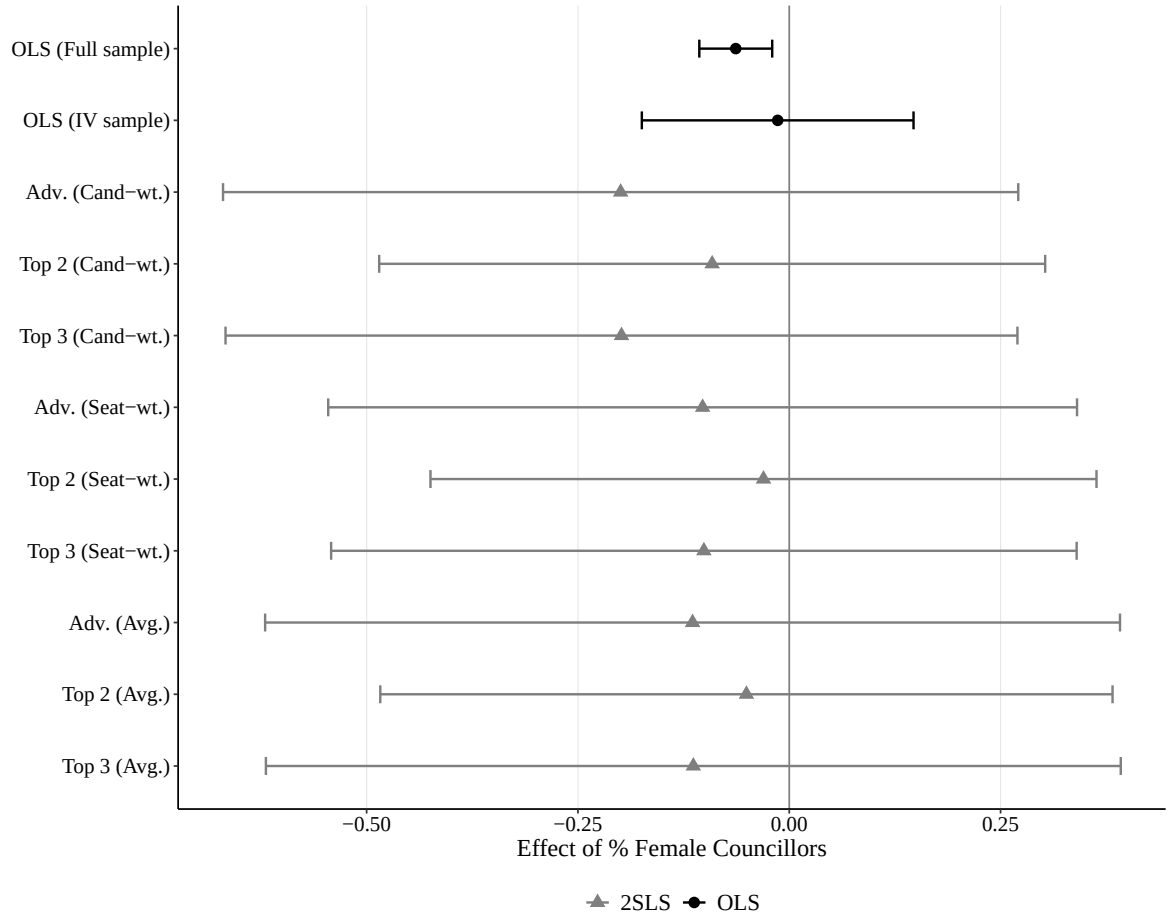

Figure 2.11: Effect of Female Councillors on Non-Physical Abuse: OLS and IV Estimates

Notes: The figure plots OLS and 2SLS coefficient estimates of the effect of female councillor share on non-physical domestic abuse experienced by female respondents. Horizontal bars represent 95% confidence intervals. The full-sample OLS uses 23,071 observations; all other estimates use the restricted IV sample of 3,696 observations. Nine instrument specifications are shown, varying the definition of advantageous ballot positions (advantageous, top 2, top 3) and weighting scheme (candidate-weighted, seat-weighted, simple average). Full estimation results are reported in Appendix Table B.17.

Figure 2.11 summarises the results. The full-sample OLS estimate, shown at the top, confirms a statistically significant negative association between female councillor share and non-physical abuse experienced by female respondents. However, the IV estimation requires

[27]See Chapter 1 for details on the construction of instrumental variables. Specifications include candidate-weighted, seat-weighted, and simple averages of the share of female candidates in advantageous ballot positions, with positions defined as top N (where N equals seats contested), top 2, and top 3. All specifications are reported in Appendix Table B.17.

restricting the sample to local authorities where the ballot ordering instrument is available, reducing the sample from 23,071 to 3,696 observations. The OLS estimate in this restricted subsample is substantially decreased and statistically insignificant, as shown by the wider confidence interval that comfortably spans zero. This decrease may reflect differences in the composition of local authorities included in the IV sample or simply the loss of statistical power associated with the substantially reduced sample size.

The two-stage least squares estimates, shown for nine instrument specifications, are uniformly negative in direction but imprecise, with wide confidence intervals that span zero in all cases. First-stage F-statistics range from approximately 5.5 to 12.1, with several specifications falling below the conventional threshold of 10 suggested by Staiger and Stock (1997), raising concerns about weak instrument bias. The pattern visible in Figure 2.11 is clear: whilst all point estimates lie to the left of zero – broadly consistent with the direction of the full-sample OLS – the restricted sample lacks sufficient power to distinguish these estimates from zero.

These results are best characterised as inconclusive. Crucially, the insignificance of the OLS estimate in the IV subsample means that the null second-stage results cannot be interpreted as evidence against a causal relationship; the restricted sample simply lacks the power to detect effects of the magnitude observed in the full sample. The substantial reduction in sample size – from over 23,000 to under 4,000 observations – combined with generally weak first-stage relationships, severely limits the inferential power of this exercise. The IV analysis therefore neither confirms nor rules out a causal interpretation of the main findings.

Despite these limitations, the IV analysis serves a useful purpose within this chapter. It demonstrates transparency about the difficulty of establishing causality in this setting and provides a benchmark for future research. Studies with access to larger samples, alternative sources of exogenous variation in female representation, or quasi-experimental designs may be better positioned to establish whether the associations documented here reflect causal relationships. The main findings – that female representation is associated with both increased police recording and decreased self-reported abuse – remain robust and substantively important, even as their causal interpretation awaits more definitive evidence.

2.8 Conclusion

This chapter has examined whether and how female political representation at the local level is related to domestic abuse in England. Using a combination of administrative data from police records and self-reported data from the Crime Survey for England and Wales (CSEW), the analysis identifies a positive association between the share of female councillors and the number of police-recorded domestic abuse cases. At first glance, this pattern could be interpreted in multiple ways. One possibility is that increased female representation leads to an actual rise in domestic abuse incidents. An alternative interpretation is that greater representation enhances victims' willingness or ability to report abuse, thereby making previously hidden cases more visible in official data.

To distinguish between these possibilities, the chapter compares the police-recorded trends with results from individual-level survey data (CSEW), which captures both reported and unreported experiences of abuse. The survey evidence does not show any increase in actual abuse prevalence. In fact, the results suggest a small but meaningful reduction in non-physical abuse amongst women. This divergence between data sources indicates that the rise in police-recorded cases is unlikely to reflect a worsening of underlying abuse conditions. Instead, it points toward changes in reporting behaviour, particularly amongst female victims.

A series of placebo tests reinforces this interpretation. There is no similar association between female councillors and other categories of crime, either in police records or in survey data. Nor is there evidence of a relationship between female representation and abuse committed by other family members. These null findings help isolate the observed effects to the specific context of intimate partner violence against women, and suggest that broader shifts in crime or victimisation patterns are unlikely to explain the results.

To understand how female representation might be associated with increased reporting, the chapter explores two complementary channels and one institutional moderator. First, individual-level analysis from the CSEW indicates that a higher share of female councillors is associated with increased victim-initiated reporting to the police, particularly amongst female victims. Whilst police may become aware of incidents through various channels, many incidents occur in private settings and remain hidden unless victims come forward themselves. The observed rise in self-initiated reporting suggests that female political representation may be associated

with environments in which women feel more supported or empowered to disclose abuse. Second, mediation analysis shows that female councillors are associated with increased local spending on public health services, including specialist programmes for domestic abuse victims. These budget allocations are positively correlated with police-recorded abuse cases, suggesting that expanded service provision may help more victims seek help or come into contact with authorities. Finally, heterogeneity analysis reveals that in local authorities with statutory partnerships with Refuge, the association between female representation and police-recorded abuse is negative, whilst it remains positive in councils without such partnerships. This contrast suggests that the availability of specialist domestic abuse services may moderate how female representation relates to the visibility of abuse in official statistics, potentially by providing alternative support pathways that reduce reliance on police recording.

The chapter also employs instrumental variable analysis, exploiting quasi-random variation in female representation induced by ballot ordering, to probe the causal interpretation of the main findings. However, the IV analysis proves inconclusive. The estimation requires restricting the sample to a substantially smaller subset of local authorities, in which the OLS association is already statistically insignificant. The two-stage least squares estimates remain negative in direction across all specifications but are imprecise, and several first-stage F-statistics fall below conventional thresholds for instrument strength. These results neither confirm nor contradict a causal interpretation: the restricted sample simply lacks the statistical power to detect effects of the magnitude observed in the full sample. Establishing causality in this setting remains an important challenge for future research, which may benefit from larger samples, alternative sources of exogenous variation, or quasi-experimental designs.

Despite the limitations of the causal analysis, the overall body of evidence points toward a consistent interpretation. The findings suggest that female councillors are associated with increased visibility of abuse in official statistics, operating through enhanced victim reporting and the allocation of resources toward public health services that support abuse victims. The strongest associations are observed amongst female victims and for non-physical forms of abuse, highlighting the potential for female political leadership to influence both the recognition and reporting of violence that often goes unseen.

This chapter contributes to ongoing debates about the role of female political representation in shaping social outcomes. Prior studies in developing countries have documented increases

in officially recorded violence against women following gains in female representation, typically interpreting these patterns as reporting improvements. By combining police records with individual-level survey data, this chapter provides more direct evidence for the reporting mechanism in a high-income democracy, showing that increases in police-recorded abuse coincide with decreases in self-reported prevalence. The findings raise important questions for future research, including how local services interact with political representation to shape reporting behaviour, whether the institutional channels identified here – particularly the role of specialist partnerships – operate similarly in other contexts, and how broader gender norms mediate the relationship between political representation and women's willingness to engage with authorities. The evidence presented here offers a foundation for deeper investigation into the institutional and behavioural pathways through which political representation translates into lived experiences.

Chapter 3

The Impact of China's Two-Child Policy on Female Employment Outcomes

Abstract

In early 2014, China implemented the Two-Child Policy, relaxing the one-child family planning regulations enforced since the 1980s. Whilst this pro-natalist policy has not substantially increased fertility rates, its broader socioeconomic implications, particularly for women, warrant thorough investigation. This study examines the impact of the Two-Child Policy on labour market outcomes for women who became eligible to have additional children following the policy's implementation, utilising longitudinal data from the China Family Panel Studies (CFPS) database. Employing a difference-in-differences (DID) framework, this research demonstrates that eligible women experience statistically significant adverse labour market effects operating through two channels: wage penalty and decreased employment probability. Further analysis demonstrates heterogeneous effects across various socioeconomic factors, with women in prime childbearing ages, less-educated women, and rural eligible women disproportionately bearing the adverse consequences of the policy. The robustness of these findings is confirmed through extensions of the DID framework.

3.1 Introduction

The relationship between women's domestic responsibilities and employment opportunities has been a subject of extensive scholarly discourse. Empirical and anecdotal evidence suggests that childbearing can significantly impact women's labour market outcomes (Anderson et al., 2002; Jia and Dong, 2013; Nizalova et al., 2016; Xiao, 2017). This impact may arise from the joint determination of work and fertility decisions, with pregnancy and childcare often necessitating career interruptions. Additionally, women with children may face increased discrimination in the labour market. However, disentangling these effects has been challenging due to the lack of plausibly exogenous variation.

Since the 1980s, China's one-child policy has significantly shaped its demographic landscape, resulting in an ageing society and a declining youth population. In response to these demographic pressures, China relaxed the strict one-child policy in early 2014 by implementing a selective two-child policy, allowing certain couples to have a second child. From the perspective of policymakers, this deregulation of family planning was expected to increase fertility rates and alleviate the burden on the working population and pension systems. However, this pronatalist policy may inadvertently exacerbate discrimination against women in the labour market, given their primary role in childcare and the career interruptions typically associated with childbirth.

While Chinese labour protection laws prohibit employers from reducing salaries, dismissing, or terminating women's contracts on maternity leave, the reality often diverges from these legal protections. Bulte et al. (2011) report instances of large corporations threatening wage reductions, benefit cuts, bonus denials, and even dismissals. Furthermore, Fan et al. (2016) suggest that the two-child policy may negatively impact women's work expectations and increase their unemployment risk. These observations underscore the importance of investigating the unintended consequences of this policy on female employment outcomes.

This study aims to provide empirical evidence on the effects of the two-child policy on women's incomes and to examine potential discriminatory impacts on their labour market outcomes. Employing a difference-in-differences (DID) framework, I analyse data from the China Family Panel Studies (CFPS) database to isolate the causal effect of the policy change. The primary focus is on women of childbearing age who have not had a second child, as they constitute the main target group of this policy. Specifically, this research investigates the

impact of the significant shift from China's long-standing strict one-child policy to a more relaxed approach that encourages childbearing.

The significance of this study lies in its contribution to the broader literature on the economic implications of fertility policies and its potential to inform policy decisions aimed at mitigating unintended labour market consequences. By providing robust empirical evidence on the effects of the two-child policy, this research offers valuable insights for policymakers seeking to balance demographic objectives with gender equality in the labour market.

The structure of this chapter is as follows: Section 3.2 reviews and synthesises previous empirical evidence to contextualise the current state of female labour market discrimination. Section 3.3 provides a detailed discussion of China's population policy and the status of female labour force participation. Section 3.4 outlines the data and identification strategy employed in this study. Section 3.5 presents the main results, and explores potential mechanisms. Section 3.6 analyses heterogeneity across various socioeconomic factors. Section 3.7 assesses the robustness of the main findings and extends the difference-in-differences (DID) framework. Finally, Section 3.8 concludes with a discussion of the main findings and their potential policy implications.

3.2 Literature Review

Population policies are fundamental tools employed by governments to achieve balanced demographic growth and facilitate smooth economic development. These policies have varied in their objectives, with some nations implementing measures to control excessive population growth, while others have adopted pronatalist policies to address low fertility rates and ageing populations (Del Boca and Wetzels, 2007; Recoules, 2011). The economic impacts of these policies extend beyond their primary demographic objectives, influencing various aspects of social and economic life, particularly in the realm of labour markets and gender dynamics. Anti-natalist policies, such as birth restrictions, have been shown to have positive economic effects. Ngo (2020) find that such policies can increase maternal employment by reducing the number of children per woman. Similarly, Li and Zhang (2007) argue that increasing birth rates can negatively impact economic growth, suggesting that birth control policies may boost economic development. However, the effects of pronatalist policies are less understood, especially in the context of rapid policy shifts. While pronatalist policies

combined with parental leave benefits and subsidised childcare can significantly benefit maternal employment (Geyer et al., 2015), without appropriate complementary measures, such policies may lead to adverse outcomes. For instance, Tan and Liu (2020) notes that China's two-child policy may temporarily reduce family income.

The relationship between women's labour market performance and fertility decisions has been a subject of extensive research, yielding mixed empirical results. Several studies have found that having children negatively impacts female employment (Agüero and Marks, 2008; Cáceres-Delpiano, 2012; Lundborg et al., 2017; Zhang, 2017; Zhou et al., 2021). Conversely, delayed childbearing and lower fertility have been associated with increased female labour force participation (Goldin, 1990). However, some research has not found a significant link between maternal wages and fertility (Agüero and Marks, 2008; Korenman and Neumark, 1992). These mixed results partly stem from methodological challenges in studying the relationship between fertility and labour market outcomes. Many studies addressed the endogeneity of fertility decisions, proposing various approaches such as twin studies (Zhang, 2017) and instrumental variables like infertility (Agüero and Marks, 2008; Lundborg et al., 2017) or preference for mixed sibling-sex (Angrist and Evans, 1999). The diversity of methodological approaches and contexts in these studies underscores the complexity of the relationship between fertility and women's labour market outcomes, highlighting the need for detailed, context-specific research.

Cultural and social factors play a crucial role in shaping the effects of population policies, interacting with economic incentives to influence both fertility decisions and labour market outcomes. Traditional economic literature asserts that women have a comparative advantage in household production (Killingsworth and Heckman, 1986). However, as women have become more vocal in family and society, this ideology has been challenged. Li (2021) notes that some women delay marriage and have fewer children to capitalise on increased job opportunities. Moreover, media portrayals and government propaganda can significantly influence fertility preferences (Ferrara et al., 2012). In the Chinese context, the transition from the one-child policy to the two-child policy provides a unique opportunity to study the effects of a pronatalist policy shift. The one-child policy period saw widespread propagation of slogans promoting the value of female children, potentially influencing societal attitudes and leading to increased investment in human capital for women born during this period (Hare, 2016). These cultural and social dynamics add layers of complexity to the analysis of population

policies and their economic impacts, necessitating approaches that consider both economic and sociocultural factors.

Despite the extensive literature on anti-natalist policies, there remains a gap in understanding the labour market implications of pronatalist measures, particularly in the context of rapid policy shifts like China's transition to a two-child policy. This study aims to address this gap by examining the effects of China's two-child policy on women's labour market outcomes, regardless of actual fertility changes. By employing a Difference-in-Differences (DID) methodology, I investigate the unintended consequences of the pronatalist policy shift, highlighting how the mere possibility of increased fertility can influence labour market dynamics for eligible women. This research thus contributes to the literature by examining how a pronatalist policy can affect women's employment and incomes, even in the absence of actual changes in fertility behaviour. This perspective offers insights into the broader societal and economic impacts of population policies beyond their direct demographic effects, contributing to a better understanding of how population policies interact with economic and social factors. By focusing on the labour market implications of a pronatalist policy shift, this study bridges the gap between demographic research and labour economics, offering valuable insights for policymakers navigating the complex interaction between population policies and economic outcomes.

3.3 Background

3.3.1 Population Policy and Fertility

China's population policies have undergone significant transformations since the establishment of the People's Republic of China in 1949, reflecting the country's changing demographic challenges and socioeconomic priorities. Initially, the government adopted a pronatalist attitude, considering people to be the most critical force. Women who gave birth to many children were given the title of 'Mother Heroine', an honorary title borrowed from the Soviet Union that recognised women for bearing and raising large families. This pronatalist stance was challenged by the economist Ma Yinchu, who in 1957 advanced his *New Population Theory*, arguing that rapid population growth would hinder China's economic development (Greenhalgh, 2008). Despite Ma's efforts, attempts to implement birth control in the 1950s and 1960s were largely unsuccessful due to political disruptions such as the

Great Leap Forward and the Cultural Revolution. These events led to widespread famine followed by a post-famine fertility rebound. Consequently, between 1962 and 1970 China's population increased by 157 million.[1] In response, China adopted the 'later, longer, fewer' campaign in the 1970s, encouraging later marriage, longer birth spacing, and fewer births. This policy marked the beginning of systematic efforts to reduce population growth, paving the way for stricter measures in the following decades.

In 1980, the 'later, longer, fewer' approach was replaced by the stricter One-Child Policy, marking a pivotal moment in China's demographic history. Family planning was formally incorporated into the Constitution of the People's Republic of China and began to become the basic population policy of the country. The policy mandated that a couple could only have one child, with exceptions for certain conditions that exhibited urban-rural and intraprovince differences. The implementation of the One-Child Policy was closely tied to China's household registration system, known as *hukou*[2]. For over three decades from the 1980s, the urban *hukou* had a strict policy of allowing only one child per couple. On the other hand, the rural *hukou* had different policies depending on the sex and number of children already present, and the province they are located in, allowing a couple to have up to two children. The specific provinces and details of the implementation of each policy are shown in Table C.1 in the Appendix. Additionally, ethnic minority groups enjoyed less strict birth control policies, which allowed them to have two or more children.

The One-Child Policy effectively controlled population growth, with China's fertility rate falling well below replacement level since the 1990s, as illustrated in Figure 3.1. However, this persistent low fertility rate began to exert increasing pressure on the country, particularly in terms of an ageing population and shrinking workforce. The challenges of an ageing population, characterised by low fertility rates and growing life expectancy, became increasingly apparent. An ageing population refers to a significant increase in the number of individuals above the age of 65 in comparison to the younger population and as a proportion of the total population (United Nations and Social Affairs, 2020). An ageing population can cause a shortage of young labour, and can cause pension and welfare systems to collapse. Ageing will also inevitably increase pressure on health systems. In addition, older people are less

[1] Population data are from the National Bureau of Statistics of China (n.d.).

[2] Household registration is divided into "agricultural *hukou* (rural *hukou*)" and "non-agricultural *hukou* (urban *hukou*)", based on geographic location and the relationship of family members. Agricultural *hukou* (rural *hukou*) can be granted land use rights in the rural areas where they are located. However, they are not entitled to specific policies in the cities.

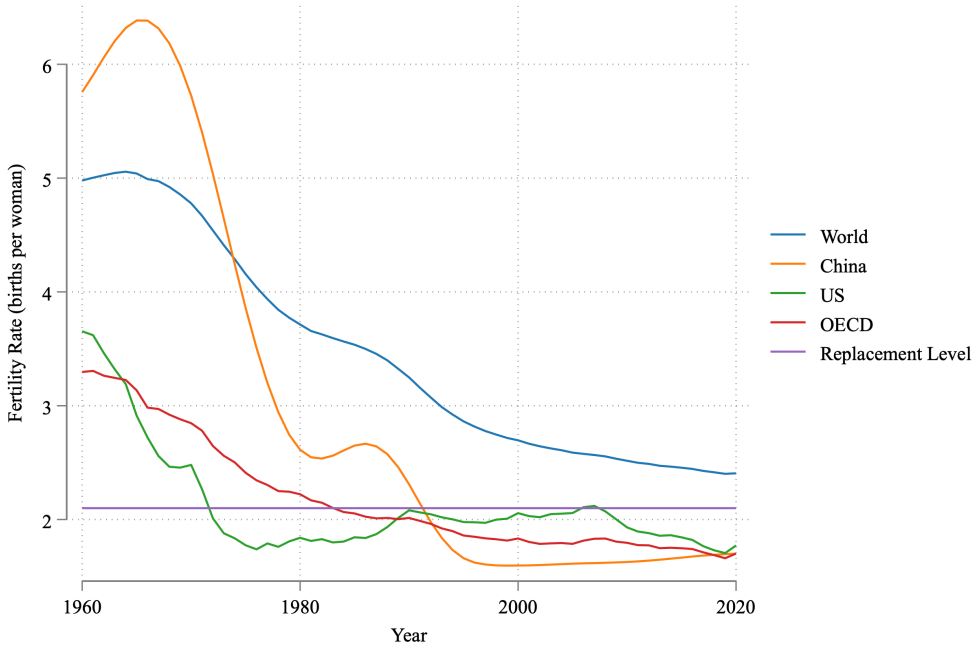

Figure 3.1: Fertility Rate Trends Over the Years (Births per Woman)
Data Source: The World Bank, https://data.worldbank.org/.

likely to actively contribute to the productive capacity of an economy.

The ageing problem has been a subject of long-time discussion. Guramulta (2019) argues that the increasing numbers over retirement age and a diminished fertility rate results in the progressive reduction of capable future generations fit to enter the labour market. Since the 1950s, ageing has started to increase sharply in developed countries. Thane (1984) summarised the challenges of an ageing population in Britain to the economy, in particular to labour supply, by changing housing demand, education, health, social security and other social services. Guramulta (2019) also pointed out the long-term impact of population ageing and the diminished number of capable young people inevitably lead to a lowered long-term development capacity. According to the Seventh National Population Census, as of November 1 2020, there were 264.02 million people aged 60 and over in China, accounting for 18.70% of the population, of whom 13.50% were aged 65 and over (National Bureau of Statistics of China, 2021). As can be seen from the Appendix Figure C.1 Population Pyramid, the base of the pyramid is gradually narrowing, and as a consequence the long-term balanced development of the population will continue to pressure China.

In response to these demographic pressures, China began to gradually relax its strict birth control policies. Since 1984, a strictly selective Two-Child Policy has been implemented across the country, allowing couples who are both only children to have two children. The specific

implementation schedule by province is detailed in Table C.2 in the Appendix, with Henan Province being the last province to implement this policy in 2011. Before the policy was implemented, Henan Province had predicted that the policy would generate approximately 18,000 additional births annually. However, according to the province's statistics bureau, there were only 600 cases of couples applying for birth quotas in the province after two years of the policy being introduced.[3]

A further relaxation occurred in November 2013, when the Communist Party of China (CPC) Central Committee proposed "to adhere to the basic policy of family planning and initiate the implementation of the policy of allowing couples in which one of the parents is an only child to have two children." However, the impact of this policy was less significant than expected. Among the estimated more than 11 million couples who were eligible to have a second child under the new policy, only 1.69 million couples had applied for a second child under the new rule as of August 2015, representing just 15.4% of those couples (Xi, 2015). The impact of the selective Two-Child Policy was much lower than officially expected, with the total number of births falling instead of increasing in 2015, see details in Figure 3.2 Panel (A).

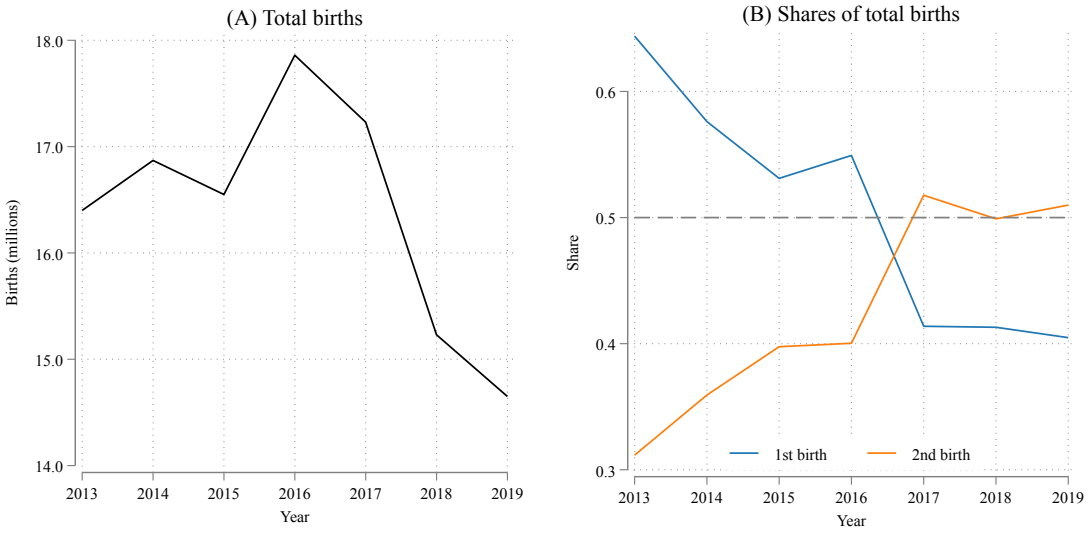

Figure 3.2: Total Births and Shares by Birth Order, 2013–2019
Data source: National Bureau of Statistics of China (http://www.stats.gov.cn/english/)

The most significant policy shift came in October 2015, when the CPC passed a change to the *Population and Family Planning Law of the People's Republic of China*, promoting a universal Two-Child Policy. This law came into effect on January 1, 2016. Provincial governments worked on detailed implementation measures during the year, and the specific implementation schedule for each province is also detailed in Appendix Table C.2. While

[3]Reported by Ifeng News (2015), citing data from the Henan Statistics Bureau.

there has been an increase in the number of births since 2016, it failed to bring a lasting impact, as seen in Figure 3.2 Panel (B). The rate of second childbirth has increased after this adjustment, but the rate of first childbirth has continued to decline. Since 2017, the birth rate of the second child has been proportionately higher than the birth rate of the first child. This results from the reduced desire of the younger generation to have children and the declining number of women of childbearing age, the utility of children decreases, and the cost of raising them increases.

Today, Chinese women remain relatively active in the labour force, while their family responsibilities have not diminished under deeply rooted gender stereotypes. Increasing the future workforce is the goal of the current population policy. However, having more children may cause some women to temporarily or permanently withdraw from the labour market. One can also foresee that a rebound in births may be short-lived, and cease to exist when the accumulated second-child demand of the current generation is released. These dynamics underscore the need for a thorough understanding of the relationship between population policies, fertility trends, and labour market outcomes, particularly for women, in contemporary China.

3.3.2 Women in Labour Market

At the inception of the People's Republic of China in 1949, Chinese women were portrayed as equals in the workplace. The phrase "Women hold up half the sky" became a famous slogan about the role of women in China since the 1960s, symbolising the ideal of gender equality. This principle was enshrined in law from the country's beginning, granting women the same rights as men in politics, business, culture, social life, and family life. According to Maoist political discourse, gender equality could be realised through women's participation in paid jobs. For most urban women, full-time work outside the home became the norm (Hare, 2016). Generations of Chinese men and women have grown up with the idea of gender equality, which has contributed to a strong female workforce and elevated female status in China.

This ideological stance, however, stands in contrast to the traditional Confucian principle that "the work of men takes place outside, and women's work takes place at home", which has long dominated Confucian countries. In China, communist ideology has had a significant impact on these Confucian traditions. Consequently, the position of women in the workplace in China has often been more advanced than in other Confucian-influenced Asian countries.

For instance, the percentage of women in the lower or single house of government is 5.4% in Thailand, 10.2% in Japan, 23.0% in Singapore, and 24.9% in China (Inter-Parliamentary Union, 2019). However, following the reform and opening up policy initiated in 1978, the status of women experienced a regression. The reform of state-owned enterprises, the dramatic increase in private enterprises, and the more rigorous accounting of economic costs by employers have led to discrimination against the employment of women who gave birth and raised children. This phenomenon is not unique to China; Bonin and Euwals (2005) also point out that women in East Germany were dismissed at a higher rate than men at the beginning of the economic transition.

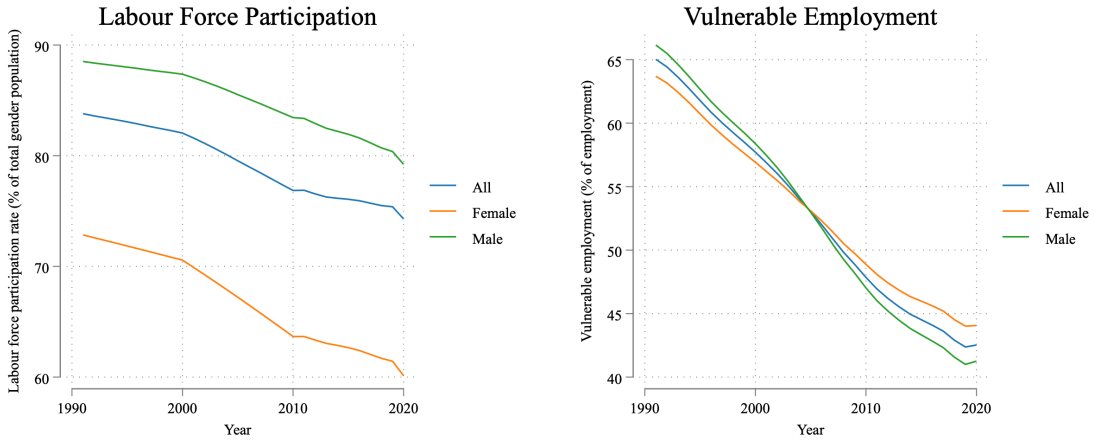

Figure 3.3: Labour Force Participation Trends in China (1990-2020)
Data Source: World Bank Group, Gender Data Portal: China.

The changing status of women in the Chinese labour market is evident in various labour force participation trends. Studies have shown that the participation rate of women in the labour force is declining (Chi and Li, 2014; Du and Dong, 2013). Figure 3.3 illustrates the labour force participation trend in China from 1990 to 2020. The left graph shows that the overall participation rate is decreasing over time, with the decline being dramatically steeper for females compared to the flatter trend for males. The right graph reveals an interesting shift in vulnerable employment situations over time. Workers in vulnerable employment are those least likely to have formal work arrangements, social protection, or safety nets against economic shocks.[4] Although the overall proportion of vulnerable employment in total employment has decreased as the economy developed and the labour market matured, the proportion of female vulnerable employment in total female employment has been converging with that of males since the 1990s and has surpassed it since 2006. This trend likely indicates

[4]Vulnerable employment refers to the sum of own-account workers and contributing family workers (World Bank, n.d.).

a significant transition in the position of women in the Chinese labour market.

Despite the high labour force participation rates historically seen in China, significant gender disparities persist, particularly in terms of wage equality. The gender pay gap in China remains a pressing issue, with Shi et al. (2011) documenting a significant evolution of this gap among urban employees over time, suggesting persistent and potentially worsening gender wage inequalities in China's urban labour markets. This disparity can be attributed to various factors, including occupational segregation, with women overrepresented in lower-paying sectors and underrepresented in leadership positions. The gender pay gap is not uniform across all sectors and regions of China, with some studies indicating that it may be more pronounced in the private sector and in coastal regions where market forces play a larger role in wage determination (Ma, 2018).

The introduction of the Two-Child Policy has raised concerns about its potential impact on gender inequalities in the labour market, particularly in the context of existing wage disparities. Unlike other countries that have paired their pronatalist policies with welfare measures to encourage childbirth (Recoules, 2011), China's Two-Child Policy allows couples to have two children without accompanying supporting welfare measures. This policy change means that women of childbearing age are now eligible to give birth twice and take maternal leave with a basic wage, potentially impacting employers' benefits. Moreover, employers may factor women's potential maternal leave into their employment decisions, potentially opting for a more stable and time-guaranteed labour force. These factors could potentially exacerbate the existing gender pay gap, as employers might be less willing to invest in female employees' career development or offer them high-paying positions.

The impact of China's population policies on women's labour market participation is further complicated by the historical implementation of the One-Child Policy. Between 1982 and 2013, the population policy was not uniformly applied, with discretionary policy decisions shown in Appendix Table C.1. The One-Child Policy was implemented strictly for the vast majority of urban residents, resulting in a significant number of only children. The 1% National Population Sample Survey conducted in 2005 reveals the policy's growing impact over time: among those born between 1980 and 1989, 19.43% (455,655 individuals) were only children, rising to 32.75% (838,250 individuals) for the 1990-1999 birth cohort (National Bureau of Statistics of China, 2007).

Consequently, the number of only children in urban areas is substantial, and today, these two generations of only children have become the backbone of the labour market. This demographic shift has introduced unique dynamics to the labour market, particularly for women. As only children are often heavily invested in by their families (Hare, 2016), they tend to receive more valuable educational resources. This is especially true for only daughters, who may benefit from educational opportunities that might have been divided among siblings in larger families. This increased investment in human capital often translates into a stronger tendency to remain in the labour market and potentially different career expectations compared to previous generations.

However, women who grew up in a one-child environment now face unique challenges as China's fertility policies evolve. The transition to the Two-Child Policy presents a complex scenario for these women, who must navigate potential changes in societal expectations, workplace dynamics, and personal aspirations. Their situations deserve a thorough study, particularly in light of how the Two-Child Policy may affect their labour market outcomes and career trajectories. Understanding these dynamics is crucial for assessing the full impact of China's shifting population policies on women's roles in both the family and the workforce.

3.4 Empirical Strategy

3.4.1 Data

This study analyses the China Family Panel Studies (CFPS) database, which tracks participants biennially in twenty-five provinces or their administrative equivalents (municipalities and autonomous regions) in China.[5] With approximately 95% of the country's population concentrated in these areas, the CFPS sample can be considered nationally representative. The survey covers a wide range of socio-economic aspects, including detailed questionnaires on economic, social, demographic, occupational, and educational characteristics. This study utilises six waves of the CFPS (2010, 2012, 2014, 2016, 2018, 2020), which cover the necessary years for two-child policy analysis and meet the identification requirements of the research question. The panel structure of the data allows for control of unobservable individual heterogeneity. To obtain a fully balanced panel, the dataset retains only individuals

[5]The CFPS excludes Hong Kong, Macao, and Taiwan, as well as Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan. For simplicity, 'provinces' is used hereafter to refer to all provincial-level administrative units.

present in the 2010 baseline wave who are observed across all six survey waves, yielding up to six observations per individual. An unbalanced dataset is also used for robustness checks in a later section.

In the CFPS, individuals' residence typically aligns with their household registration (*hukou*) type: those with non-agricultural hukou generally reside in urban areas, while those with agricultural hukou tend to live in rural areas. However, China's rapid socio-economic development and social reforms have led to increased internal migration, creating a more complex demographic landscape. This complexity is particularly relevant to family planning policies, which can vary across different jurisdictions. To address potential gaps in policy implementation for migrant populations, the Family Planning Law stipulates that family planning for migrants is a shared responsibility between the governments of their hukou location and their current residence, with the latter taking precedence.[6] Considering these factors, this study includes individuals with both agricultural and non-agricultural hukou types, regardless of their current residence (urban or rural).

This study includes both ethnic minorities and Han Chinese in the primary analysis, reflecting the demographic composition of the Chinese population. Han Chinese constitute the overwhelmingly dominant ethnic group, with ethnic minorities accounting for approximately 8.5% of the national population according to the 2010 Census.[7] The ethnic minority share in the analytical sample (7.96%) closely mirrors this national figure, supporting the representativeness of the CFPS data. Their inclusion contributes to the comprehensive nature of the analysis and allows for potential subgroup analyses in future research. It is worth noting that family planning policies in China have historically varied across ethnic groups. The implementation of these policies for ethnic minorities is determined at the provincial level, resulting in substantial regional variation.[8] For instance, provinces such as Fujian and Yunnan had distinct regulations for ethnic minorities even before the implementation of the two-child policy. Fujian allowed minority couples to have two children under specific conditions, while Yunnan permitted two children for ethnic minorities, with additional allowances for certain subgroups.[9] Despite these historical differences in policy application, the main study does

[6] "The family planning tasks associated with transient populations shall be jointly managed by the people's governments of their places of origin and their current residences, and the latter shall play the main role." See Article 14 of the *Population and Family Planning Law of the People's Republic of China (2001)*.

[7] Han Chinese constitute the main ethnic group in China. The Han population was 1,220,844,520, accounting for about 91.6% of the national population in 2010 (excluding Hong Kong, Macao, and Taiwan); see the National Ethnic Affairs Commission of the People's Republic of China (n.d.).

[8] See Article 18 of the *Population and Family Planning Law of the People's Republic of China (2001)*.

[9] See *Regulation on Population and Family Planning of Fujian Province*. Exceptional conditions in Fujian:

not exclude ethnic minorities or treat them as a separate control group. Instead, they are included in the main analysis to provide a more complete picture of the policy's effects across China's population.

The panel structure of the data facilitates the inclusion of both employed and unemployed individuals in the sample, allowing for the observation of dynamic changes in employment status across all subjects over time. Among the unemployed cohort, an analysis of the underlying reasons for non-participation in the labour force reveals several key factors, including age, physical conditions, and domestic responsibilities. A significant observation is the prevalence of housework as a reason for unemployment. This category encompasses various domestic duties such as childbirth, childcare, and general household management. Although the response rate for this specific question was limited, the available data provide valuable insights. Figure C.2 in the Appendix illustrates a stark gender disparity: the overwhelming majority of individuals citing housework as the primary reason for unemployment are women.

This observation underscores the importance of focusing the research on female subgroups, particularly those who may become more vulnerable to labour market disadvantages following changes in population policy. The inclusion of both employed and unemployed individuals in the sample is crucial for a comprehensive analysis of the policy's impact on women's labour market outcomes. It allows for the examination of potential transitions between employment statuses and provides a more comprehensive understanding of the factors influencing women's labour force participation in the context of evolving family planning policies.

The sample for this study is restricted to individuals aged 20 to 50. While the official retirement age for women in China is 55, the upper age limit of 50 was chosen to align with the study's specific research objectives, taking into account both fertility considerations and potential age-related labour market discrimination. This age range ensures that the sample captures the prime working and childbearing years of the population under study.

The final sample comprises 7,285 individuals, consisting of 3,925 females and 3,360 males,

Two children can be born if one of the following circumstances is met: both spouses are rural residents or have lived or worked in an ethnic minority township or village for five years. *Regulation on Population and Family Planning of Yunnan Province.* Yunnan stipulates that ethnic minorities may have two children; ethnic minority farmers and herders with exceptional circumstances may have an additional child after approval; no limit on the number of children for ethnic groups with a small total population.

yielding a total of 34,829 observations over the period from 2010 to 2020, collected biennially. The primary focus of the analysis is on the female subpopulation, given the study's emphasis on the effects of fertility policies on women's labour market outcomes. Table 3.1 presents the summary statistics of key variables for the female subsample. This table provides an overview of the outcome variable, main explanatory variable, and control variables used in the analysis. For a more comprehensive view of the sample characteristics, Table C.3 in the Appendix offers detailed descriptive statistics for the entire sample, including separate summaries for the male subsample. This additional information allows for a broader understanding of the sample composition and potential gender differences in key variables.

Table 3.1: Summary Statistics for Female Respondents

	Mean	Std. Dev	Min	Max	N	Obs (N×T)
Log of raw income	9.495	1.198	1.609	12.948	3410	10047
Employment status	0.806	0.395	0	1	3925	18867
Post-2014 extra-birth eligible	0.463	0.499	0	1	3352	17908
Age	39.430	7.659	20	50	3925	18867
Years of education	7.605	4.470	0	19	3925	18867
Married	0.938	0.241	0	1	3925	18867
Log of GRP	10.509	0.422	9.482	11.765	3925	18867
Sex ratio	104.518	3.512	98.230	118.620	3925	18867
Log of family expenditure	10.572	0.844	6.215	15.450	3925	16564

Notes: The sample includes all female respondents in the panel dataset. N reports the number of distinct individuals contributing non-missing observations for each variable, and $N \times T$ reports the total number of person-year observations. The post-2014 extra-birth eligibility variable has a smaller sample size due to missing information on the number of children.

The primary outcome variable in this study is defined as the logarithm of annual wage income, which is observed only for individuals with positive earnings. Women with zero income are excluded from the wage regressions. This transformation allows for a more normalised distribution of the wage data and facilitates the interpretation of coefficients as percentage changes. Employment status is modelled separately as a distinct outcome (see Section 3.5.2), allowing the analysis to disentangle the policy's effects on the extensive margin (whether women work) from its effects on the intensive margin (how much working women earn). The main explanatory variable is the eligibility to have another child after the implementation of the two-child policy. Eligibility is determined by the number of children a woman had prior to the policy implementation. Approximately 5% of observations lack sufficient fertility history data in the CFPS to construct this variable and are therefore excluded from the regression analysis. By leveraging the unique personal identifiers in the CFPS and matching individuals with the family coefficient database, it is possible to accurately ascertain each woman's

fertility status before the policy change. This approach allows for a precise identification of those who became eligible for an additional child under the new policy framework.

To enhance the model's explanatory power and control for potential confounding factors, a series of covariates are included. At the individual level, these controls comprise age, years of education, marital status, and employment status. Regional-level controls include gross regional product per capita and sex ratio by province. Additionally, the logarithm of total family expenditure is included as a family-level control. These control variables are selected based on their theoretical relevance to both fertility decisions and labour market outcomes, as supported by existing literature in labour economics and demography. The inclusion of individual, family, and regional controls allows for a more comprehensive analysis of the policy effects, accounting for personal characteristics, household economic conditions, and broader economic factors that may influence the relationship between fertility and labour market outcomes.

3.4.2 Methodology

To explore the causal effects of the two-child policy on labour market outcomes, this study employs a difference-in-differences (DID) approach. This method allows for a comparison of outcomes between those affected by the policy and those not affected. Although the two-child policy was first implemented in 2014, with further relaxation of restrictions in 2016, the policy shock is considered to occur in 2014 to satisfy the no-anticipation assumption required for the DID method (Wooldridge, 2021). Setting the policy shock in 2016 would potentially confound the empirical results due to anticipation effects. The extension section will explore multiple time periods to examine the dynamic effects of the policy.

The female population is divided into two groups: the eligible and others. Based on previous studies (Cáceres-Delpiano, 2012; Lundborg et al., 2017; Zhou et al., 2021), which suggest that having more children negatively impacts mothers' workplace outcomes, it is hypothesized that women legally eligible for an extra child may face discrimination in the labour market. The eligible group is defined as women who had less than two children (specifically, one child or no children) at the beginning of 2014. A dummy variable, $Eligible$, is used to denote membership in the eligible group, with 1 indicating eligibility and 0 otherwise.

The difference-in-differences framework is as follows:

$$Wage_{ijt} = \beta_1 * Eligible_i * Post_{2014} + \beta_2 * Eligible_i + \beta_3 * Post_{2014} + X_{ijt} + \delta_i + \gamma_t + \epsilon_{ijt} \tag{3.1}$$

where i refers to each individual, j refers to the province in which individual i resides, and t refers to the year. The outcome variable $Wage_{ijt}$ is the logarithm of annual wage income, which is defined only for individuals with positive earnings; women with zero income are therefore excluded from the wage regressions. The logarithmic transformation is used to better fit a normal distribution and reduce heteroskedasticity, and this semi-elastic specification allows the coefficients to be interpreted as approximate percentage changes in wages. Individual, family, and region-level control variables (X_{ijt}) are included, affecting both fertility decisions and labour market outcomes. The individual control variables are age, marital status, and years of education. The regional control variables are GRP per capita and sex ratio by province. The family-level control is the logarithm of total family expenditure. δ_i refers to individual fixed effects, γ_t refers to year fixed effects, and ϵ_{ijt} represents the error term.

$Eligible_i$ equals 1 for women eligible to have an extra child (the eligible group), and 0 for women who already had two children at the beginning of 2014 (others). $Post_{2014}$ equals 1 for the year 2014 and onwards, and 0 otherwise. The interaction variable $Eligible_i * Post_{2014}$ is a dummy variable equal to 1 if and only if the observation is from the eligible group after the policy has been introduced. The coefficient of interest is β_1, which is the difference-in-differences estimate of the effect of the two-child policy implementation on women in China.

To provide a complete picture of the policy's labour market effects, I also examine the policy's impact on employment status as a binary outcome. The primary employment specification uses a probit model estimated on the full female sample, including women with zero earnings, thus capturing the policy's effect on the extensive margin of labour supply. Since fixed-effects probit is not estimable in short panels due to the incidental parameters problem, the probit specification includes province fixed effects rather than individual fixed effects, with standard errors clustered at the individual level. As a complementary specification, I also estimate a linear probability model (LPM) with individual and year fixed effects, which provides directly comparable identification to the wage equation. The separation of the wage and employment analyses is important: the wage regressions, being conditional on positive earnings, estimate the intensive-margin effect of the policy on working women's pay, while the employment regressions capture whether the policy affected women's likelihood of par-

ticipating in the labour market at all.

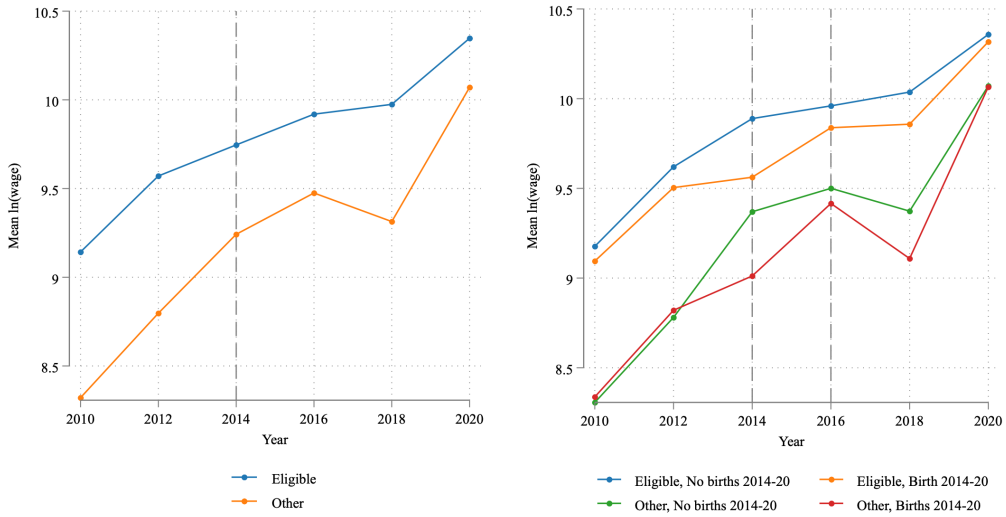

Figure 3.4: Average Log Wage Trends by Eligibility Status

To support the parallel trend assumption for the DID approach, the left graph in Figure 3.4 compares the average wage of the eligible group and others. This graph shows that both groups were on similar trajectories before 2014. Given that women's wages are likely to be affected by actually giving birth, the right graph in Figure 3.4 presents a more detailed average log wage trend, separating those who actually gave birth after policy implementation within both the eligible and other groups. This figure also shows parallel trends before the policy implementation, especially between those who actually gave birth and those who did not. While the raw wage trends show some fluctuation, particularly a decline for the other group in 2018, the regression analysis controls for individual fixed effects and year fixed effects, which account for time-invariant individual characteristics, person-specific wage trajectories, and common shocks affecting all groups in a given year. The difference-in-differences estimate therefore identifies the differential effect of policy eligibility, net of these individual and aggregate time trends. Figure C.3 uses an event study approach to further explore the support for the parallel trend assumption, showing no significant effect before 2014, followed by a generally negative effect on the eligible group.

3.5 Two-Child Policy Impact on Women's Labour Market Outcomes

The implementation of the two-child policy in China marks a significant shift in the country's population control measures. This section examines the policy's impact on women's labour

market outcomes, focusing on two primary dimensions. First, I analyse the wage penalties associated with the policy, exploring whether and to what extent the relaxation of fertility restrictions has affected women's earnings. Second, I examine the policy's effects on employment probability, investigating whether the two-child policy has altered women's labour force participation. By studying these dimensions, I aim to provide a comprehensive understanding of how the two-child policy has reshaped the landscape of female labour market outcomes in China.

3.5.1 Wage Penalties Associated with the Two-Child Policy

Table 3.2: DID Regression Results: Baseline Model

	(1)	(2)	(3)	(4)	(5)	(6)
$Eligible \times Post_{2014}$	-0.431***	-0.298***	-0.323***	-0.297***	-0.284***	-0.173***
	(0.048)	(0.048)	(0.045)	(0.048)	(0.048)	(0.051)
$Post_{2014}$	1.096***	1.690***	1.648***	1.732***	1.618**	1.751
	(0.038)	(0.047)	(0.041)	(0.645)	(0.641)	(1.078)
$Eligible$	0.852***		0.692***			
	(0.048)		(0.046)			
Individual Ctrl				✓	✓	✓
Provincial Ctrl					✓	✓
Family Ctrl						✓
Individual FE		✓		✓	✓	✓
Provincial FE			✓			
Year FE		✓	✓	✓	✓	✓
Observations	9,599	9,599	9,599	9,599	9,599	7,690
R-squared	0.187	0.312	0.304	0.314	0.315	0.226

Notes: The dependent variable is the logarithm of annual wage income, restricted to women with positive earnings. The sample comprises a balanced panel of females aged 20-50 from the CFPS (2010-2020). Individual controls include age, marital status, and years of education. Regional controls include GRP per capita and sex ratio. Family control is the logarithm of total family expenditure. Standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3.2 reports the impact of the Two-Child Policy on the wages of women eligible to have additional children, with subsequent columns progressively adding controls and fixed effects. Across all specifications, the coefficients on the interaction term $Eligible \times Post_{2014}$ are negative and statistically significant at the 1% level, indicating that women who became eligible to have more children after the policy implementation experienced a decrease in wages relative to women who already had two children. These baseline results support the initial hypothesis that women face wage penalties as a consequence of the policy. The coefficient decreases as additional controls and fixed effects are introduced, suggesting that a substantial portion of the raw wage differential is attributable to observable individual, regional, and family characteristics as well as time-invariant individual heterogeneity. Nev-

ertheless, the coefficient in the preferred specification remains economically meaningful and highly significant, indicating that the policy is associated with a reduction in annual wages of approximately 16 per cent for eligible women.[10]

The stability of the negative coefficient across specifications needs further comment. As controls are progressively added, the coefficient declines substantially but remains robustly negative, indicating that the wage penalty cannot be explained by differences in human capital, regional economic conditions, or family circumstances between eligible and ineligible women. The persistence of a significant effect in the fully specified model, which includes individual fixed effects that absorb all time-invariant individual characteristics, suggests that the observed wage penalty reflects a genuine change in the labour market following policy implementation, rather than pre-existing differences between the two groups.

It is important to consider the potential endogeneity in the above model. Intra-household fertility decisions, while influenced by policy, are not solely determined by it; the Two-Child Policy grants some women the right to have more children, but it does not mandate that they exercise this right. Moreover, fertility behaviour in response to policy changes tends to occur over an extended period. Previous studies (Cáceres-Delpiano, 2012; Lundborg et al., 2017; Zhang, 2017) have demonstrated that fertility behaviour can be detrimental to women's employment. Therefore, the primary focus of this analysis is to ascertain whether the Two-Child Policy resulted in labour market discrimination against women in the form of lower wages. To obtain a clear impact, it is essential to disentangle the policy effect from the effect of actual fertility decisions. Consequently, this study aims to isolate the net effect of the Two-Child Policy by examining outcomes separately from career interruptions or other wage penalties directly caused by childbirth.

To address this fertility-induced concern, this study extends the baseline analysis by examining outcomes specifically for women who did not actually give birth after the policy implementation. Figure 3.4 (right graph) illustrates that women who actually gave birth after the policy implementation not only experienced a lower wage growth rate but also witnessed a

[10]The magnitude of this estimate is broadly consistent with recent studies examining China's fertility policy relaxation. Chen and Wang (2024) find that the 2016 universal Two-Child Policy reduced women's employment income by approximately 20.86 per cent, whilst Huang et al. (2024) report that the same policy expanded the urban gender wage gap by 12.86 per cent. Differences in outcome definitions, sample composition, and identification strategies across the three studies preclude precise comparisons, but the convergence of findings across distinct policy shocks and empirical approaches strengthens the overall evidence that China's fertility policy relaxation has imposed measurable wage costs on women.

decreasing average wage within their own group (in both the eligible and other groups). To isolate the effect of eligibility from actual fertility decisions, a subsample of women whose child status remained unchanged since 2014 was selected for regression analysis. The results are presented in columns (1) and (2) of Table 3.3. These results remove the impact of having additional children on women and focus on the effects of the policy itself, demonstrating that even without actually giving birth, the eligibility to have more children impacts women's labour market outcomes. The underlying assumption is that no individual has additional children after the two-child policy, thus eliminating the fertility effect. Whilst this approach does not fully resolve all sources of endogeneity, it provides valuable insights into the policy's effect independent of realised fertility choices.

Table 3.3: DID Regression Results by Number of Children

	No births after 2014		Women with at least 1 child		Exclude 1-child family	
	(1)	(2)	(3)	(4)	(5)	(6)
$Eligible \times Post_{2014}$	-0.333***	-0.185***	-0.308***	-0.193***	-0.209**	-0.022
	(0.057)	(0.061)	(0.049)	(0.051)	(0.095)	(0.104)
Controls		✓		✓		✓
Individual FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	6,458	4,852	9,019	7,221	5,012	3,919
R-squared	0.350	0.263	0.315	0.232	0.338	0.242

Notes: The dependent variable is the logarithm of annual wage income, restricted to women with positive earnings. The sample comprises a balanced panel of females aged 20-50 from the CFPS (2010-2020). All regressions include $Eligible$ and $Post_{2014}$, but they are absorbed by year effects or individual effects in some regressions, so the results are not reported. Individual controls include age, marital status, and years of education. Regional controls include GRP per capita and sex ratio. Family control is the logarithm of total family expenditure. Standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

This analysis provides compelling evidence that the mere possibility of having additional children, rather than actual fertility changes, influences women's wages. Such findings suggest that the policy's impact extends beyond its direct effect on fertility, potentially reflecting changes in employer behaviour or women's career decisions in anticipation of possible future fertility. While not resolving the endogeneity issue, these results offer crucial insights into the broader labour market implications of the Two-Child Policy, highlighting the need for comprehensive policy approaches that consider both direct and indirect effects on women's economic outcomes. The consistency of the negative effect, even when controlling for actual fertility behaviour, underscores the far-reaching consequences of the policy on women's labour market experiences and emphasises the importance of considering unintended conse-

quences in policy design and implementation.

Moreover, this finding mitigates a related concern regarding the composition of the wage regression sample. Since the wage analysis is restricted to women with positive earnings, differential exit from employment among eligible women could, in principle, alter the composition of the working sample and bias the wage estimates. The fact that the wage penalty persists among women who had no births after 2014 – and who are therefore unlikely to have exited employment due to childbearing – suggests that the main wage results are not driven by such compositional changes in the workforce.

To further examine the policy's effect on the most directly affected group, columns 3 and 4 of Table 3.3 restrict the eligible group to women with exactly one child, comparing them to women with two or more children. Women with one child represent the primary target of the Two-Child Policy from both a policy and employer discrimination perspective. Not only did they gain the right to have a second child under the reform, but they are also more likely to actually exercise this right compared to childless women, who would need to have two sequential births to reach the same family size. As noted in the background section, one-child families constitute the majority of the labour force, making this comparison particularly policy-relevant. The results show a significant wage penalty for women with one child (Table 3.3, column 4). This estimate is slightly larger in magnitude than the baseline result that includes all eligible women (Table 3.2, column 6), suggesting that the wage discrimination is particularly concentrated among women who are most likely to have a second child.

As society develops, women's willingness to have children generally decreases, and those who have already had one child can be considered a group with fertility potential. This interpretation is further supported by the results in the final two columns of Table 3.3, which exclude one-child mothers from the regression. Specifically, these columns compare the difference between women who did not have children and those in the other group (women who already had two children at the beginning of 2014) after the two-child policy was implemented. The results are no longer significant (column 6), indicating that the significant negative effect is primarily driven by one-child women. These findings suggest that women with one child are the subgroup that suffers the most from the policy change in terms of wage penalties.

The consistent negative coefficients across all regression models provide strong evidence that

the two-child policy has indeed resulted in wage penalties for eligible women. This finding aligns with theoretical predictions of statistical discrimination, where employers may anticipate higher costs or lower productivity from women who are now eligible to have more children, even if they do not actually do so. The larger and statistically significant estimate obtained after removing the effects of actual fertility behaviour (Table 3.3, column 2) is particularly revealing. It suggests that the mere possibility of having additional children, rather than the act itself, is driving the wage penalty. This points to a form of anticipatory discrimination in the labour market, where women's potential, rather than actual fertility decisions, are influencing their wage outcomes.

The more pronounced effect on one-child families (Table 3.3, columns 3 and 4) underscores the policy's disproportionate impact on this group. This could be because employers view women with one child as more likely to have a second child compared to women with no children, who may be perceived as less family-oriented. Alternatively, it could reflect a societal expectation that one-child families "should" have a second child, leading to greater discrimination against this group.

Table 3.4: DID Regression Results by Firstborn Gender

	Firstborn: Girl		Firstborn: Boy	
	(1)	(2)	(3)	(4)
$Eligible \times Post_{2014}$	-0.343***	-0.205***	-0.337***	-0.214**
	(0.072)	(0.075)	(0.082)	(0.087)
Controls		✓		✓
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	3,454	2,682	3,793	2,970
R-squared	0.355	0.277	0.318	0.231

Notes: The outcome variable is the logarithm of annual wage income. All regressions control $Eligible$ and $Post_{2014}$, but they are absorbed by year effects or individual effects in some regressions, so the results are not reported. A pooled specification including both groups with a triple interaction term ($Eligible \times Post_{2014} \times$ Firstborn Boy) yields a coefficient of -0.076 (SE = 0.118, p = 0.517), indicating no statistically significant difference between the two groups. Standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A further analysis examines whether the wage penalty differs based on the gender of the firstborn child. Despite the well-documented preference for boys in Chinese families (Das Gupta et al., 2003), Table 3.4 shows that women with a firstborn girl and those with a firstborn boy both experience significant wage penalties after the policy implementation. Testing for differential effects using a pooled specification with a triple interaction term indicates that these effects are not statistically distinguishable from one another ($p = 0.517$), suggesting that em-

ployers' anticipation of potential second births, and the associated discrimination, operates relatively uniformly regardless of the firstborn's gender. This finding indicates that the two-child policy may have, to some extent, neutralised the impact of traditional son preference on labour market discrimination, as women are penalised for their eligibility rather than their specific family composition.

3.5.2 Employment Effects of the Two-Child Policy

Table 3.5: Impact of the Two-Child Policy on Women's Employment Status

	All Women		No births after 2014	
	Probit	LPM	Probit	LPM
$Eligible \times Post_{2014}$	-0.350***	-0.100***	-0.430***	-0.129***
	(0.051)	(0.013)	(0.065)	(0.016)
$Eligible$	0.265***		0.406***	
	(0.046)		(0.062)	
$Post_{2014}$	0.842***		0.896***	
	(0.044)		(0.057)	
Observations	15,600	15,605	9,404	9,412
Pseudo R^2/R^2	0.113	0.479	0.131	0.470
	Women with at least 1 child		Exclude 1-child family	
	Probit	LPM	Probit	LPM
$Eligible \times Post_{2014}$	-0.365***	-0.107***	-0.171	-0.042
	(0.053)	(0.013)	(0.120)	(0.036)
$Eligible$	0.296***		0.071	
	(0.047)		(0.136)	
$Post_{2014}$	0.845***		0.863***	
	(0.045)		(0.050)	
Observations	14,845	14,857	9,257	9,261
Pseudo R^2/R^2	0.115	0.478	0.129	0.467

Notes: The dependent variable is a binary indicator equal to one if the individual is employed. Probit columns report probit coefficients, which indicate the direction and significance of the effects but are not directly interpretable as marginal effects; average marginal effects are presented graphically in Figure 3.5. LPM columns report linear probability model (LPM) estimates, which can be interpreted as changes in the probability of employment. The probit specification includes province fixed effects; the LPM specification includes individual and year fixed effects, which absorb the main effects of $Eligible$ and $Post_{2014}$. All specifications include controls for age, years of education, marital status, GRP per capita, sex ratio, and log family expenditure. Standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In addition to the wage effects documented above, I examine the policy's impact on women's employment status to provide a complete picture of its labour market consequences. Table 3.5 presents the results using both a probit model and a linear probability model (LPM). The probit specification includes province fixed effects, while the LPM includes individual and year

fixed effects, providing a complementary identification strategy (see Section 3.4). Across all women (columns 1-2), the interaction term $Eligible \times Post_{2014}$ is negative and statistically significant in both specifications, indicating that the policy reduced the employment probability of eligible women. This result is robust to the choice of functional form: the probit coefficient indicates the direction and significance of the effect, while the LPM estimate of approximately 10 percentage points provides a directly interpretable magnitude. The results are further disaggregated by subsamples: women with no births after 2014 (columns 3-4), women with at least one child (columns 5-6), and women excluding one-child families (columns 7-8). Consistent with the wage analysis, the negative employment effect is concentrated among women with at least one child, while the effect for the sample excluding one-child families is not statistically significant, reinforcing the finding that one-child women bear the brunt of the policy's adverse labour market consequences.

Figure 3.5 illustrates the marginal effects of eligibility on employment probability before and after 2014, derived from the probit specification. In the pre-policy period ($Post_{2014} = 0$), being eligible is associated with an increased probability of employment. However, in the post-policy period, this relationship reverses, with eligibility associated with a decreased probability of employment. This suggests that the two-child policy may have reduced the employment advantage that eligible individuals previously held. Before 2014, being eligible (i.e., having fewer than two children) was associated with higher employment probability. This could be because women with fewer children were perceived as more committed to their careers or because they faced fewer work-family conflicts. The positive association might also reflect selection effects, where more career-oriented women chose to have fewer children.

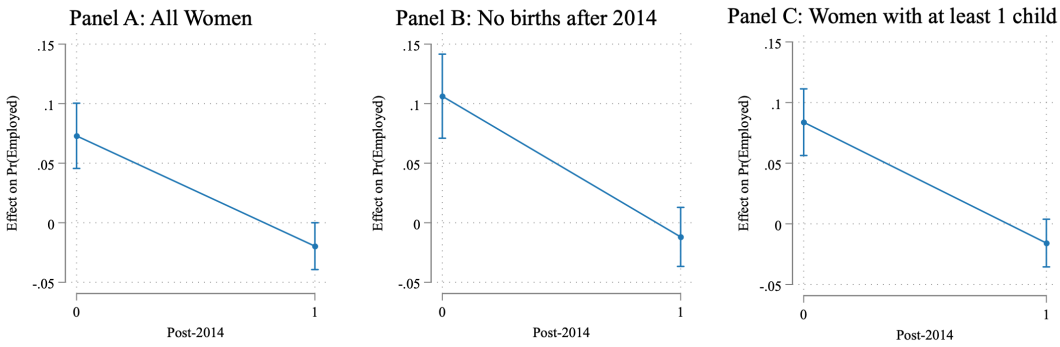

Figure 3.5: Marginal Effects of Eligibility on Employment Probability Before and After 2014

There are some potential explanations for the reversal of the relationship between eligibility and employment probability. One is employer-driven statistical discrimination. The policy

change may have altered employers' perceptions and expectations regarding female employees' future productivity and attachment to the labour force. Anticipating potential increases in fertility-related absences and associated costs, employers might exhibit a decreased propensity to hire or retain eligible women. This aligns with Becker (1971)'s theory of statistical discrimination, where employers make decisions based on group characteristics rather than individual attributes.

Furthermore, the shift in population policy potentially influenced women's labour market decisions by adjusting their career expectations. Some women might opt for labour market exit or reduced participation in anticipation of expanded familial responsibilities, even in the absence of immediate fertility changes. This phenomenon can be contextualised within the framework of Mincer and Polachek (1974)'s human capital model, which posits that expected future labour force interruptions can influence current labour decisions. Lastly, the two-child policy may have reinforced or reshaped societal expectations regarding women's roles, potentially legitimising a greater emphasis on family responsibilities over career pursuits. This shift in social norms could be interpreted through Akerlof and Kranton (2000)'s identity economics, which suggests that changes in social categories and associated behavioural expectations can significantly impact economic outcomes.

These employment effects, combined with the wage penalties discussed earlier, paint a comprehensive picture of the challenges that eligible women face in the labour market following the implementation of the two-child policy. The findings suggest that the policy has had unintended consequences, potentially exacerbating gender inequalities in the workplace.

3.6 Heterogeneity Effects of the Two-Child Policy

The impact of the two-child policy on women's labour market outcomes exhibits considerable heterogeneity across demographic and occupational subgroups. This section examines the differential effects of the policy with respect to age, education level, job category, and urban-rural residence. To investigate these heterogeneous effects, I employ subsample analyses using Equation (3.1). The key regression coefficients and their associated 95% confidence intervals are visually presented in the following subsections, with detailed regression results available in the appendix.

3.6.1 Age-related Heterogeneity

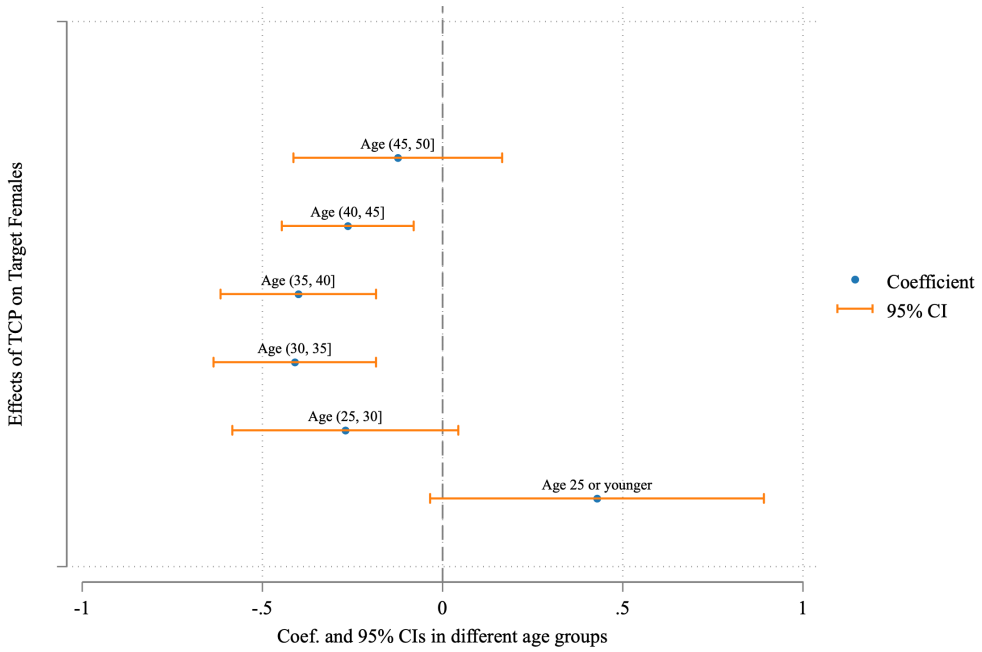

Figure 3.6: Heterogeneous Effects Across Different Age Groups

Figure 3.6 illustrates the policy's differential impact across age cohorts. The results reveal a clear gradient in which the negative wage effects are concentrated among women in their prime childbearing and early career years, with the intensity of the penalty varying systematically by age. Women aged 30-35 and 35-40 demonstrate the largest and most precisely estimated negative coefficients. This finding aligns with the lifecycle theory of labour supply (Mincer and Polachek, 1974), which posits that women in these years face the greatest trade-offs between work and family responsibilities, as the competing demands of career progression and childrearing are at their most acute. From the demand side, the concentration of effects in this age range is also consistent with employer-side statistical discrimination: following the relaxation of fertility restrictions, employers may perceive women aged 30-40 as the group most likely to exercise their newly granted right to a second child, leading to anticipatory wage adjustments or reduced investment in these workers' career development (Kleven et al., 2019).

The negative effects extend to women aged 25-30, though at a somewhat smaller magnitude. This group represents a transitional cohort: they are beginning to enter the phase of the lifecycle in which fertility decisions become more immediate, and employers may accordingly start to factor perceived fertility risk into wage-setting and hiring decisions. The fact that the penalty is already evident for this group, albeit less pronounced than for the 30-40 cohort,

suggests that statistical discrimination operates not only in response to imminent childbearing expectations but also on the basis of anticipated future fertility potential.

The impact attenuates for women aged 40-45, who still exhibit a negative effect but of a smaller magnitude than their younger counterparts. This mitigation likely reflects the greater stability in both career trajectories and family composition typically achieved by this age group, as well as diminished employer expectations of further childbearing. For women aged 45-50, the effect becomes small and statistically insignificant, consistent with the interpretation that employers no longer perceive meaningful remaining fertility potential for this cohort, rendering the policy change effectively irrelevant to their labour market treatment.

Interestingly, women aged 25 or younger exhibit a positive and marginally significant effect. This result may reflect the fact that employers perceive this cohort as facing less immediate fertility pressure, given the longer expected interval before childbearing, which reduces the intensity of statistical discrimination in the short term. Additionally, this younger cohort may benefit from entering the labour market during a period when attitudes towards working mothers and family-friendly policies were evolving, or they may possess educational credentials that provide greater protection against discrimination. However, this finding should be interpreted with caution given the small subsample and marginal significance level. The detailed regression results for age-related heterogeneity are presented in Table C.4.

3.6.2 Education-level Heterogeneity

Figure 3.7 presents the policy's effects across various education levels. The most striking finding is the significant positive effect for women with college education and above, which runs counter to the overall negative trend observed in the baseline results. This finding suggests that higher education serves as a substantial buffer against the policy's adverse labour market impacts. Higher education is often associated with increased labour market attachment, higher opportunity costs of leaving employment, and greater bargaining power (Eckstein and Lifshitz, 2011). Moreover, highly educated women are more likely to be employed in occupations that offer family-friendly policies and greater workplace flexibility, which may mitigate the negative effects of the Two-Child Policy on this group.

Women with senior high school or vocational senior school education face a small and statistically insignificant negative effect, with confidence intervals comfortably crossing zero.

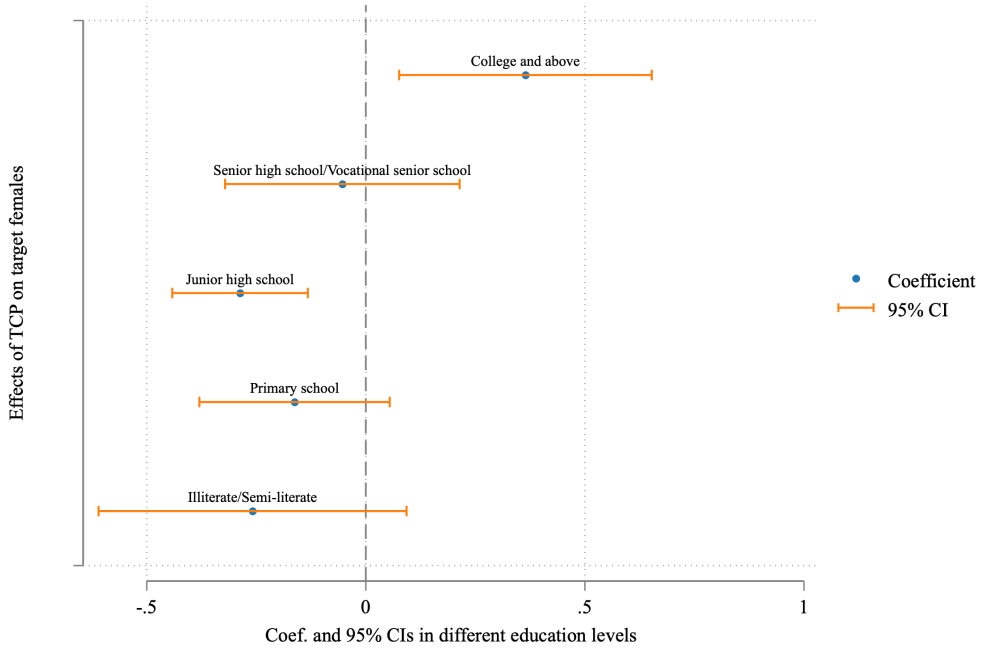

Figure 3.7: Heterogeneous Effects Across Different Education Levels

This group may occupy an intermediate position in which their educational attainment provides some degree of labour market protection, but not to the extent observed among college-educated women.

The most robust negative impact is found among women with junior high school education, who exhibit a highly significant wage penalty estimated on the largest education subsample. This group occupies a particularly vulnerable position in the labour market: they are sufficiently integrated into formal employment to be exposed to employer-side statistical discrimination based on perceived fertility potential, yet they lack the educational credentials, specialised skills, or bargaining power that might afford protection against such discrimination. In the Chinese context, junior high school represents the terminal educational qualification for a substantial proportion of women who subsequently enter manufacturing, low-skilled service, or routine clerical employment – precisely the occupational segments in which employers may perceive the highest costs associated with potential maternity leave and childcare-related absences.[11] Furthermore, as Brewster and Rindfuss (2000) suggest, women with lower levels of education may face greater societal expectations to prioritise childbearing and family responsibilities over career advancement, compounding the labour market disadvantage induced by the policy.

[11] Junior high school (grade 9) is the final year in China's nine-year compulsory education system.

The pattern across the lowest education levels reveals a non-monotonic relationship that warrants careful interpretation. Women with primary school education show a negative but statistically insignificant effect, whilst illiterate or semi-literate women exhibit a larger point estimate that reaches marginal significance. However, the estimates for both groups are considerably less precise than those for the junior high school subsample, reflecting smaller sample sizes and greater heterogeneity in labour market experiences at the bottom of the education distribution. Substantively, women with very low levels of formal education are disproportionately concentrated in informal or agricultural employment, where formal employment relationships are less prevalent and the policy's impact is likely to operate through different channels (such as adjustments to working hours or task allocation) that are less readily captured in standard wage regressions. The detailed regression results for education-level heterogeneity are presented in Table C.5.

3.6.3 Job Category Heterogeneity

Figure 3.8 shows the policy's impact across various job categories. The occupational heterogeneity observed in these results can be understood through the lens of job flexibility and worker replaceability. As Goldin (2014) argues, occupations differ substantially in the degree to which they penalise temporal inflexibility – the extent to which physical presence during fixed hours is essential, and workers can be readily substituted for one another. In the context of the Two-Child Policy, these occupational characteristics are likely to mediate the intensity of employer-side statistical discrimination, as employers in rigid, low-flexibility sectors face higher anticipated costs from potential maternity-related absences and are therefore more inclined to adjust wages or hiring practices in response to perceived fertility risk.

Consistent with this framework, production workers experience the largest and most statistically significant negative effects, suggesting that women in manufacturing and industrial occupations face substantial wage penalties following the policy implementation. These occupations typically involve fixed schedules, physical presence requirements, and limited scope for flexible work arrangements. Workers in these roles are also comparatively easy to substitute, which reduces employers' incentive to retain individual workers through the period of a potential career interruption. Agricultural workers show negative effects, though these estimates do not reach statistical significance, likely reflecting the heterogeneous nature of agricultural employment (spans formal wage labour and informal self-employment) and the consequent difficulty of capturing a uniform policy effect in this category.

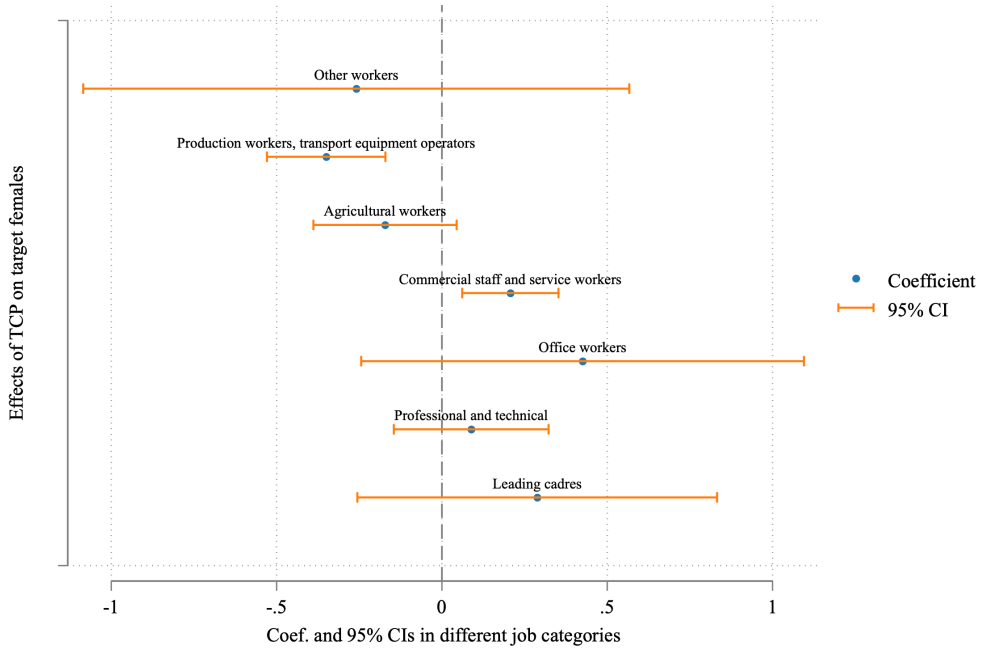

Figure 3.8: Heterogeneous Effects Across Different Job Categories

In contrast, service workers exhibit a significant positive effect, indicating that women in customer-facing roles experienced wage increases rather than penalties following the policy change. This finding, which initially appears to contradict the baseline results, may be consistent with the statistical discrimination framework when considered alongside sector-specific labour market conditions. During this period, China's rapidly expanding service economy was characterised by persistent labour shortages and intense competition for workers, which may have reduced employers' capacity to discriminate based on perceived fertility risk. Additionally, many service-sector roles may offer greater scheduling flexibility than manufacturing positions, further reducing the perceived cost of helping workers with family responsibilities.

Professional and technical workers appear relatively unaffected, with coefficients near zero. This group benefits from specialised skills and occupation-specific human capital that make them less easily replaceable, which shifts the balance of bargaining power in their favour and reduces employers' ability to impose fertility-related wage penalties. Office workers and leading cadres show positive point estimates, but these are estimated on small subsamples with wide confidence intervals and are statistically insignificant, indicating no strong conclusions about the policy's impact on these groups.

The pattern of occupational heterogeneity is broadly consistent with the findings from the

education-level analysis: the occupational categories that experience the largest negative effects (production and agricultural workers) tend to employ women with lower levels of formal education, whilst the categories that are protected (professional, technical, and service workers) tend to employ women with higher educational attainment and greater labour market bargaining power. The detailed regression results for job category heterogeneity are presented in Table C.6.

3.6.4 Urban-Rural Heterogeneity

Building upon the analysis of job category heterogeneity, I now turn to the urban-rural divide. This dimension is particularly salient given that rural areas in China typically exhibit less diverse economic structures, with a greater dependence on agriculture and informal employment, and such working environments may offer reduced protection against discrimination and fewer opportunities for women to balance work and family responsibilities. To explore potential urban-rural differences, I utilise the urban-rural classification provided by the CFPS project, which follows standards issued by the National Bureau of Statistics to determine whether respondents primarily reside in rural or urban areas.[12]

As illustrated in Figure 3.9, the results reveal a divergence between urban and rural women, where rural women experience a negative and statistically significant effect. This urban-rural divergence can be understood as the spatial patterns documented in the previous subsections. The urban labour market combines those protective factors identified in the age, education, and occupational analyses. Additionally, they have greater access to formal childcare infrastructure, which reduces the perceived cost of combining work with an additional child. Conversely, the rural labour market concentrates the vulnerability factors: lower education levels, less job flexibility, higher replaceability, and stronger adherence to traditional gender norms that emphasise women's domestic responsibilities. The Two-Child Policy may have reinforced these norms, leading to greater labour market penalties for rural women who are already more constrained in their ability to signal sustained labour market commitment.

A crucial factor that may exacerbate the policy's negative effects in rural areas is the rela-

[12]The urban-rural classification code is a three-digit number, formulated in accordance with the State Council's approved *Regulations on the Statistical Classification of Urban and Rural Areas" (State Council Document [2008] No. 60)* and the *Rules for the Compilation of Statistical Division Codes and Urban-Rural Classification Codes"* issued by the National Bureau of Statistics (*National Statistics Document [2009] No. 91*). This classification applies to the 31 provinces (autonomous regions and municipalities) where the National Bureau of Statistics surveys, excluding Taiwan Province, the Hong Kong Special Administrative Region, and the Macao Special Administrative Region.

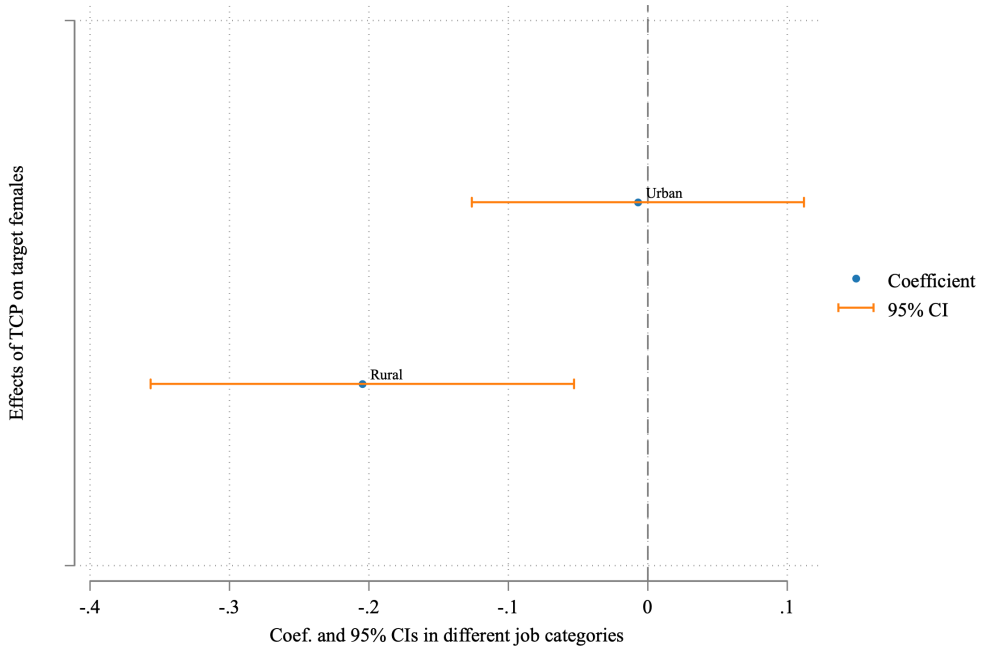

Figure 3.9: Heterogeneous Effects by Urban-Rural Classification

tive absence of formal childcare facilities. Urban areas typically have better access to institutional childcare, which can mitigate the impact of increased fertility on women's labour market participation and reduce the anticipated costs that employers associate with potential maternity-related absences. The lack of accessible, affordable childcare in rural regions may force more women to choose between work and family responsibilities, and may simultaneously strengthen employers' expectations that eligible women will indeed withdraw from the labour force, thereby intensifying statistical discrimination.

Taken together, the heterogeneity results across age, education, occupation, and geography tell a coherent story. The policy's adverse effects are concentrated among women who face the greatest exposure to employer-side statistical discrimination and possess the fewest resources to buffer against it: women in their prime childbearing years, with lower levels of education, employed in low-flexibility occupations, and residing in rural areas where institutional support structures are weakest. These findings have important implications for the design of targeted policy interventions, suggesting that efforts to mitigate the unintended labour market consequences of pro-natalist policies should be directed toward the most vulnerable group of the female workforce. The detailed regression results for urban-rural heterogeneity are presented in Table C.7.

3.7 Robustness Checks and Extended Analyses

It is important to ensure the validity and applicability of the main findings regarding the impact of China's two-child policy on female employment outcomes. This section presents a series of robustness checks and extended analyses to support and enrich the primary results.

3.7.1 Robustness Check

I implement additional strategies to validate the robustness of primary findings. The main analysis utilised a balanced panel from the China Family Panel Studies (CFPS) from 2010 to 2020, including only individuals present in all waves. While this approach ensures sample consistency, it potentially limits the analysis by excluding individuals who entered or exited the survey during the study period.

To address this limitation and enhance the representativeness of the analysis, the first robustness check involves expanding the sample to an unbalanced panel. This unbalanced panel incorporates all available observations, including individuals who joined the survey after 2010, those who dropped out before 2020, and those who missed intermediate waves. By relaxing the requirement for continuous presence across all waves, I significantly increase the sample size and capture a more comprehensive picture of population dynamics over the study period. Using this expanded unbalanced panel, I replicate the main difference-in-differences regression model, as specified in Equation (3.1). This approach assesses whether the main findings remain consistent when accounting for a more dynamic and potentially more representative population sample. The results of this analysis are shown in Table 3.6, along with a plot of marginal effects of eligibility on employment probability in Figure C.4.

The unbalanced panel analysis results support the main findings, showing the robustness of the observed wage penalties and the decreased likelihood of employment for eligible women after the two-child policy. This consistency across balanced and unbalanced panels strengthens the validity of the main conclusions. It mitigates concerns about potential attrition bias, which could arise from systematic differences between individuals who remain in the survey throughout and those who do not.

As an additional robustness check, I explore an alternative specification using ethnicity as a proxy for treatment, given that the one-child policy was more strictly applied to Han Chinese.

Table 3.6: Robustness Checks: Unbalanced Panel

	TWFE LHS: Wage		Probit LHS: Employment	
	(1)	(2)	(3)	(4)
$Eligible \times Post_{2014}$	-0.208***	-0.106**	-0.375***	-0.362***
	(0.043)	(0.045)	(0.035)	(0.037)
Controls		✓		✓
Individual FE	✓	✓		
Year FE	✓	✓		
Observations	14,620	12,475	28,071	25,441
R-squared	0.262	0.206		
Pseudo R2			0.048	0.069

Notes: All regressions control $Eligible$ and $Post_{2014}$, but they are absorbed by year effects or individual effects in some regressions, so the results are not reported. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

I compare Han Chinese women (treatment group) to ethnic minority women (control group) using a difference-in-differences model. This model included indicators for Han ethnicity and the post-2014 period, as well as the interaction term between these two variables. It also included the standard control variables, fixed effects for province and year. The results, presented in Table 3.7 columns (1) and (2), show no significant effect for Han Chinese women relative to ethnic minority women after the policy relaxation. This specification tests whether the wage penalty could be driven by employers making broad assumptions about Han Chinese women as a group, given that the one-child policy was historically applied more strictly to them. The null result indicates this is not the case. Instead, the significant negative effects found in the baseline analysis emerge only when treatment is defined by individual-level eligibility (women with 0-1 children vs. those with 2+ children). This confirms that the labour market discrimination operates at the individual level, with employers responding to specific women's eligibility status rather than making blanket assumptions based on ethnicity.

To further validate the focus on the female population, I extend the analysis to include both male and female populations. I introduce a triple interaction term, "$Eligible \times Post_{2014} \times Female$", to the difference-in-differences model. The results of this analysis are presented in Table 3.7, columns (3) and (4). The findings strongly support the initial approach. While the coefficient for "$Eligible \times Post_{2014}$" is positive and significant, indicating a general increase in log annual income for those eligible for an additional child after 2014, the coefficient for the triple interaction "$Eligible \times Post_{2014} \times Female$" is substantially negative. This indicates that women experienced a significantly different, and indeed negative, effect compared to

Table 3.7: Robustness Checks: Ethnicity-Based Treatment and Gender Differences

	Ethnicity Treatment		Gender Difference	
	(1)	(2)	(3)	(4)
$Han\ Chinese \times Post_{2014}$	-0.022	0.031		
	(0.094)	(0.097)		
$Eligible \times Post_{2014}$			0.038	0.119***
			(0.031)	(0.033)
$Eligible \times Post_{2014} \times Female$			-0.395***	-0.466***
			(0.026)	(0.027)
Controls		✓		✓
Provincial FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	10,056	8,147	20,259	16,489
R-squared	0.262	0.340	0.251	0.301

Notes: The outcome variable is the logarithm of annual wage income. All regressions control $Eligible$ and $Post_{2014}$, but they are absorbed by year effects or individual effects in some regressions, so the results are not reported. Robust standard errors in parentheses, clustered at the individual level. Results remain substantively unchanged when clustering at the provincial level. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

men. The magnitude of this negative interaction more than offsets the general positive effect, resulting in a net negative impact on women's log annual income.

These robustness checks collectively reinforce the validity of the main findings, demonstrating the consistency of the observed negative impact on women's wages across different sample specifications and analytical approaches. They also highlight the specificity of this effect on women eligible for an additional child, rather than the general trend across all demographic groups.

3.7.2 Dynamic Treatment Effects

As introduced in the policy background, the relaxation of the one-child policy occurred in two stages. In 2014, the government introduced a selective two-child policy, allowing couples where one spouse was from an only-child family to have a second child. In 2016, this policy was further relaxed to allow all couples to have a second child without formal application or reporting.

To analyse the impact of these policy changes, I employ a multiple time periods approach. The study covers six-year periods ($t = 2010, 2012, 2014, 2016, 2018, 2020$) with no units legally eligible for a second child in $t = 2010$ and 2012. From $t = 2014$, couples where either spouse is from a one-child family become eligible ($Eligible_{2014}$). This design satisfies the

assumptions of staggered interventions with no reversibility (Wooldridge, 2021), meaning that once a unit is subjected to the intervention, it remains treated. An additional cohort ($Eligible_{2016}$) joins the eligible group in 2016, alongside the 2014 cohort ($Eligible_{2014}$). A "never treated" group consists of those who already had their second child by 2014.

To implement this design, I use period dummies instead of a simple pre/post dummy, and individual dummies rather than a treated/not-treated group. A dummy variable $Treat$ takes the value of 1 if individual i is already treated at time t. Specifically, for women without a second child, $Treat$ is 1 from 2014 onward if one spouse is from an only-child family, and 1 from 2016 onward if neither spouse is from an only-child family. For women with two children by 2014, this term remains 0. This leads to estimating a two-way fixed effect (TWFE) model:

$$Wage_{it} = \alpha_0 + \alpha_i + \alpha_t + \alpha_3 \times Treat_{it} + \epsilon_{it} \tag{3.2}$$

While TWFE has been the traditional approach, it can lead to biased estimates when treatment effects are heterogeneous across groups or time periods (Goodman-Bacon, 2021). To address this, I also employ newly developed solutions for multiple time period difference-in-differences (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). Figure 3.10 presents the dynamic treatment effects of the two-child policy on women's wages, comparing these newer methods with the traditional TWFE OLS approach.

The pre-treatment period (2010-2012 for the 2014 cohort and 2010-2014 for the 2016 cohort, corresponding to -3 to -1 in Figure 3.10) shows no statistically significant differences between treatment and control groups across all methods, supporting the parallel trends assumption and validating the DID approach. This suggests that before the policy implementation, there were no significant differences in wage trends between the treatment and control groups. The post-treatment effects (2014-2020 for the 2014 cohort and 2016-2020 for the 2016 cohort, corresponding to 0-3 in Figure 3.10) reveal a consistent downward trend across all estimation methods, indicating a negative impact of the two-child policy on women's wages. This negative effect appears to intensify over time, with the largest impacts observed in the later periods. The growing magnitude of the effect suggests that having (or being eligible for) a second child on women's wages may be cumulative, potentially due to factors such as increased childcare responsibilities, career interruptions, or shifts in employer perceptions.

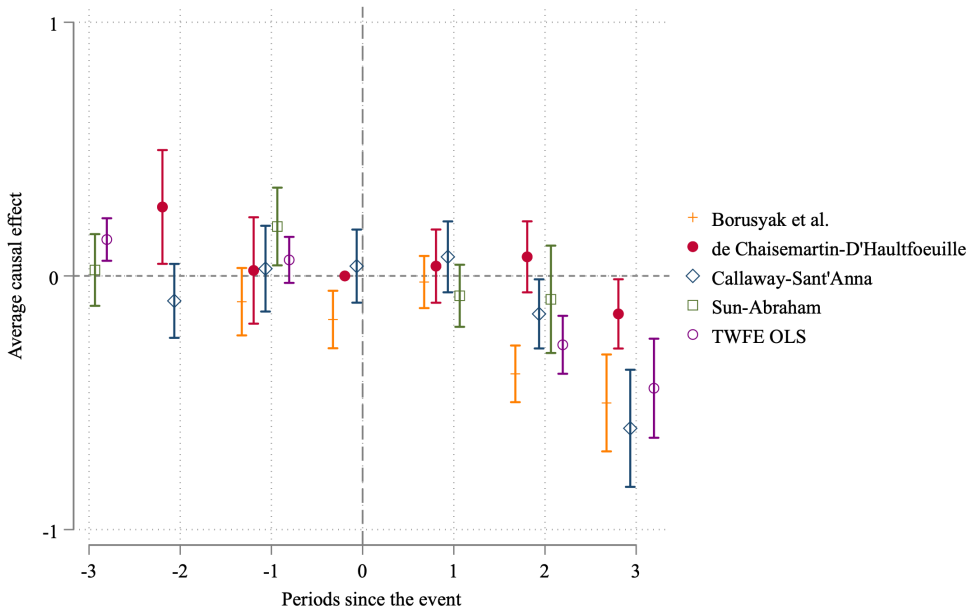

Figure 3.10: Event Study Estimators Comparisons

Notably, there is some divergence in the estimates provided by different methods, particularly in the post-treatment period. The traditional TWFE OLS approach shows a less pronounced negative effect compared to the other methods. This aligns with recent literature highlighting potential biases in TWFE estimates when treatment effects are heterogeneous across groups or time periods (Goodman-Bacon, 2021). In contrast, the method proposed by Borusyak et al. (2024) yields the largest negative effects, especially in periods 2 and 3 (2018-2020). This method may be capturing the full extent of the policy's impact, accounting for both the 2014 selective two-child policy and the 2016 universal two-child policy. The methods proposed by de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), and Sun and Abraham (2021) provide estimates that generally fall between those of TWFE and Borusyak et al. (2024). These newer approaches likely offer more accurate estimates than TWFE by accounting for heterogeneity and dynamic effects, which are particularly relevant given the staggered nature of the policy implementation in 2014 and 2016.

Overall, these results strongly suggest that the relaxation of the one-child policy had a negative impact on women's wages in China, an effect that appears to have intensified over time. The stronger effect in later periods might reflect both the cumulative impact of the 2014 policy change and the additional effect of the 2016 universal two-child policy. These findings highlight the importance of considering long-term and potentially heterogeneous effects when evaluating family planning policies and their impact on labour market outcomes for

women.

3.8 Conclusion

This study provides empirical evidence on the unintended consequences of China's Two-Child Policy on women's labour market outcomes. Employing a difference-in-differences (DID) framework and data from the China Family Panel Studies (CFPS), my findings reveal significant wage penalties and employment effects for women eligible to have additional children under the new policy. The primary results indicate that women eligible for a second child under the Two-Child Policy experienced a substantial wage penalty compared to their ineligible counterparts. This effect persisted even when controlling for actual fertility behaviour, suggesting that the mere possibility of increased fertility potential led to labour market discrimination. The wage penalty was particularly pronounced for women with one child, who appeared to be the most targeted group from the policy's perspective. Furthermore, my analysis of employment effects revealed a reversal in the relationship between eligibility and employment probability following the policy implementation. While eligibility was associated with higher employment probability before 2014, this relationship inverted post-policy, indicating potential statistical discrimination by employers and shifts in women's labour market decisions.

My study also uncovered significant heterogeneity in the policy's impact across different demographic and occupational subgroups. Women aged 30-40 experienced the most substantial negative effects, aligning with lifecycle theories of labour supply. Educational attainment appeared to moderate the policy's impact, with college-educated women experiencing positive effects, contrary to the overall trend. Occupational heterogeneity was also evident, with production workers and transport equipment operators facing the largest negative effects, while office workers and leading cadres showed positive, albeit variable, outcomes. Importantly, my analysis of urban-rural differences revealed that rural women bore the brunt of the policy's adverse effects on labour market outcomes. This finding can be attributed to several interrelated factors, including stronger adherence to traditional gender norms in rural areas, lower education levels among rural residents, and the relative absence of formal childcare facilities in rural regions. The lack of accessible, affordable childcare in rural areas may force more women to choose between work and family responsibilities, further widening the urban-rural gap in labour market outcomes following the implementation of the Two-Child

Policy.

These findings have important implications for policymakers seeking to balance demographic objectives with gender equality in the labour market. The unintended consequences of the Two-Child Policy highlight the need for complementary measures to support women's labour market participation and mitigate potential discrimination. Such measures could include strengthened anti-discrimination laws, improved childcare infrastructure, and policies to encourage more equitable distribution of domestic responsibilities. The observed urban-rural disparities underscore the importance of targeted interventions to address the disproportionate effects of the Two-Child Policy on rural women's labour market outcomes. Several limitations of this study should be acknowledged. First, while the DID approach allows for causal inference, it relies on the parallel trends assumption, which may not hold perfectly in all cases. Second, the study's reliance on survey data may introduce some measurement error, particularly in self-reported income figures. Finally, the focus on short-term effects may not capture the full long-term implications of the policy change.

It is noteworthy that since this study was conducted, China has further relaxed its population policy, introducing a three-child policy in 2021. This development underscores the urgency with which Chinese policymakers view the country's demographic challenges, including an ageing population and declining birth rates. However, the findings of this study serve as an important reminder that as we address these pressing population issues, it is crucial to simultaneously safeguard and advance women's rights and economic opportunities. The negative labour market impacts observed following the implementation of the Two-Child Policy highlight the potential for unintended consequences when population policies are not accompanied by robust measures to protect women's status in the workforce.

In conclusion, this study contributes to the growing body of literature on the economic implications of fertility policies by providing robust evidence on the labour market consequences of China's Two-Child Policy. The findings underscore the complex interplay between population policies, gender dynamics, and labour market outcomes, highlighting the need for policy approaches that consider both demographic objectives and economic equality. By illuminating the heterogeneous effects across various demographic and socioeconomic dimensions, this research provides a perspective on how pronatalist policies can inadvertently exacerbate existing inequalities in the labour market. As China and other countries continue to deal with demographic challenges, these insights can inform more targeted and effective

policy interventions. Future population policies must not only address demographic concerns but also actively work to mitigate potential negative impacts on women's economic opportunities and outcomes, ensuring that efforts to boost fertility do not come at the cost of gender equality in the labour market.

Conclusion

This thesis has examined how gender mediates the relationship between individuals and public policy across three domains: local government spending, domestic abuse, and labour markets. Drawing on data from England and China, and employing a range of identification strategies, the three chapters demonstrate that gendered outcomes depend critically on how political power is distributed, how social problems are recorded, and how policies interact with local economic conditions. This concluding chapter draws together the main findings, highlights the themes that connect them, and identifies limitations and directions for future research.

The first chapter established that female political representation causally shifts local government spending priorities in England. Exploiting quasi-random variation from alphabetical ballot ordering, the analysis showed that a ten percentage point increase in female councillors raises children's social care spending by approximately 1.43 percentage points – a 20 per cent increase relative to baseline – with offsetting reductions in infrastructure spending. These effects are concentrated in councils where women hold at least 30 per cent of seats, consistent with the critical mass hypothesis. The chapter also introduced a novel instrumental variable strategy based on ballot ordering that provides the methodological foundation for the subsequent chapter.

The second chapter extended this framework to domestic abuse, revealing a gap between what administrative data capture and what individuals experience. Areas with higher female representation record more domestic abuse cases, yet confidential survey data show no corresponding increase – and possibly a decrease – in prevalence. This divergence suggests that increased recording reflects changes in reporting behaviour rather than worsening conditions, plausibly driven by victim-initiated contact with the police and higher local spending on public health services. The presence of specialist domestic abuse partnerships further moderates this relationship, underscoring that institutional infrastructure shapes whether political representation translates into the visibility of abuse. The instrumental variable analysis, while

directionally consistent, proved inconclusive due to limited statistical power, leaving fully causal identification as a task for future work.

The third chapter turned to a different policy context but a complementary question: how policies designed to address one objective can generate unintended gendered consequences in another domain. Using longitudinal data and a difference-in-differences design, the analysis showed that Chinese women newly eligible for a second child under the Two-Child Policy experienced significant wage penalties and reduced employment probability – effects that persisted even after controlling for actual fertility behaviour, consistent with statistical discrimination by employers. As in the English chapters, the impact of a uniform national policy proved highly contingent on institutional context: adverse effects were concentrated among women aged 30-40, less-educated women, rural women, and those in production-oriented occupations, while college-educated women and professionals fared better. In both England and China, the findings reveal a common pattern: whether the mechanism is political representation or fertility regulation, gendered policy effects are amplified or attenuated by the local availability of services, the strength of prevailing gender norms, and the structure of labour markets.

Three themes connect these chapters. The first is the importance of women's political voice in shaping public policy. The first two chapters provide complementary evidence that descriptive representation translates into substantive policy change: women in local government redirect spending toward care-related services and are associated with environments in which domestic abuse becomes more visible through enhanced reporting and service provision. The third chapter, though set in a different institutional context, reinforces this theme by demonstrating what happens when policy is designed without adequate attention to gendered consequences – the Two-Child Policy's adverse labour market effects highlight the cost of excluding women's perspectives from policy design. Together, these findings make a case not only that who holds political office matters for what governments do, but that the absence of women's voice in policy processes can leave gendered harms unaddressed.

The second theme is the value of careful data construction in enabling credible analysis of gendered outcomes. The first chapter assembled a novel panel dataset linking local election results with detailed financial accounts. The second chapter showed that relying on police-recorded data alone would produce misleading conclusions, and that only by combining administrative records with confidential survey evidence could reporting behaviour be distin-

guished from changes in prevalence. The third chapter drew on longitudinal household data to track individual women through a national policy reform, capturing labour market effects that cross-sectional data would obscure. Across all three chapters, the empirical contributions depend as much on the data infrastructure as on the identification strategies employed.

The third theme is the role of institutional context in mediating policy effects. Whether it is the critical mass threshold in English local councils, the moderating role of specialist partnerships in the domestic abuse setting, or the urban-rural and occupational heterogeneity in China's labour market, the findings consistently show that the consequences of policy and representation are shaped by the environment in which they operate. This has a direct implication: uniform policies will produce unequal outcomes unless they are accompanied by measures that account for variation in local conditions.

Several limitations should be acknowledged. In the first two chapters, data constraints on English local elections mean that the ballot-order instrumental variable cannot be constructed for every local authority, restricting the sample available for causal analysis. In the second chapter, this restriction is particularly consequential: the reduced sample lacks the statistical power needed to establish causality, and resolving this remains an important challenge. The third chapter relies on the parallel trends assumption inherent in difference-in-differences designs, which, while supported by pre-trend analysis, may not hold perfectly across all subgroups. All three chapters focus on relatively short-term effects, and longer-run dynamics remain open questions.

These limitations point toward directions for future research. The ballot-order instrument could be applied in other electoral contexts, and improvements in the availability of local election data in England would strengthen the scope for causal analysis. The critical mass finding warrants investigation across different levels of government and political systems. The relationship between political representation and domestic abuse reporting would benefit from larger samples or alternative sources of exogenous variation, as well as deeper investigation into how local services and gender norms mediate reporting behaviour. The labour market effects of fertility policy could be examined over a longer time horizon, particularly following China's introduction of the three-child policy in 2021. More broadly, comparative work examining how different countries' approaches to representation, public safety, and fertility policy interact to shape women's outcomes would extend the insights developed here.

These findings carry concrete implications for policy. Chapter 1 demonstrates that female and male politicians allocate public resources differently, with women directing spending toward care-related services such as children's social care. This suggests that beyond efforts to increase women's political representation – important as they are – there is a practical case for embedding gender-aware analysis directly into budgetary processes, ensuring that spending on care, protection, and public health receives adequate scrutiny regardless of the gender composition of any particular council. The critical mass finding reinforces this point: where women's representation falls below a threshold, their policy priorities may be systematically underweighted, making procedural safeguards all the more necessary. The reporting mechanism identified in Chapter 2 implies that increases in recorded domestic abuse should not automatically be interpreted as policy failure; instead, policymakers should invest in the institutional infrastructure – specialist partnerships, public health services, and accessible reporting channels – that enables victims to come forward. Chapter 3 shows that pronatalist policies require complementary measures, including strengthened anti-discrimination enforcement and expanded childcare provision, to protect women's economic position. Across all three settings, the evidence points to a common lesson: policies that do not explicitly account for gendered consequences risk deepening the inequalities they aim to address.

Appendix A

Appendix for Chapter 1

Figure A.1 reproduces the official sample ballot paper for local council elections as prescribed in The Local Elections (Principal Areas) (England and Wales) Rules 2006.[1] The template sets out the standardised format for electing councillors to principal local authorities in England and Wales. Candidates are listed in strict alphabetical order by surname, with their name, address, and party affiliation (or "Independent") displayed in a uniform format. This statutory design ensures that ballot position is determined entirely by surname spelling, providing the exogenous variation that forms the basis of our identification strategy.

[1]The full ballot paper format and regulations are available at: https://www.legislation.gov.uk/uksi/2006/3304/schedule/2

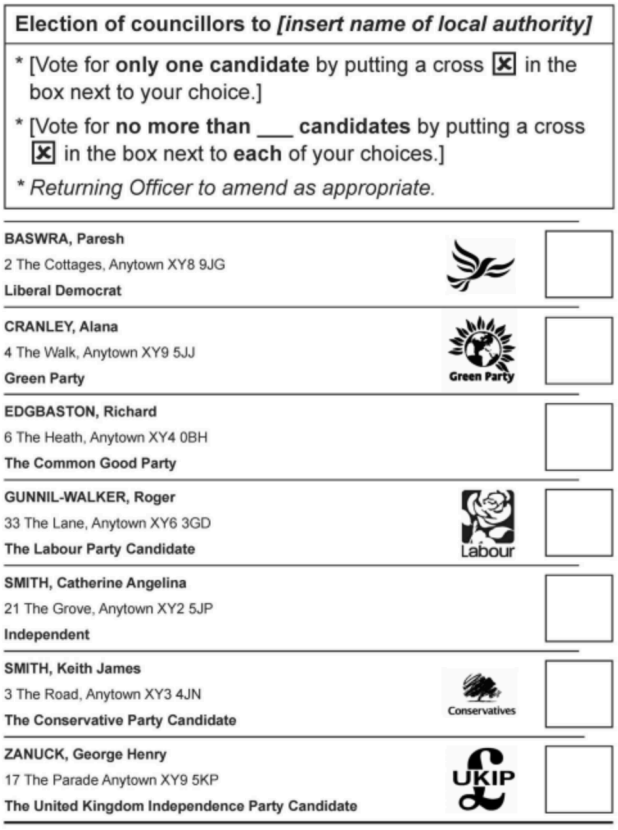

Figure A.1: Sample Ballot Paper for English Local Council Elections

Notes: Ballot paper format from The Local Elections (Principal Areas) (England and Wales) Rules 2006, Schedule 2.

Table A.1: Summary Statistics for Instrumental Variables

	Mean	Std Dev	Min	Max	N
Panel A: Ward-Aggregated IVs (Unweighted Average)					
Advantageous positions (Main)	0.333	0.076	0.088	0.593	585
Top 1 position only	0.332	0.101	0.000	0.647	585
Top 2 positions	0.328	0.075	0.059	0.611	585
Top 3 positions	0.315	0.067	0.067	0.556	585
Panel B: Ward-Aggregated IVs (Seat-Weighted)					
Advantageous positions	0.333	0.072	0.111	0.580	585
Top 1 position only	0.331	0.106	0.000	0.646	585
Top 2 positions	0.329	0.079	0.031	0.611	585
Top 3 positions	0.319	0.067	0.021	0.553	585
Panel C: Ward-Aggregated IVs (Candidate-Weighted)					
Advantageous positions	0.333	0.073	0.098	0.586	585
Top 1 position only	0.332	0.107	0.000	0.642	585
Top 2 positions	0.330	0.080	0.023	0.604	585
Top 3 positions	0.323	0.066	0.015	0.549	585
Panel D: Ward-Aggregated IVs (Female-Candidate-Weighted)					
Advantageous positions	0.449	0.098	0.163	0.814	585
Top 1 position only	0.449	0.137	0.000	0.882	585
Top 2 positions	0.443	0.097	0.083	0.765	585
Top 3 positions	0.431	0.075	0.056	0.641	585
Panel E: Local Authority-Level IV					
LA-wide ranking	0.331	0.069	0.111	0.540	583

Notes: This table reports summary statistics for all instrumental variables used in the analysis. Each IV measures the share of advantageous ballot positions occupied by female candidates. Panels A-D present ward-aggregated measures using different weighting schemes and position thresholds. "Advantageous positions" defines the top N positions as advantageous, where N equals ward seats; other fixed thresholds use top 1, 2, or 3 positions regardless of seats. Panel E presents the LA-level instrument constructed by ranking all candidates across the local authority. Sample comprises 211 local authorities observed in election years, 2010-2019.

Table A.2: Balance Tests: Alphabetical Advantage and Pre-Determined Characteristics

Variable	(1) Low Alphabetical Advantage	(2) High Alphabetical Advantage	(3) Difference
Log of total service expenditure	10.91	11.07	0.16
	(1.84)	(1.78)	(0.30)
Council size (# of councillors)	52.37	52.06	-0.31
	(15.79)	(14.72)	(0.74)
Population (log 1000s)	5.11	5.09	-0.02
	(0.87)	(0.67)	(0.79)
Conservative control	0.61	0.51	-0.09***
	(0.49)	(0.50)	(0.00)
Labour control	0.16	0.21	0.05**
	(0.37)	(0.41)	(0.04)
Observations	522	523	

Notes: This table compares local authorities with low and high alphabetical advantage, defined as below and above the median share of female candidates in advantageous ballot positions. Standard errors in parentheses. While some differences exist in political party composition, these are controlled for through party fixed effects in all regression models. Key covariates including expenditure, council size, and population show no significant differences between groups. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.3: OLS Estimates of the Relationship Between Female Representation and Spending in Election Years

	(1) Children Care	(2) Adult Care	(3) Public Health	(4) Education
% Female Cllr	0.028*	0.011	0.013	-0.064
	(0.016)	(0.023)	(0.009)	(0.043)
Observations	541	534	407	573
R-squared	0.884	0.923	0.834	0.947
	(5) Highways & Transport	(6) Cultural	(7) Environmental	(8) Planning Services
% Female Cllr	-0.020	-0.017	-0.076	-0.018
	(0.020)	(0.040)	(0.051)	(0.038)
Observations	315	568	573	555
R-squared	0.530	0.632	0.868	0.671

Notes: This table reports OLS estimates of the effect of female councillor representation on spending, using a subsample from election years. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4: Female Representation and Care-Related Subcategories

	Children Care			*Adult Care*		
	early	fam_sup	ythjust	phy_18_64	phy_65plus	carer_sup
%FemCllr	0.00784	0.0189	-0.00379	0.00137	0.0211	0.00325
	(0.00829)	(0.0183)	(0.00473)	(0.0106)	(0.0286)	(0.00221)
Obs	373	507	539	376	374	371
1st-stage F	39.25	40.23	41.09	38.33	37.64	38.07
	Public Health					
	sex_con	nhs	protla	chd_prog	obes_adt	obes_chd
%FemCllr	-0.000134	0.000750	-0.000515	0.000351	0.00310	0.00193
	(0.00173)	(0.000569)	(0.00114)	(0.000503)	(0.00225)	(0.00135)
Obs	410	410	410	410	408	409
1st-stage F	37.85	37.85	37.85	37.85	35.91	36.85
	Public Health					
	subs_drug	subs_alcohol	smok_cess	chd_5_19	pub_mental	misc
%FemCllr	0.0106**	0.00280	0.000614	-0.00173	-0.00193	0.0150
	(0.00528)	(0.00308)	(0.00112)	(0.00330)	(0.00177)	(0.0147)
Obs	410	409	410	409	211	407
1st-stage F	37.85	37.94	37.85	38.77	28.81	35.55

Notes: This table reports 2SLS estimates of the effect of female councillor representation on the share of total net current expenditure (NCE) allocated to disaggregated care-related subcategories. Subcategory definitions follow the General Fund Revenue Account Outturn guidance. Abbreviations are as follows: *early* refers to Sure Start; *fam_sup* to family support services; *ythjust* to youth justice (children); *phy_18_64* and *phy_65plus* to physical support for adults aged 18-64 and 65+, respectively; *adt_carer_sup* to support for adult carers; *sex_con* to sexual health (contraception); *nhs* to the NHS health check programme; *protla* to health protection; *chd_prog* to the National Child Measurement Programme; *obes_adt* and *obes_chd* to adult and child obesity programmes; *subs_drug* and *subs_alcohol* to drug and alcohol misuse treatment; *smok_cess* to smoking cessation services; *chd_5_19* to the public health programme for children aged 5-19; *pub_mental* to public mental health; and misc to miscellaneous public health services. The models use the same specification as in the main analysis. The instrument is the female share in advantageous alphabetical ballot positions. All models include controls for log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. $^{***}\ p < 0.01$, $^{**}\ p < 0.05$, $^{*}\ p < 0.10$.

Table A.5: Robustness to Alternative Critical Mass Thresholds

	Children Care		Public Health		Highways and Transport		Environmental	
	(1) Above Threshold	(2) Below Threshold	(3) Above Threshold	(4) Below Threshold	(5) Above Threshold	(6) Below Threshold	(7) Above Threshold	(8) Below Threshold
Threshold 20%								
%FemCllr	0.189***	-0.0067	0.0670**	0.00210	-0.181***	-0.0409	-0.332*	0.256
	(0.0694)	(0.0324)	(0.0330)	(0.00795)	(0.0606)	(0.0301)	(0.185)	(0.183)
Obs	494	47	372	35	291	24	526	47
R^2	0.860	0.985	0.810	0.993	0.316	0.886	0.855	0.943
1st-stage F	31.55	6.15	32.72	8.85	17.81	10.32	33.75	6.15
Threshold 25%								
%FemCllr	0.214**	0.0322	0.0585	0.0144	-0.0854*	-0.127**	-0.368*	0.0600
	(0.0838)	(0.0496)	(0.0357)	(0.0168)	(0.0500)	(0.0497)	(0.217)	(0.340)
Obs	429	112	320	87	248	67	460	113
R^2	0.860	0.940	0.821	0.948	0.471	0.770	0.858	0.895
1st-stage F	23.36	10.38	27.26	10.08	14.64	7.14	24.91	10.28
Threshold 28%								
%FemCllr	0.221**	0.0952	0.0754*	-0.0059	-0.0826	-0.227**	-0.0979	-0.552*
	(0.0897)	(0.0787)	(0.0433)	(0.0214)	(0.0551)	(0.104)	(0.261)	(0.319)
Obs	341	200	258	149	201	114	370	203
R^2	0.866	0.895	0.796	0.915	0.443	0.421	0.872	0.870
1st-stage F	15.01	18.34	19.97	15.62	11.28	5.27	15.89	18.53
Threshold 29%								
%FemCllr	0.205**	0.148	0.0562	0.0120	-0.0436	-0.242	0.0261	-0.493
	(0.0953)	(0.0917)	(0.0436)	(0.0263)	(0.0536)	(0.150)	(0.288)	(0.318)
Obs	297	244	224	183	169	146	323	250
R^2	0.891	0.864	0.844	0.870	0.473	0.255	0.883	0.861
1st-stage F	10.74	15.27	13.70	11.46	8.63	2.27	11.17	15.51
Threshold 30%								
%FemCllr	0.234**	0.135*	0.0629	0.0131	-0.0366	-0.163*	0.0489	-0.453
	(0.110)	(0.0781)	(0.0497)	(0.0224)	(0.0672)	(0.0856)	(0.319)	(0.278)
Obs	284	257	212	195	155	160	308	265
R^2	0.881	0.874	0.840	0.872	0.447	0.465	0.881	0.866
1st-stage F	8.97	18.04	10.87	14.46	5.97	4.44	9.46	18.26
Threshold 33%								
%FemCllr	0.267**	0.137**	0.153*	0.0104	-0.055	-0.159**	0.355	-0.440*
	(0.126)	(0.0689)	(0.0791)	(0.0183)	(0.0601)	(0.0736)	(0.301)	(0.229)
Obs	176	365	136	271	111	204	197	376
R^2	0.879	0.879	0.779	0.861	0.403	0.502	0.866	0.863
1st-stage F	7.51	23.23	6.15	24.15	8.27	6.72	8.43	24.31
Threshold 35%								
%FemCllr	0.269**	0.153**	0.188*	0.0207	0.0502	-0.173***	0.207	-0.408*
	(0.131)	(0.0652)	(0.110)	(0.0176)	(0.0712)	(0.0660)	(0.286)	(0.224)
Obs	137	404	106	301	78	237	153	420
R^2	0.887	0.875	0.744	0.858	0.637	0.439	0.896	0.857
1st-stage F	5.10	28.11	3.12	29.47	3.61	9.89	5.76	28.61

Notes: Each panel reports 2SLS estimates of the effect of female councillor representation on spending as a percentage of total service expenditure. Councils are classified as "above-threshold" or "below-threshold" depending on whether their baseline female representation exceeds the indicated cutoff. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.6: Timing Dynamics of Female Representation Effects on Spending

	Year After (t+1)		Year Before (t-1)	
	(1) Female above 28%	(2) Female below 28%	(3) Female above 28%	(4) Female above 28%
Panel A: Children Care				
% Female Cllr	0.238**	-0.100	0.0847	0.143*
	(0.0973)	(0.125)	(0.0535)	(0.0851)
Observations	284	149	255	150
First-stage F-stat	8.52	9.09	20.29	15.58
Panel B: Highways and Transport				
% Female Cllr	-0.121	-0.344*	-0.0673	-0.0572
	(0.0754)	(0.197)	(0.157)	(0.121)
Observations	165	92	204	132
First-stage F-stat	5.64	3.62	4.70	3.77

Notes: The table presents IV estimates examining temporal relationships between female political representation and spending outcomes. Year after election effects (t+1) columns show effects of current female representation on next-period spending, which capture policy implementation lags. Year before election effects (t-1) columns present effects on previous-period spending as a placebo test. Columns (1) and (3) restrict to female-friendly councils, while columns (2) and (4) focus on male-dominated councils. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7: Robustness of 2SLS Estimates to Alternative IV Constructions (Children's Social Care)

	(1) AlphaAdv	(2) Top 1	(3) Top 2	(4) Top3
Panel A: Ward-Aggregated IVs (Unweighted Average)				
% Female Cllr	0.143***	0.205**	0.194***	0.130***
	(0.0536)	(0.0860)	(0.0573)	(0.0458)
Observations	541	541	541	541
First-stage F-stat	40.88	22.13	51.18	67.84
Panel B: Ward-Aggregated IVs (Seat-Weighted)				
% Female Cllr	0.138***	0.192**	0.203***	0.138***
	(0.0480)	(0.0763)	(0.0582)	(0.0448)
Observations	541	541	541	541
First-stage F-stat	47.65	24.10	48.60	68.80
Panel C: Ward-Aggregated IVs (Candidate-Weighted)				
% Female Cllr	0.160***	0.205**	0.220***	0.153***
	(0.0537)	(0.0828)	(0.0631)	(0.0488)
Observations	541	541	541	541
First-stage F-stat	39.09	21.64	41.40	59.28
Panel D: Ward-Aggregated IVs (Female-Candidate-Weighted)				
% Female Cllr	0.274**	0.330*	0.330*	0.277**
	(0.130)	(0.188)	(0.188)	(0.109)
Observations	541	541	541	541
First-stage F-stat	7.37	4.87	4.87	13.10
Panel E: Local Authority-Level IV				
% Female Cllr	0.132***			
	(0.0486)			
Observations	539			
First-stage F-stat	41.32			

Notes: This table reports 2SLS estimates of the effect of female councillor representation on local government spending for children's social care. The specifications vary the construction of the instrumental variable. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of the largest party, and fixed effects for year, region, and political party control. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8: Robustness of 2SLS Estimates to Alternative IV Constructions (Public Health)

	(1) AlphaAdv	(2) Top 1	(3) Top 2	(4) Top3
Panel A: Ward-Aggregated IVs (Unweighted Average)				
% Female Cllr	0.0548**	0.112***	0.0779***	0.0681**
	(0.0279)	(0.0425)	(0.0282)	(0.0281)
Observations	407	407	407	407
First-stage F-stat	37.73	21.34	45.65	55.55
Panel B: Ward-Aggregated IVs (Seat-Weighted)				
% Female Cllr	0.0568**	0.113***	0.0733***	0.0629**
	(0.0266)	(0.0412)	(0.0269)	(0.0266)
Observations	407	407	407	407
First-stage F-stat	44.04	21.81	45.38	57.25
Panel C: Ward-Aggregated IVs (Candidate-Weighted)				
% Female Cllr	0.0662**	0.120***	0.0790***	0.0684**
	(0.0290)	(0.0435)	(0.0280)	(0.0277)
Observations	407	407	407	407
First-stage F-stat	35.47	19.06	40.09	48.57
Panel D: Ward-Aggregated IVs (Female-Candidate-Weighted)				
% Female Cllr	0.0772	0.162*	0.162*	0.0939
	(0.0644)	(0.0971)	(0.0971)	(0.0591)
Observations	407	407	407	407
First-stage F-stat	5.47	4.35	4.35	9.32
Panel E: Local Authority-Level IV				
% Female Cllr	0.0397			
	(0.0271)			
Observations	405			
First-stage F-stat	39.57			

Notes: This table reports 2SLS estimates of the effect of female councillor representation on local government spending for public health. The specifications vary the construction of the instrumental variable. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of the largest party, and fixed effects for year, region, and political party control. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9: Robustness of 2SLS Estimates to Alternative IV Constructions (Highways and Transports)

	(1) AlphaAdv	(2) Top 1	(3) Top 2	(4) Top3
Panel A: Ward-Aggregated IVs (Unweighted Average)				
% Female Cllr	-0.157***	-0.179***	-0.133***	-0.109***
	(0.0554)	(0.0661)	(0.0489)	(0.0330)
Observations	315	315	315	315
First-stage F-stat	17.69	10.79	23.69	33.09
Panel B: Ward-Aggregated IVs (Seat-Weighted)				
% Female Cllr	-0.162***	-0.171**	-0.134**	-0.111***
	(0.0522)	(0.0682)	(0.0535)	(0.0340)
Observations	315	315	315	315
First-stage F-stat	17.61	9.26	18.50	25.66
Panel C: Ward-Aggregated IVs (Candidate-Weighted)				
% Female Cllr	-0.168***	-0.143**	-0.128**	-0.102***
	(0.0602)	(0.0672)	(0.0589)	(0.0361)
Observations	315	315	315	315
First-stage F-stat	12.79	8.50	14.89	20.28
Panel D: Ward-Aggregated IVs (Female-Candidate-Weighted)				
% Female Cllr	-0.487	-0.307	-0.307	-0.138
	(0.529)	(0.236)	(0.236)	(0.133)
Observations	315	315	315	315
First-stage F-stat	0.66	1.37	1.37	2.97
Panel E: Local Authority-Level IV				
% Female Cllr	-0.126*			
	(0.0703)			
Observations	315			
First-stage F-stat	10.88			

Notes: This table reports 2SLS estimates of the effect of female councillor representation on local government spending for highways and transport services. The specifications vary the construction of the instrumental variable. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of the largest party, and fixed effects for year, region, and political party control. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10: Robustness of 2SLS Estimates to Alternative IV Constructions (Environmental)

	(1) AlphaAdv	(2) Top 1	(3) Top 2	(4) Top3
Panel A: Ward-Aggregated IVs (Unweighted Average)				
% Female Cllr	-0.302*	-0.338	-0.283*	-0.253*
	(0.169)	(0.223)	(0.162)	(0.135)
Observations	573	573	573	573
First-stage F-stat	43.32	24.46	52.55	70.46
Panel B: Ward-Aggregated IVs (Seat-Weighted)				
% Female Cllr	-0.255	-0.303	-0.282*	-0.252*
	(0.158)	(0.214)	(0.165)	(0.136)
Observations	573	573	573	573
First-stage F-stat	49.99	26.30	49.82	71.24
Panel C: Ward-Aggregated IVs (Candidate-Weighted)				
% Female Cllr	-0.275	-0.319	-0.287	-0.273*
	(0.170)	(0.219)	(0.174)	(0.141)
Observations	573	573	573	573
First-stage F-stat	40.78	23.53	42.47	60.76
Panel D: Ward-Aggregated IVs (Female-Candidate-Weighted)				
% Female Cllr	-0.0849	-0.210	-0.210	-0.211
	(0.331)	(0.408)	(0.408)	(0.257)
Observations	573	573	573	573
First-stage F-stat	7.69	5.73	5.73	13.38
Panel E: Local Authority-Level IV				
% Female Cllr	-0.114			
	(0.165)			
Observations	571			
First-stage F-stat	43.00			

Notes: This table reports 2SLS estimates of the effect of female councillor representation on local government spending for environmental services. The specifications vary the construction of the instrumental variable. Controls include log total expenditure, total elected councillors, log population, sex ratio, seat share of the largest party, and fixed effects for year, region, and political party control. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.11: Heterogeneous Effects by Council Size

	Children Care		Public Health		Highways and Transport		Environmental	
	(1) Below Cutoff	(2) Above Cutoff	(3) Below Cutoff	(4) Above Cutoff	(5) Below Cutoff	(6) Above Cutoff	(7) Below Cutoff	(8) Above Cutoff
Cutoff at 40 Councillors								
% FemCllr	0.0807***	0.215***	0.00955	0.0962**	-0.0465	-0.144***	-0.452	-0.227
	(0.0313)	(0.0814)	(0.0200)	(0.0444)	(0.0487)	(0.0510)	(0.277)	(0.190)
Obs	144	397	105	302	53	262	144	429
R^2	0.844	0.838	0.738	0.764	0.612	0.438	0.593	0.889
1st-stage F	16.09	23.75	12.74	26.00	15.73	11.69	16.09	25.95
Cutoff at 45 Councillors								
% FemCllr	0.0732*	0.240**	-0.00732	0.112**	-0.0793	-0.167***	-0.487**	-0.138
	(0.0428)	(0.0963)	(0.0171)	(0.0531)	(0.0613)	(0.0534)	(0.248)	(0.217)
Obs	212	329	151	256	84	231	212	361
R^2	0.858	0.838	0.810	0.767	0.497	0.402	0.685	0.894
1st-stage F	27.89	16.65	21.84	19.12	7.26	11.09	27.89	18.84
Cutoff at 50 Councillors								
% FemCllr	0.177**	0.148**	0.0247	0.0835**	-0.0637	-0.162***	-0.365	-0.311
	(0.0706)	(0.0755)	(0.0279)	(0.0401)	(0.0647)	(0.0566)	(0.233)	(0.197)
Obs	302	239	215	192	123	192	305	268
R^2	0.843	0.850	0.855	0.758	0.539	0.389	0.743	0.895
1st-stage F	25.02	21.56	20.61	22.15	9.21	11.80	25.75	22.98
Cutoff at 55 Councillors								
% FemCllr	0.145**	0.162	0.0352	0.106*	-0.148**	-0.113	-0.328*	-0.135
	(0.0598)	(0.105)	(0.0308)	(0.0545)	(0.0580)	(0.0817)	(0.196)	(0.209)
Obs	378	163	270	137	176	139	390	183
R^2	0.875	0.841	0.850	0.748	0.307	0.488	0.798	0.933
1st-stage F	31.13	19.58	28.61	13.88	13.83	5.72	32.90	18.50
Cutoff at 60 Councillors								
% FemCllr	0.123**	0.121	0.0459*	-0.0327	-0.159***	-0.0806	-0.293	0.134
	(0.0548)	(0.135)	(0.0269)	(0.0456)	(0.0616)	(0.0599)	(0.181)	(0.125)
Obs	447	94	320	87	216	99	468	105
R^2	0.882	0.721	0.846	0.736	0.280	0.623	0.826	0.892
1st-stage F	35.33	6.29	31.71	8.27	12.88	6.66	37.13	6.58

Notes: Each panel reports 2SLS estimates using different cutoffs for council size, measured as the total number of elected councillors. "Below Cutoff" indicates councils with size less than or equal to the cutoff, while "Above Cutoff" indicates councils with size greater than the cutoff. Dependent variables are spending categories as a share of total net current expenditure. Controls include log total expenditure, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.12: Female Representation and Local Government Spending (Log Levels)

	(1) Total Services	(2) Children Social Care	(3) Adult Social Care	(4) Public Health	(5) Education
Panel A: OLS					
% Female Cllr	-0.009	0.002	0.003	0.009*	-0.001
	(0.008)	(0.004)	(0.005)	(0.005)	(0.002)
Obs	1997	908	966	738	944
R^2	0.854	0.948	0.945	0.924	0.955
Panel B: 2SLS					
% Female Cllr	-0.007	0.040	0.015	0.025*	-0.007
	(0.022)	(0.029)	(0.035)	(0.014)	(0.007)
Obs	573	239	258	214	253
R^2	0.854	0.937	0.938	0.944	0.950
First-stage F-stat	43.56	5.88	8.42	15.26	9.90
	(6) Highways & Transport	(7) Cultural	(8) Envir	(9) Planning Services	
Panel A: OLS					
% Female Cllr	-0.015**	0.001	-0.000	0.006	
	(0.006)	(0.004)	(0.002)	(0.004)	
Obs	1187	1984	1997	1937	
R^2	0.907	0.825	0.926	0.582	
Panel B: 2SLS					
% Female Cllr	-0.061**	-0.001	-0.003	0.003	
	(0.025)	(0.013)	(0.007)	(0.012)	
Obs	313	568	573	555	
R^2	0.905	0.818	0.923	0.628	
First-stage F-stat	16.73	43.23	43.32	41.14	

Notes: This table presents OLS and 2SLS estimates for the effect of female councillor representation on local government spending levels. The dependent variables are the log of spending levels (in £1,000s) for each category, rather than spending shares as in the main specification. This tests whether female representation affects absolute spending levels. All specifications include log total expenditure, total elected councillors, log population, sex ratio, seat share of largest party, and fixed effects for year, region, and political party control. Column 1 uses total services spending as the dependent variable and excludes log total expenditure from the controls; columns 2–8 control for log total expenditure to hold the overall budget constant. Standard errors clustered at local authority level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix B

Appendix for Chapter 2

B.1 Data Sources and Coverage

B.1.1 Local Authority Coverage and Data Availability

Table B.1 summarises the coverage of each local authority in the dataset, including electoral cycles, the availability of councillor and police-recorded domestic abuse data, and police force areas.

Table B.1: Council Characteristics, Electoral Cycles, and Domestic Abuse Data Availability

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
Barking and Dagenham	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Barnet	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Bexley	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Brent	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Bromley	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Camden	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Croydon	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Ealing	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Enfield	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Greenwich	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Hackney	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Hammersmith and Fulham	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Haringey	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Harrow	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Havering	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Hillingdon	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Hounslow	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Islington	Whole	2016-22	Metropolitan Police	2017-20	2015-17

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Table B.1 – continued from previous page

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
Kensington and Chelsea	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Kingston upon Thames	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Lambeth	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Lewisham	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Merton	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Newham	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Redbridge	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Richmond upon Thames	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Southwark	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Sutton	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Tower Hamlets	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Waltham Forest	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Wandsworth	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Westminster	Whole	2016-22	Metropolitan Police	2017-20	2015-17
Cumbria	Whole	2016-22	Cumbria Constabulary	2015-20	2015-20
Blackburn with Darwen	Thirds	2016-22	Lancashire Constabulary	2015-20	2015-20
Blackpool	Whole	2016-22	Lancashire Constabulary	2015-20	2015-20
Lancashire	Whole	2016-22	Lancashire Constabulary	2015-20	2015-20
Knowsley	Thirds	2016-22	Merseyside Police	2015-20	2015-20
Liverpool	Thirds	2016-22	Merseyside Police	2015-20	2015-20
St. Helens	Thirds	2016-22	Merseyside Police	2015-20	2015-20
Sefton	Thirds	2016-22	Merseyside Police	2015-20	2015-20
Wirral	Thirds	2016-22	Merseyside Police	2015-20	2015-20
Bolton	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Bury	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Manchester	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Oldham	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Rochdale	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20

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Table B.1 – continued from previous page

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
Salford	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Stockport	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Tameside	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Trafford	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Wigan	Thirds	2016-22	Greater Manchester Police	2019-20	2019-20
Halton	Thirds	2016-22	Cheshire Constabulary	N/A	2015-20
Warrington	Thirds	2016-22	Cheshire Constabulary	N/A	2015-20
Cheshire East	Whole	2016-22	Cheshire Constabulary	N/A	2015-20
Cheshire West and Chester	Whole	2016-22	Cheshire Constabulary	N/A	2015-20
Northumberland	Whole	2016-22	Northumbria Police	2017-20	2017-20
Newcastle upon Tyne	Thirds	2016-22	Northumbria Police	2017-20	2017-20
North Tyneside	Thirds	2016-22	Northumbria Police	2017-20	2017-20
South Tyneside	Thirds	2016-22	Northumbria Police	2017-20	2017-20
Sunderland	Thirds	2016-22	Northumbria Police	2017-20	2017-20
Gateshead	Thirds	2016-22	Northumbria Police	2017-20	2017-20
Darlington	Whole	2016-22	Durham Constabulary	2017-20	2017-20
County Durham	Whole	2016-22	Durham Constabulary	2017-20	2017-20
York	Whole	2016-22	North Yorkshire Police	2015-20	2015-20
North Yorkshire	Whole	2016-22	North Yorkshire Police	2015-20	2015-20
Bradford	Thirds	2016-22	West Yorkshire Police	2015-20	2015-20
Calderdale	Thirds	2016-22	West Yorkshire Police	2015-20	2015-20
Kirklees	Thirds	2016-22	West Yorkshire Police	2015-20	2015-20
Leeds	Thirds	2016-22	West Yorkshire Police	2015-20	2015-20
Wakefield	Thirds	2016-22	West Yorkshire Police	2015-20	2015-20
Barnsley	Thirds	2016-22	South Yorkshire Police	2019-20	2018-20
Doncaster	Whole	2016-22	South Yorkshire Police	2019-20	2018-20
Rotherham	Whole	2016-22	South Yorkshire Police	2019-20	2018-20
Sheffield	Thirds	2016-22	South Yorkshire Police	2019-20	2018-20
Kingston upon Hull, City of	Thirds	2016-22	Humberside Police	2015-20	2017-20
East Riding of Yorkshire	Whole	2016-22	Humberside Police	2015-20	2017-20

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Table B.1 – continued from previous page

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
North East Lincolnshire	Thirds	2016-22	Humberside Police	2015-20	2017-20
North Lincolnshire	Whole	2016-22	Humberside Police	2015-20	2017-20
Hartlepool	Thirds	2016-22	Cleveland Police	2015-19	2015-19
Middlesbrough	Whole	2016-22	Cleveland Police	2015-19	2015-19
Redcar and Cleveland	Whole	2016-22	Cleveland Police	2015-19	2015-19
Stockton-on-Tees	Whole	2016-22	Cleveland Police	2015-19	2015-19
Birmingham	Whole	2016-22	West Midlands Police	2015-20	2015-20
Coventry	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Dudley	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Sandwell	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Solihull	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Walsall	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Wolverhampton	Thirds	2016-22	West Midlands Police	2015-20	2015-20
Stoke-on-Trent	Whole	2016-22	Staffordshire Police	2015-20	2015-20
Staffordshire	Whole	2016-22	Staffordshire Police	2015-20	2015-20
Herefordshire, County of	Whole	2016-22	West Mercia Police	2018-20	2018-20
Telford and Wrekin	Whole	2016-22	West Mercia Police	2018-20	2018-20
Shropshire	Whole	2016-22	West Mercia Police	2018-20	2018-20
Worcestershire	Whole	2016-22	West Mercia Police	2018-20	2018-20
Warwickshire	Whole	2016-22	Warwickshire Police	2015-20	2015-20
Derby	Thirds	2016-22	Derbyshire Constabulary	2017-20	2017-20
Derbyshire	Whole	2016-22	Derbyshire Constabulary	2017-20	2017-20
Nottingham	Whole	2016-22	Nottinghamshire	2020	2016-20
Nottinghamshire	Whole	2016-22	Nottinghamshire	2020	2016-20
Lincolnshire	Whole	2016-22	Lincolnshire Police	2015-20	2015-20
Leicester	Whole	2016-22	Leicestershire Police	2015-20	2015-20
Rutland	Whole	2016-22	Leicestershire Police	2015-20	2015-20
Leicestershire	Whole	2016-22	Leicestershire Police	2015-20	2015-20
Northamptonshire	Whole	2016-22	Northamptonshire Police	2015-20	2015-20
Peterborough	Thirds	2016-22	Cambridgeshire Constabulary	2015-21	2015-21

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Table B.1 – continued from previous page

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
Cambridgeshire	Whole	2016-22	Cambridgeshire Constabulary	2015-21	2015-21
Norfolk	Whole	2016-22	Norfolk Constabulary	2015-20	2015-20
Suffolk	Whole	2016-22	Suffolk Constabulary	2015-20	2015-20
Luton	Whole	2016-22	Bedfordshire Police	2015-21	2015-21
Bedford	Whole	2016-22	Bedfordshire Police	2015-21	2015-21
Central Bedfordshire	Whole	2016-22	Bedfordshire Police	2015-21	2015-21
Hertfordshire	Whole	2016-22	Hertfordshire Constabulary	2015-20	2015-20
Southend-on-Sea	Thirds	2016-22	Essex Police	2015-20	2015-20
Thurrock	Thirds	2016-22	Essex Police	2015-20	2015-20
Essex	Whole	2016-22	Essex Police	2015-20	2015-20
Bracknell Forest	Whole	2016-22	Thames Valley Police	2015-20	2015-20
West Berkshire	Whole	2016-22	Thames Valley Police	2015-20	2015-20
Reading	Thirds	2016-22	Thames Valley Police	2015-20	2015-20
Slough	Thirds	2016-22	Thames Valley Police	2015-20	2015-20
Windsor and Maidenhead	Whole	2016-22	Thames Valley Police	2015-20	2015-20
Wokingham	Thirds	2016-22	Thames Valley Police	2015-20	2015-20
Milton Keynes	Thirds	2016-22	Thames Valley Police	2015-20	2015-20
Buckinghamshire	Whole	2016-22	Thames Valley Police	2015-20	2015-20
Oxfordshire	Whole	2016-22	Thames Valley Police	2015-20	2015-20
Portsmouth	Thirds	2016-22	Hampshire and Isle of Wight Constabulary	N/A	N/A
Southampton	Thirds	2016-22	Hampshire and Isle of Wight Constabulary	N/A	N/A
Isle of Wight	Whole	2016-22	Hampshire and Isle of Wight Constabulary	N/A	N/A
Hampshire	Whole	2016-22	Hampshire and Isle of Wight Constabulary	N/A	N/A
Surrey	Whole	2016-22	Surrey Police	2015-20	2015-20
Medway	Whole	2016-22	Kent Police	2015-20	2015-20
Kent	Whole	2016-22	Kent Police	2015-20	2015-20
Brighton and Hove	Whole	2016-22	Sussex Police	2017-20	2017-20
East Sussex	Whole	2016-22	Sussex Police	2017-20	2017-20
West Sussex	Whole	2016-22	Sussex Police	2017-20	2017-20

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Table B.1 – continued from previous page

Councils	Electoral cycles	Councillors' Info	Police Force (Area)	DA incidents	DA crimes
City of London	Whole	2016-22	City of London Police	2015-20	2015-20
Plymouth	Thirds	2016-22	Devon & Cornwall Police	2018-20	2015-20
Torbay	Whole	2016-22	Devon & Cornwall Police	2018-20	2015-20
Cornwall	Whole	2016-22	Devon & Cornwall Police	2018-20	2015-20
Isles of Scilly	Whole	2016-22	Devon & Cornwall Police	2018-20	2015-20
Devon	Whole	2016-22	Devon & Cornwall Police	2018-20	2015-20
Bath and North East Somerset	Whole	2016-22	Avon and Somerset Police	2015-20	2015-20
Bristol, City of	Thirds	2016-22	Avon and Somerset Police	2015-20	2015-20
North Somerset	Whole	2016-22	Avon and Somerset Police	2015-20	2015-20
South Gloucestershire	Whole	2016-22	Avon and Somerset Police	2015-20	2015-20
Somerset	Whole	2016-22	Avon and Somerset Police	2015-20	2015-20
Gloucestershire	Whole	2016-22	Gloucestershire Constabulary	2015-20	2015-20
Swindon	Thirds	2016-22	Wiltshire Police	2015-20	2015-20
Wiltshire	Whole	2016-22	Wiltshire Police	2015-20	2015-20
Bournemouth	Whole	2016-22	Dorset Police	2015-20	2015-20
Poole	Whole	2016-22	Dorset Police	2015-20	2015-20
Dorset	Whole	2016-22	Dorset Police	2015-20	2015-20

B.1.2 Political Party Dominance in Local Authorities

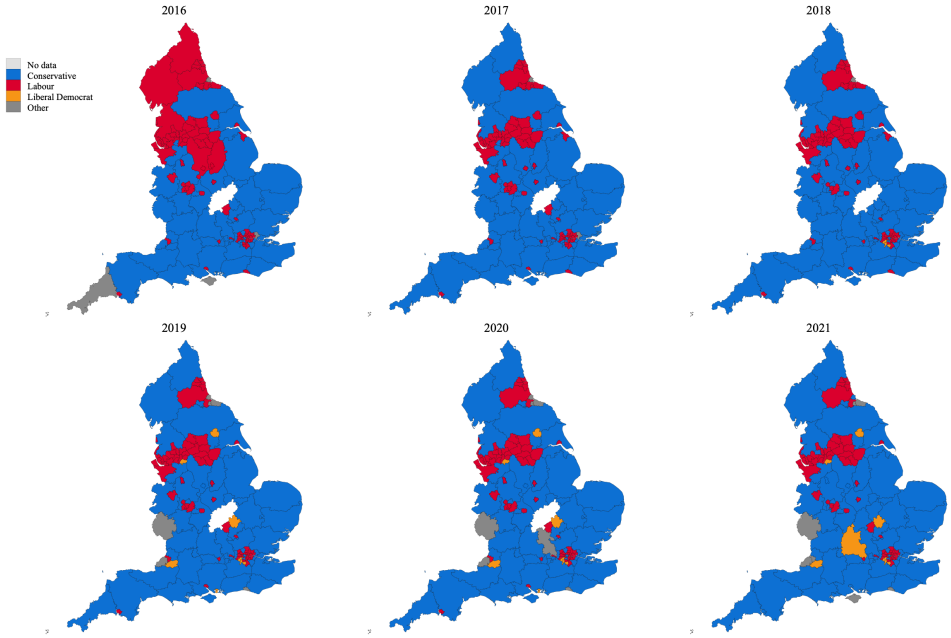

Figure B.1: Dominant Political Party by Local Authority, 2016–2021

Notes: Northamptonshire, shown as blank due to boundary changes, was predominantly Conservative during this period.

B.1.3 Freedom of Information Requests and Police Data Access

Table B.2 presents the responses from police forces to Freedom of Information (FOI) requests regarding domestic abuse-related incidents and crimes (2015/16–2020/21). We distinguish between incidents reported but not recorded as crimes and those recorded as crimes under the National Crime Recording Standard.

Table B.2: Responses to Freedom of Information (FOI) Requests on Domestic Abuse-Related Incidents and Crimes by Police Force

Police Force	DA incidents	DA crimes	Detailed
Metropolitan Police	2017-20	2015-17	N/A
Cumbria Constabulary	2015-20	2015-20	N/A
Lancashire Constabulary	2015-20	2015-20	Since 2015 both Domestic Abuse Incidents and Domestic Abuse Crimes are recordable on the crime system: Initially on the SLEUTH system up to the 28th November 2018 and from the 28th November 2018 on the CONNECT Investigations system. The force acknowledges a potential underreporting for the year 2018/2019 due to the system changeover, estimating an approximate shortfall of 100 records.
Merseyside Police	2015-20	2015-20	N/A
Greater Manchester Police	2019-20	2019-20	In July 2019, GMP implemented one of the largest IT infrastructure changes in UK policing. Greater Manchester Police have been unable to supply data for the second, third and fourth quarters of 2019/20, due to issues with the implementation of new IT systems. Therefore they have been excluded from the 2019/20 financial year.

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Table B.2 – continued from previous page

Police Force	DA incidents	DA crimes	Detailed
Cheshire Constabulary	N/A	2015-20	Cheshire Constabulary declined the request for information on domestic abuse incidents, citing the excessive manual effort required to cross-reference nearly 17,000 incidents in 2020-2021 alone against their crime recording system. They highlighted that fulfilling the request would far exceed the cost threshold as defined by Data Protection and Freedom of Information (Fees and Appropriate Limit) Regulations 2004. However, they provide data on domestic abuse crimes within the permissible cost limit.
Northumbria Police	2017-20	2017-20	Northumbria Police indicated that fulfilling the entire request would surpass the prescribed cost limit, thus exempting them from providing a complete response under the Act. Nevertheless, under Section 16 of the Freedom of Information Act 2000, they provide information from 2017/18 to 2020/21.
Durham Constabulary	2017-20	2017-20	Durham Constabulary was unable to provide data for the years 2015/16 and 2016/17 due to the information being held on a legacy system, which would require an effort exceeding the statutory time limit to retrieve. While the request was refused under Section 12 of the FOI Act, they provided some data for the years 2017/18 onwards as a gesture of goodwill.
North Yorkshire Police	2015-20	2015-20	N/A
West Yorkshire Police	2015-20	2015-20	N/A
South Yorkshire Police	2019-20	2018-20	South Yorkshire Police noted limitations in extracting historical data due to system changes, particularly before 2018. However, they were able to provide more recent data from 2018 to 2021, highlighting the use of the Connect System for crime recording and the Smart system for logging incidents, with data broken down by district areas.

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Table B.2 – continued from previous page

Police Force	DA incidents	DA crimes	Detailed
Humberside Police	2015-20	2017-20	Humberside Police provide domestic abuse-related crimes data from 06/06/2017 to 31/12/2021 for the stated BCUs as accurate data is unavailable before this date.
Cleveland Police	2015-19	2015-19	N/A
West Midlands Police	2015-20	2015-20	N/A
Staffordshire Police	2015-20	2015-20	N/A
West Mercia Police	2018-20	2018-20	West Mercia Police noted that to ensure the Force remains compliant with GDPR and MoPI regulations, certain MoPI 3 level data is automatically deleted from Force systems after a period of 5 years. As a result, the data within this FOI will only date back 5 years (as of 2023) to reduce confusion and the misinterpretation of data as records prior to 5 years will potentially not be complete or accurate.
Warwickshire Police	2015-20	2015-20	N/A
Derbyshire Constabulary	2017-20	2017-20	Derbyshire Constabulary noted the data has been extracted from the Constabulary's Crime System which is, to all intent a 'living' system in that it is constantly being updated. As such the data can only be verified for the date/time extracted from 2017.
Nottinghamshire	2020	2016-20	Nottinghamshire Police pointed out that with regard to incidents, only one full financial year is available (2020/21) due to the implementation of a new system two months prior to the start of the financial year. And crimes include reports made during the financial years 2016/17 – 2020/21.
Lincolnshire Police	2015-20	2015-20	N/A
Leicestershire Police	2015-20	2015-20	N/A
Northamptonshire Police	2015-20	2015-20	N/A
Cambridgeshire Constabulary	2015-21	2015-21	N/A
Norfolk Constabulary	2015-20	2015-20	N/A
Suffolk Constabulary	2015-20	2015-20	N/A

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Table B.2 – continued from previous page

Police Force	DA incidents	DA crimes	Detailed
Bedfordshire Police	2015-21	2015-21	In May 2018 Bedfordshire Police invested in a new crime recording system, therefore the data provided has been split between CMS data (the crime recording system up to 22.05.2018) & Athena data (the crime recording system from 23.05.2018 onwards).
Hertfordshire Constabulary	2015-20	2015-20	N/A
Essex Police	2015-20	2015-20	N/A
Thames Valley Police	2015-20	2015-20	N/A
Hampshire and Isle of Wight Constabulary	N/A	N/A	Hampshire Constabulary refused the FOI request on the grounds that providing the requested data would exceed the appropriate 18-hour time and £450 cost limit, citing the lack of readily available statistical information and the challenges in extracting accurate data from their crime recording system.
Surrey Police	2015-20	2015-20	N/A
Kent Police	2015-20	2015-20	Kent Police noted that the data has been extracted from STORM call recording system and Athena Crime on 31 May 2023 and is subject to change. The incident data has been collated from STORM, and filtered to any calls logged with a qualifier description of 'Domestic Abuse', 'DA Intimate Partner' or 'DA Family Member'. The crime date has been collated from Athena Crime, and filtered to those with a Domestic Abuse marker.
Sussex Police	2017-20	2017-20	Sussex Police provided data on domestic abuse incidents and crimes from 2017/18, adhering to Management of Police Information (MOPI) rules, and thus excluding data for the requested period of 2015/16.
City of London Police	2015-20	2015-20	N/A
Devon & Cornwall Police	2018-20	2015-20	Devon & Cornwall Police detailed that the data sources for incidents are from the Force Incident Recording System STORM and crimes from the Force Crime Recording System UNIFI, with incident data available only from 01/04/18 onwards.

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Table B.2 – continued from previous page

Police Force	DA incidents	DA crimes	Detailed
Avon and Somerset Police	2015-20	2015-20	N/A
Gloucestershire Constabulary	2015-20	2015-20	N/A
Wiltshire Police	2015-20	2015-20	N/A
Dorset Police	2015-20	2015-20	N/A

B.1.4 CSEW Questionnaire Items for Abuse Measures

Table B.3 lists the CSEW questions used to construct abuse-related variables ('Any Abuse', 'Physical Abuse', 'Non-Physical Abuse'), offering full transparency in measurement.

Table B.3: Key Variables and Related Questions from CSEW questionnaire

Variables	**Related questions from CSEW IPV module**
Any Abuse	Comprised of Physical Abuse and Non-Physical Abuse.
Physical Abuse (including sexual assault)	Has a partner or ex-partner used force on you in the last 12 months? For example, they may have pushed you, slapped you, hit, punched or kicked you, choked you or used a weapon against you.
	And have you been injured (even if only slightly) in the last 12 months as a result of the force used on you? By injured, we mean things such as bruises, black eyes, cuts or scratches, or broken bones.
	Has a partner or ex-partner indecently exposed themselves to you (i.e. flashing) in a way that caused you fear, alarm or distress in the last 12 months?
	Has a partner or ex-partner touched you in a sexual way (e.g. touching, grabbing, kissing or fondling) in the last 12 months, when you did not want it?
	In the last 12 months, has a partner or ex-partner ever penetrated your [mouth, vagina or anus/mouth or anus] with their penis when you made it clear that you did not agree or when you were not capable of consent?
	In the last 12 months, has a partner or ex-partner ever penetrated your [vagina or anus/anus] with an object (including their fingers) when you made it clear that you did not agree or when you were not capable of consent?
Non-Physical Abuse (including stalking)	Has a partner or ex-partner done any of these things to you in the last 12 months? – Prevented you from having your fair share of the household money – Stopped you from seeing friends and relatives – Repeatedly belittled you to the extent that you felt worthless
	Has a partner or ex-partner frightened or threatened you in any way in the last 12 months? For example, they may have threatened to hurt you, to kill you, to use a weapon on you, or to hurt someone close to you [such as your children]?
	Has a partner or ex-partner frightened or threatened you in any way in the last 12 months? For example, they may have threatened to hurt you, to kill you, to use a weapon on you, or to hurt someone close to you [such as your children]?

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Table B.3 – continued from previous page

Variables	Related questions from CSEW IPV module
	In the last 12 months has a partner or ex-partner sent you more than one unwanted letter, text message or card that was either obscene or threatening and which caused you fear, alarm or distress?
	In the last 12 months has a partner or ex-partner made more than one obscene, threatening, nuisance or silent phone call to you which caused you fear, alarm or distress?
	In the last 12 months has a partner or ex-partner waited or loitered outside your home or workplace on more than one occasion in a manner which caused you fear, alarm or distress?
	In the last 12 months has a partner or ex-partner followed you around and watched you on more than one occasion in a manner which caused you fear, alarm or distress?
	In the last 12 months has a partner or ex-partner sent you more than one unwanted email or social network message that was obscene or threatening and which caused you fear, alarm or distress?
	In the last 12 months has a partner or ex-partner put personal, obscene or threatening information about you on the internet on more than one occasion and which caused you fear, alarm or distress?

Notes: These questions related to experiences of abuse by a partner or ex-partner in the last 12 months, we have applied the same question format to three other contexts: 'abuse by partner or ex-partner since age 16', 'abuse by a family member in the last 12 months', and 'abuse by a family member since age 16'. 'Partner' refers to a boyfriend, girlfriend, husband, wife or civil partner. 'Family member' encompasses parents, children, brothers or sisters, or any other relatives, excluding partners. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

B.1.5 CSEW Questionnaire Items for Other Victimisation

Table B.4: CSEW Victimisation Question Wording

Column in Table B.15	Variable	Description
(1)	vehicle theft	If vehicle stolen or driven away without permission
(2)	vehicle stole	If something stolen off or out of vehicle
(3)	car damage	If vehicle tampered with or damaged
(4)	deliberate damage personal items	If personal items have been deliberately damaged
(5)	threaten to damage /use force	If anyone has threatened to damage things/ use force or violence
(6)	steal from house	Did anyone get into your place and steal or try to steal anything?
(7)	damage your house	Did anyone get into your house and cause damage?
(8)	steal from outside house	Did anyone stolen from outside the house/flat?
(9)	damage outside house	Did anyone damaged outside your residence?
(10)	fraud	Did any of these things happen? - your personal information or account details were used or tried to be used to obtain money, or buy goods or services - you were tricked or deceived out of money or goods (in person, by telephone or online) - someone TRIED to trick or deceive you out of money or goods (in person by telephone or online) - your personal information or details were accessed or used without your permission - an internet-enabled device of yours was infected or interfered with, for example, by a virus
(11)	info loss	Have your personal information or account details been used to obtain money, or buy goods or services without your permission or knowledge?
(12)	infected device	Has a computer or other internetenabled device of yours been infected or interfered with, for example by a virus?
(13)	try to get in	Did anyone tried to get in to steal or cause damage?

Source: Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

B.1.6 Refuge Partnerships

Table B.5: Statutory Partnership with Refuge

Counties and Unitary Authorities Code	Year 2016	Year 2017	Year 2018	Year 2019	Year 2020
E09000005	Brent	Brent	Brent	Brent	Brent
E09000009	Ealing	Ealing	Ealing	Ealing	Ealing
E09000012	Hackney	Hackney	Hackney	Hackney	Hackney
E09000018	Hounslow	Hounslow	Hounslow	Hounslow	Hounslow
E09000022	Lambeth	Lambeth	Lambeth	Lambeth	Lambeth
E09000023	Lewisham	Lewisham	Lewisham	Lewisham	Lewisham
E09000026	Redbridge	Redbridge	Redbridge	Redbridge	Redbridge
E09000027	Richmond	Richmond	Richmond	Richmond	Richmond
E09000028	Southwark	Southwark	Southwark	Southwark	Southwark
E09000031	Waltham Forest	Waltham Forest	Waltham Forest	Waltham Forest	Waltham Forest
E10000003	Cambridgeshire	Cambridgeshire	Cambridgeshire	Cambridgeshire	Cambridgeshire
E10000015	Hertfordshire	Hertfordshire	Hertfordshire	Hertfordshire	Hertfordshire
E10000011	East Sussex	East Sussex	East Sussex	East Sussex	East Sussex
E10000031	Warwickshire	Warwickshire	Warwickshire	Warwickshire	Warwickshire
E06000015	Derby	Derby	Derby	Derby	Derby
E06000007	Warrington	Warrington	Warrington	Warrington	Warrington
E08000026	Coventry	Coventry	Coventry	Coventry	
E09000021		Kingston on Thames	Kingston on Thames	Kingston on Thames	Kingston on Thames
E06000051	Shropshire				
E09000002				Barking and Dagenham	Barking and Dagenham
E09000017				Hillingdon	Hillingdon
E09000024					Merton
E09000032					Wandsworth
E09000033					Westminster

B.2 Summary Statistics

B.2.1 Summary Statistics for CSEW Variables

The summary statistics are based on data from the Crime Survey for England and Wales (CSEW) covering the years 2016 to 2019. For detailed information on the questions used to construct the variables 'Any Abuse', 'Physical Abuse', and 'Non-Physical Abuse', please refer to Table B.3. The values 0 and 1 in the variables under 'Experienced abuse since age 16' and 'Experienced abuse in the last 12 months' correspond to 'No' and 'Yes', respectively. Regarding 'Respondent's characteristics', 'Health in general' is categorised from 1 ('Very Poor') to 5 ('Very Good'). The 'ONS harmonised ethnic groups' variable also ranges from 1 to 5, denoting different ethnic categories. The 'Respondent education' variable is categorised from 0 ('No Education') to 3('A-level and Above'). 'Number of children in household' and 'Number of adults in household' quantify the children (under age 16) and adults within the household, respectively. 'Gender' is coded with 0 for male and 1 for female, while 'Age' captures the respondent's age at the interview year. The presence of certain respondent conditions or statuses, such as being cohabiting, married, a Household Reference Person (HRP), having bad health, being a UK national, or born in the UK, is indicated with a 1 for 'Yes' and a 0 for 'No'.

Table B.6: Descriptive Statistics of Self-Reported CSEW Data

	(1) Obs	(2) Mean	(3) Std.Dev	(4) Min Values	(4) Min Obs	(5) Max Values	(5) Max Obs
Experienced abuse since age 16							
Any abuse by partner	55,679	0.208	0.406	0	44,087	1	11,592
Physical abuse by partner	55,610	0.163	0.369	0	46,547	1	9,063
Non-physical abuse by partner	55,641	0.154	0.361	0	47,050	1	8,591
Any abuse by family member	55,616	0.0892	0.285	0	50,654	1	4,962
Physical abuse by family member	55,553	0.0603	0.238	0	52,205	1	3,348
Non-physical abuse by family member	55,569	0.0554	0.229	0	52,493	1	3,076
Experienced abuse in the last 12 months							
Any abuse by partner	55,671	0.0533	0.225	0	52,701	1	2,970
Physical abuse by partner	55,610	0.0272	0.163	0	54,099	1	1,511
Non-physical abuse by partner	55,629	0.0420	0.201	0	53,292	1	2,337
Any abuse by family member	55,615	0.0212	0.144	0	54,437	1	1,178
Physical abuse by family member	55,599	0.0130	0.113	0	54,874	1	725
Non-physical abuse by family member	55,568	0.0153	0.123	0	54,720	1	848
Respondent's characteristics							
Health in general	77,698	1.816	0.831	1	30,884	5	609
ONS harmonised ethnic groups	77,622	1.340	0.869	1	66,190	5	912
Number of children in household	77,728	0.725	1.016	0	45,582	5	307
Gender	77,728	0.541	0.498	0	35,642	1	42,086
Age	77,728	40.17	11.60	16	658	59	2,004
Respondent is cohabiting	77,617	0.148	0.356	0	66,093	1	11,524
Respondent has been married	77,580	0.563	0.496	0	33,866	1	43,714
Whether respondent is Household Reference Person (HRP)	77,728	0.685	0.464	0	24,460	1	53,268
Urban (1) or Rural (0) indicator	77,728	0.822	0.383	0	13,871	1	63,857
Full time (1) /Part time (0) worker	62,254	0.773	0.419	0	14,132	1	48,122
Respondent education	77,559	2.047	1.049	0	7,531	3	37,071
No education (1) indicator	77,559	0.0971	0.296	0	70,028	1	7,531
Whether respondent has bad health	77,698	0.0410	0.198	0	74,511	1	3,187
Respondent is UK national	77,649	0.880	0.325	0	9,353	1	68,296
Respondent is UK born	77,640	0.808	0.393	0	14,869	1	62,771
Number of adults in household	77,728	2.039	0.909	1	20,401	5	1,396

Source: Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12

Table B.7: Descriptive Statistics of Self-Reported CSEW Data – Female Respondents

VARIABLES	(1) Obs	(2) Mean	(3) Std.Dev	(4) Min		(5) Max	
				Values	Obs	Values	Obs
Experienced abuse since age 16							
Any abuse by partner	30,236	0.278	0.448	0	21,823	1	8,413
Physical abuse by partner	30,189	0.228	0.420	0	23,301	1	6,888
Non-physical abuse by partner	30,219	0.214	0.410	0	23,757	1	6,462
Any abuse by family member	30,292	0.110	0.313	0	26,961	1	3,331
Physical abuse by family member	30,250	0.0751	0.264	0	27,979	1	2,271
Non-physical abuse by family member	30,266	0.0708	0.257	0	28,123	1	2,143
Experienced abuse in the last 12 months							
Any abuse by partner	30,230	0.0714	0.257	0	28,072	1	2,158
Physical abuse by partner	30,189	0.0376	0.190	0	29,055	1	1,134
Non-physical abuse by partner	30,209	0.0573	0.232	0	28,477	1	1,732
Any abuse by family member	30,292	0.0253	0.157	0	29,527	1	765
Physical abuse by family member	30,280	0.0162	0.126	0	29,789	1	491
Non-physical abuse by family member	30,267	0.0187	0.136	0	29,700	1	567
Respondent's characteristics							
Health in general	42,064	1.819	0.844	1	16,926	5	345
ONS harmonised ethnic groups	42,029	1.338	0.870	1	35,913	5	458
Number of children in household	42,086	0.812	1.042	0	22,585	5	178
Age	42,086	40.03	11.49	16	335	59	1,026
Respondent is cohabiting	42,030	0.143	0.350	0	36,024	1	6,006
Respondent has been married	42,010	0.578	0.494	0	17,722	1	24,288
Whether respondent is Household Reference Person (HRP)	42,086	0.586	0.493	0	17,420	1	24,666
Urban (1) or Rural (0) indicator	42,086	0.818	0.386	0	7,642	1	34,444
Full time (1) /Part time (0) worker	31,862	0.636	0.481	0	11,587	1	20,275
Respondent education	41,995	2.061	1.053	0	3,918	3	20,744
No education indicator	41,995	0.0933	0.291	0	38,077	1	3,918
Whether respondent has bad health	42,064	0.0445	0.206	0	40,194	1	1,870
Respondent is UK national	42,049	0.878	0.327	0	5,136	1	36,913
Respondent is UK born	42,039	0.809	0.393	0	8,036	1	34,003
Number of adults in household	42,086	2.001	0.897	1	11,905	5	677

Source: Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Table B.8: Descriptive Statistics of Self-Reported CSEW Data – Male Respondents

VARIABLES	(1) Obs	(2) Mean	(3) Std.Dev	(4) Min		(5) Max	
				Values	Obs	Values	Obs
Experienced abuse since age 16							
Any abuse by partner	25,443	0.125	0.331	0	22,264	1	3,179
Physical abuse by partner	25,421	0.0856	0.280	0	23,246	1	2,175
Non-physical abuse by partner	25,422	0.0837	0.277	0	23,293	1	2,129
Any abuse by family member	25,324	0.0644	0.245	0	23,693	1	1,631
Physical abuse by family member	25,303	0.0426	0.202	0	24,226	1	1,077
Non-physical abuse by family member	25,303	0.0369	0.188	0	24,370	1	933
Experienced abuse in the last 12 months							
Any abuse by partner	25,441	0.0319	0.176	0	24,629	1	812
Physical abuse by partner	25,421	0.0148	0.121	0	25,044	1	377
Non-physical abuse by partner	25,420	0.0238	0.152	0	24,815	1	605
Any abuse by family member	25,323	0.0163	0.127	0	24,910	1	413
Physical abuse by family member	25,319	0.00924	0.0957	0	25,085	1	234
Non-physical abuse by family member	25,301	0.0111	0.105	0	25,020	1	281
Respondent's characteristics							
Health in general	35,634	1.811	0.814	1	13,958	5	264
ONS harmonised ethnic groups	35,593	1.342	0.868	1	30,277	5	454
Number of children in household	35,642	0.623	0.974	0	22,997	5	129
Age	35,642	40.33	11.74	16	323	59	978
Respondent is cohabiting	35,587	0.155	0.362	0	30,069	1	5,518
Respondent has been married	35,570	0.546	0.498	0	16,144	1	19,426
Whether respondent is Household Reference Person (HRP)	35,642	0.802	0.398	0	7,040	1	28,602
Urban (1) or Rural (0) indicator	35,642	0.825	0.380	0	6,229	1	29,413
Full time (1) /Part time (0) worker	30,392	0.916	0.277	0	2,545	1	27,847
Respondent education	35,564	2.029	1.045	0	3,613	3	16,327
No education indicator	35,564	0.102	0.302	0	31,951	1	3,613
Whether respondent has bad health	35,634	0.0370	0.189	0	34,317	1	1,317
Respondent is UK national	35,600	0.882	0.323	0	4,217	1	31,383
Respondent is UK born	35,601	0.808	0.394	0	6,833	1	28,768
Number of adults in household	35,642	2.084	0.921	1	8,496	5	719

Source: Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

B.2.2 Summary Statistics for Local Authority Spending and Contextual Data

Table B.9: Descriptive Statistics of Local Authority Funds, 2016-2020

Variables	Obs	Mean	Std.Dev	Min	Max
Revenue Account Budget (Net Current Expenditure)					
Total Education Services	727	223,920	172,397	307	1,195,839
Traffic management and road safety	727	474.3	970.5	-2,350	7,591
Total Highways and Transport Services	727	15,556	19,280	-52,476	119,933
Total Children's Social Care	727	57,578	39,015	83	251,045
Social support: Substance misuse support	727	177.9	310.4	-218	2,518
Total Adult Social Care	727	106,827	85,093	685	491,298
Substance misuse: Treatment for alcohol misuse in adults	727	1,168	1,026	0	6,744
Substance misuse: Preventing and reducing harm from alcohol misuse in adults	727	272.0	363.6	0	2,153
Miscellaneous public health services - other	727	2,655	2,727	-1,083	18,927
Total Public Health	727	22,297	14,118	0	97,215
Housing welfare: Supporting people	727	2,502	3,098	-2,062	24,666
Total Housing Services	727	8,409	7,941	-687	49,295
Total Cultural and Related Services	727	11,027	8,221	-3,068	58,412
Community safety (CCTV)	727	230.0	335.3	-186	2,450
Total Environmental and Regulatory Services	727	24,759	15,489	928	86,028
Total Planning and Development Services	727	3,928	3,662	-12,465	25,506
Total Central Services	727	12,920	9,295	-3,583	81,025
Total Service Expenditure	727	492,340	344,221	5,176	2,109,762

Notes: *Measured per thousand £.*

Table B.10: Descriptive Statistics of Complementary Area-Level Data, 2016-2020

Variables	Obs	Mean	Std.Dev	Min	Max
Share of female police officer	740	31.88	2.725	23.45	41.27
Total police staff	740	14,694	16,655	1,244	47,870
Population density	740	2,832	3,399	63.55	16,791
Sex ratio in local authority level	735	98.24	3.574	90.61	122.2
Share of White population in local authority	740	0.789	0.179	0.308	0.977
Absence rate of police staff	740	0.0222	0.00702	0.00604	0.0349
Police staff count aged under 25	740	1,205	1,340	36	4,447
Police staff count aged 26 to 40	740	6,324	7,621	516	21,402
Ethnic minority staff proportion of total police force	740	0.0739	0.0638	0.00731	0.192
New female joiner proportion of total police staff	740	0.0468	0.0166	0.0161	0.119
New joiner proportion of total police staff	740	0.103	0.0306	0.0465	0.255
Female leaver proportion of total police staff	740	0.0369	0.0108	0.0178	0.0726
Leaver proportion of total police staff	740	0.0925	0.0214	0.0521	0.168
Female promotion rate within total police staff	740	0.00354	0.00246	0	0.0135
Promotion rate within total police staff	740	0.0123	0.00844	0	0.0519
Economic activity rate (aged 16-64)	730	78.38	4.105	61.40	89.20
Employment rate (aged 16-64)	730	74.71	4.698	58.70	89.20
Economic activity rate females (aged 16-64)	729	73.64	5.245	50.80	88.30
Employment rate females (aged 16-64)	729	70.31	5.688	50.80	83.50

B.3 Supplementary Regression Results

Table B.11: Experienced Domestic Abuse in the Last 12 Months Among All Respondents

	(1)	(2)	(3)	(4)
Panel A: Experienced any abuse by partner in the last 12 months				
Share of Female Councillors	-0.0345***	-0.0310**	-0.0328**	-0.0113
	(0.013)	(0.013)	(0.013)	(0.015)
Observations	55,670	44,877	44,877	44,877
Panel B: Experienced physical abuse by partner in the last 12 months				
Share of Female Councillors	-0.0122	-0.0144	-0.0160*	-0.0012
	(0.009)	(0.009)	(0.009)	(0.011)
Observations	55,609	44,840	44,840	44,840
Panel C: Experienced non-physical abuse by partner in the last 12 months				
Share of Female Councillors	-0.0290**	-0.0318***	-0.0326***	-0.0203
	(0.012)	(0.012)	(0.012)	(0.013)
Observations	55,628	44,847	44,847	44,847
Control		✓	✓	✓
Year FE			✓	✓
Regional FE				✓

Notes: The explanatory variable represents the proportion of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. The control variables, along with year and regional-fixed effects, are gradually included in the model. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Table B.12: Domestic Abuse Experienced in the Last 12 Months by Male Respondents

	(1)	(2)	(3)	(4)
Panel A: Experienced any abuse by partner in the last 12 months				
Share of Female Councillors	-0.0077	0.0010	0.0008	0.0083
	(0.014)	(0.014)	(0.014)	(0.017)
Observations	25,440	21,791	21,791	21,791
	(5)	(6)	(7)	(8)
Panel B: Experienced physical abuse by partner in the last 12 months				
Share of Female Councillors	-0.0074	-0.0100	-0.0101	-0.0088
	(0.011)	(0.011)	(0.011)	(0.015)
Observations	25,420	21,778	21,778	21,778
	(9)	(10)	(11)	(12)
Panel C: Experienced non-physical abuse by partner in the last 12 months				
Share of Female Councillors	0.0008	0.0068	0.0065	0.0170
	(0.013)	(0.012)	(0.012)	(0.014)
Observations	25,419	21,776	21,776	21,776
Control		✓	✓	✓
Year FE			✓	✓
Regional FE				✓

Notes: In this regression analysis for male respondents, the explanatory variable is the proportion of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. The control variables, along with year and regional-fixed effects, are gradually included in the model. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Table B.13: Domestic Abuse Experienced by All Respondents from Family Members in the Last 12 Months

	(1)	(2)	(3)	(4)
Panel A: Experienced any abuse in the last 12 months				
Share of Female Councillors	-0.0052	-0.0083	-0.0111	-0.0031
	(0.010)	(0.010)	(0.010)	(0.012)
Observations	55,614	44,845	44,845	44,845
	(5)	(6)	(7)	(8)
Panel B: Experienced physical abuse in the last 12 months				
Share of Female Councillors	-0.0036	-0.0114	-0.0134*	-0.0088
	(0.007)	(0.007)	(0.008)	(0.009)
Observations	55,598	44,837	44,837	44,837
	(9)	(10)	(11)	(12)
Panel A: Experienced non-physical abuse in the last 12 months				
Share of Female Councillors	-0.0026	-0.0010	-0.0033	0.0004
	(0.008)	(0.009)	(0.009)	(0.010)
Observations	55,567	44,810	44,810	44,810
Control		✓	✓	✓
Year FE			✓	✓
Regional FE				✓

Notes: In this regression, the dependent variable is experienced domestic violence by family member in the last 12 months, the explanatory variable is the share of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. The control variables, along with year and regional-fixed effects, are gradually included in the model. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Table B.14: Female Councillors and Police Recorded Other Crimes

	(1) Anti Social	(2) Public Order	(3) Robbery	(4) Burglary	(5) Damage
% of Female Cllr	-0.034	0.029	-0.000	-0.006	0.013
	(0.054)	(0.020)	(0.009)	(0.014)	(0.022)
Observations	725	724	715	717	723
R-squared	0.829	0.684	0.773	0.799	0.753
	(6) Vehicle	(7) Drug	(8) Theft fr Person	(9) Other Theft	(10) Other Crime
% of Female Cllr	0.002	0.009	-0.012	-0.007	-0.001
	(0.023)	(0.012)	(0.029)	(0.039)	(0.005)
Observations	723	722	717	723	722
R-squared	0.775	0.917	0.761	0.819	0.754
Controls	✓	✓	✓	✓	✓
PFA FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Notes: The primary explanatory variable represents the percentage of female councillors in each local council, ranging from 0 to 100. Control variables include: the size of the local government; the percentage of female police officers, total police staff, age composition, ethnic composition, absence rate, join rate, leave rate, and promotion rate in police forces; Employment rate (and for females), economic active rate (and for females), population density, sex ratio, and ethnic composition in local areas. The model includes regional and year-fixed effects. Standard error adjusted for 136 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.15: Female Councillors and Self-Reported Experiences of Other Victimisation

	(1) Vehicle Theft	(2) Vehicle Stole	(3) Car Damage	(4) Damage Other	(5) Threaten Damage
% of Female Cllr	0.0115	0.0184	0.0035	-0.0003	0.0197
	(0.008)	(0.020)	(0.024)	(0.004)	(0.016)
Observations	54,130	54,114	54,061	61,932	61,925
	(6) Steal fr House	(7) Damage House	(8) Steal out.house	(9) Damage out.house	(10) Fraud
% of Female Cllr	0.0038	0.0038	0.0097	0.0106	0.0078
	(0.008)	(0.003)	(0.013)	(0.008)	(0.032)
Observations	61,923	61,938	61,909	61,928	10,120
	(11) Info Loss	(12) Infected Device	(13) Try to get in		
% of Female Cllr	0.0400	0.0046	0.0096		
	(0.027)	(0.015)	(0.008)		
Observations	49,369	49,290	61,903		
Control	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Regional F	✓	✓	✓	✓	✓

Notes: In this regression, the dependent variable is experienced a set of other crimes in the last 12 months, the explanatory variable is the share of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. Control variables include the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, and the number of children and adults in the household. The control variables, year and regional-fixed effects, are included in all models. Standard error adjusted for 151 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12.

Table B.16: Heterogeneous Effects by London vs. Non-London Areas

	London		Non-London	
	(1)	(2)	(3)	(4)
Panel A: Domestic Abused-Related Incidents and Crimes				
% of Female Councillors	-0.094	-0.074	0.576***	0.105
	(0.080)	(0.070)	(0.117)	(0.092)
Controls & FEs		✓		✓
Observations	133	132	440	430
R-squared	0.032	0.556	0.150	0.736
	(5)	(6)	(7)	(8)
Panel B: Domestic Abused-Related Incidents				
% of Female Councillors	-0.061	-0.042	0.317***	0.067
	(0.036)	(0.031)	(0.084)	(0.066)
Controls & FEs		✓		✓
Observations	133	132	444	434
R-squared	0.059	0.572	0.105	0.704
	(9)	(10)	(11)	(12)
Panel C: Domestic Abused-Related Crimes				
% of Female Councillors	-0.022	-0.029	0.261***	0.034
	(0.046)	(0.039)	(0.046)	(0.034)
Controls & FEs		✓		✓
Observations	165	164	472	462
R-squared	0.005	0.552	0.150	0.729

Notes: The sample is split by geographic location. "London" refers to local authorities within Greater London, including the City of London; "Non-London" refers to all other local authorities. The key explanatory variable is the percentage of female councillors in each local council (0-100). Control variables include local government size; police force characteristics (percentage of female officers, total staff, age composition, ethnic composition, absence rate, entry rate, exit rate, and promotion rate); and local area characteristics (employment rate, female employment rate, economic activity rate, female economic activity rate, population density, sex ratio, and ethnic composition of the local population). All specifications include region and year fixed effects. Standard errors are clustered at the local authority level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.17: Instrumental Variable Regression Results for Female Respondents

	(1) OLS	(2) OLS (IV sample)	(3) First Stage	(4) 2SLS
% Female Councillors	-0.0633***	-0.0137		-0.1995
	(0.022)	(0.082)		(0.240)
female_adv_pos_w_cand			0.2779**	
			(0.116)	
Observations	23,071	3,696	3,696	3,696
First-stage F-stat			5.735	
	(5) First Stage	(6) 2SLS	(7) First Stage	(8) 2SLS
% Female Councillors		-0.0912		-0.1985
		(0.201)		(0.239)
female_top_2_pos_w_cand	0.3151***			
	(0.098)			
female_top_3_pos_w_cand			0.2779**	
			(0.116)	
Observations	3,696	3,696	3,696	3,696
First-stage F-stat	10.36		5.744	
	(9) First Stage	(10) 2SLS	(11) First Stage	(12) 2SLS
% Female Councillors		-0.1025		-0.0305
		(0.226)		(0.201)
female_adv_pos_w_seats	0.2910**			
	(0.113)			
female_top_2_pos_w_seats			0.3252***	
			(0.094)	
Observations	3,696	3,696	3,696	3,696
First-stage F-stat	6.626		12.05	
	(13) First Stage	(14) 2SLS	(15) First Stage	(16) 2SLS
% Female Councillors		-0.1010		-0.1144
		(0.225)		(0.258)
female_top_3_pos_w_seats	0.2910**			
	(0.113)			
female_adv_pos_avg			0.2624**	
			(0.112)	
Observations	3,696	3,696	3,696	3,696
First-stage F-stat	6.644		5.483	
	(17) First Stage	(18) 2SLS	(19) First Stage	(20) 2SLS
% Female Councillors		-0.0507		-0.1135
		(0.221)		(0.258)
female_top_2_pos_avg	0.3049***			
	(0.100)			
female_top_3_pos_avg			0.2624**	
			(0.112)	
Observations	3,696	3,696	3,696	3,696
First-stage F-stat	9.278		5.492	

Notes: This table presents OLS (full sample and IV sample) and IV regression results for female respondents. The dependent variable indicates whether the respondent experienced non-physical abuse from their partner or ex-partner in the last 12 months. The explanatory variable is the proportion of female councillors in each local councils, adjusted based on the interview year and month, with values ranging from 0 to 1. All models control for the size of local councils, respondent's age, cohabitation and marital status, whether the respondent is the household reference person, urban residency, employment status (part-time or full-time), education level, nationality, birthplace, ethnicity, health condition, the number of children and adults in the household, and region and year fixed effects. Standard error adjusted for 101 clusters in local authority area. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Office for National Statistics. (2023). *Crime Survey for England and Wales, 1996-2020: Secure Access.* [data collection]. *12th Edition.* UK Data Service. SN: 7280, DOI: http://doi.org/10.5255/UKDA-SN-7280-12

Appendix C

Appendix for Chapter 3

Table C.1: Applied Population Policies Across Provinces and Population Groups (1982–2013)

Population Policy	Details
One-Child Policy	1. The vast majority of urban residents; 2. Rural residents in six provinces and cities: Beijing, Tianjin, Shanghai, Jiangsu, Sichuan and Chongqing.
1.5-Child Policy	Rural residents whose first child is a girl may have a second child. The policy is implemented for rural residents in 19 provinces (autonomous regions), including Hebei, Shanxi, Inner Mongolia, Liaoning, Jilin, Heilongjiang, Zhejiang, Anhui, Fujian, Jiangxi, Shandong, Henan, Hubei, Hunan, Guangdong, Guangxi, Guizhou, Shaanxi and Gansu.
Two-Child Policy	1. Rural residents in five provinces (autonomous regions), including Hainan, Ningxia, Yunnan, Qinghai and Xinjiang; 2. Ethnic minorities with a population of less than 10 million (based on 1984); 3. Han Chinese residents and urban residents in Tibet.
Three-Child Policy	1. Some minorities with small populations (22 minorities with populations under 100,000); 2. Ethnic minority farmers and herders in Qinghai, Ningxia, Xinjiang, Sichuan, Gansu and other regions; 3. Ethnic minority farmers and herders in Hainan and Inner Mongolia where the first two children are girls.
Unlimited	Ethnic minority farmers and herders in Xizang Autonomous Region and remote areas.

Sources: Local regulations in each area.

Table C.2: Provincial Differences in Policy Response Time

Province	Strictly Selective Two-child Policy	Selective Two-child Policy	Universal Two-child Policy
Beijing	1984.10.01	2014.02.21	2016.03.24
Tianjin	1984.09.01	2014.02.14	2016.01.14
Hebei	1984.08.06	2014.05.30	2016.03.29
Shanxi	1987.01.01	2014.05.29	2016.01.20
Inner Mongolia	2002.12.01	2014.03.31	2016.03.30
Liaoning	1984.09.24	2014.03.28	2016.03.23
Jilin	1984.08.18	2014.03.28	2016.03.30
Heilongjiang	1984.08.02	2014.04.22	2016.04.21
Shanghai	1984.10.01	2014.03.01	2016.02.26
Jiangsu	1984.07.01	2014.03.28	2016.03.30
Zhejiang	1985.02.09	2014.01.17	2016.01.14
Anhui	1984.12.01	2014.01.23	2016.01.15
Fujian	1984.11.01	2014.03.29	2016.02.19
Jiangxi	1990.09.01	2014.01.18	2016.01.20
Shandong	1984.05.10	2014.05.31	2016.01.22
Henan	2011.11.25	2014.06.03	2016.05.27
Hubei	2003.01.01	2014.03.27	2016.01.13
Hunan	1985.01.01	2014.03.28	2016.03.30
Guangdong	1984.06.12	2014.03.27	2016.09.29
Guangxi	1985.04.03	2014.03.01	2016.01.15
Hainan	1984.06.12	2014.06.01	2016.03.31
Chongqing	1984.07.11	2014.03.26	2016.03.31
Sichuan	1984.07.11	2014.03.02	2016.01.22
Guizhou	1984.07.27	2014.05.18	2016.03.31
Yunnan	1991.04.01	2014.03.28	2016.03.31
Shaanxi	1986.07.31	2014.03.01	2016.05.26
Gansu	2001.12.01	2014.03.26	2016.04.01
Qinghai	1986.04.17	2014.03.27	2016.03.25
Ningxia	1986.08.28	2014.05.28	2016.01.21

Sources: Local regulations in each area.

Table C.3: Summary Statistics: Full Sample and Male Subsample

Panel A: All Respondents						
	Mean	Std. Dev	Min	Max	N	Obs (N×T)
Log of raw income	9.744	1.138	1.609	13.122	6625	21259
Employment status	0.861	0.346	0	1	7285	34829
Post-2014 extra-birth eligible	0.497	0.500	0	1	6216	33058
Age	39.419	7.715	20	50	7285	34829
Years of education	8.118	4.260	0	19	7285	34829
Married	0.914	0.281	0	1	7285	34829
Log of GRP	10.505	0.429	9.482	11.765	7285	34829
Sex ratio	104.576	3.527	98.230	118.620	7285	34829
Log of family expenditure	10.566	0.857	6.073	15.450	7285	30591
Panel B: All Male						
	Mean	Std. Dev	Min	Max	N	Obs (N×T)
Log of raw income	9.968	1.031	1.609	13.122	3215	11212
Employment status	0.925	0.263	0	1	3360	15962
Post-2014 extra-birth eligible	0.538	0.499	0	1	2864	15150
Age	39.405	7.780	20	50	3360	15962
Years of education	8.724	3.914	0	19	3360	15962
Married (Yes/No)	0.885	0.319	0	1	3360	15962
Log of GRP	10.501	0.436	9.482	11.765	3360	15962
Sex ratio	104.645	3.543	98.230	118.620	3360	15962
Log of family expenditure	10.559	0.873	6.073	15.450	3360	14027

Notes: Panel A includes all respondents in the panel dataset, while Panel B restricts the sample to male respondents only. N denotes the number of distinct individuals with non-missing observations for each variable, and $N \times T$ denotes the total number of person-year observations. The post-2014 extra-birth eligibility variable has a smaller sample size due to missing information on the number of children.

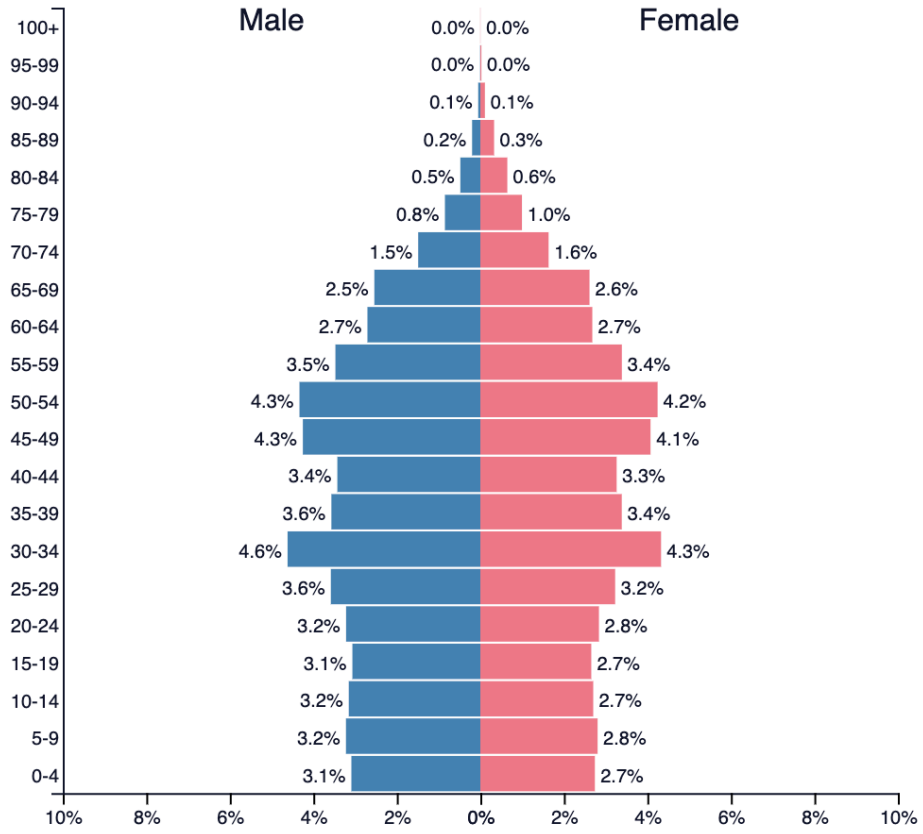

Figure C.1: China's Population Pyramid, 2020
Source: Population Pyramids of the World from 1950 to 2100. https://www.populationpyramid.net/china/2020/

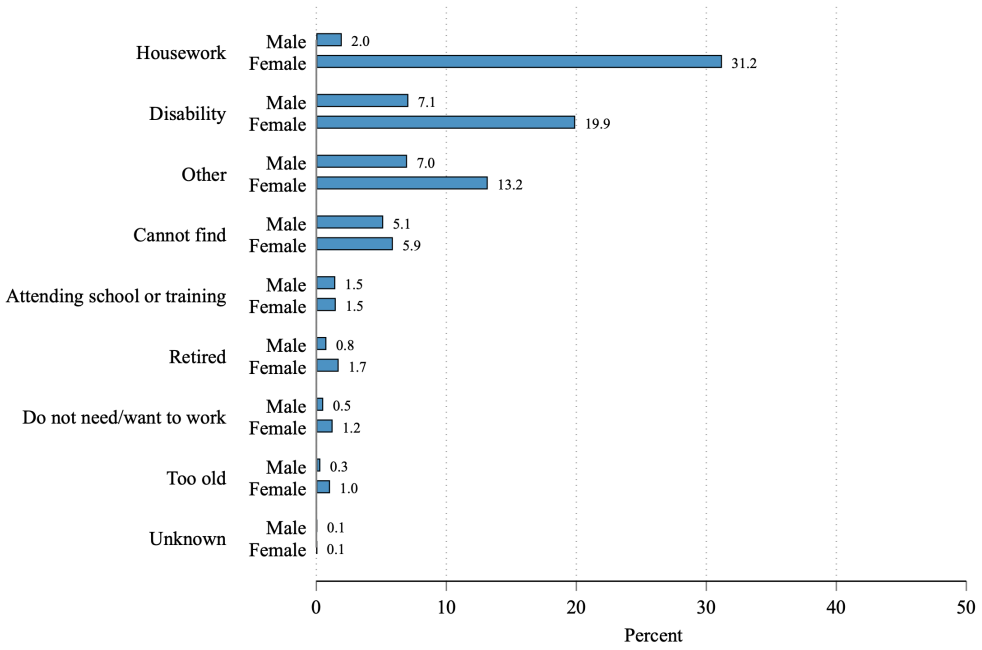

Figure C.2: Reasons for Unemployment by Gender in CFPS
Note: Excluding the "Not applicable" category, which accounts for 85.26% of the sample.

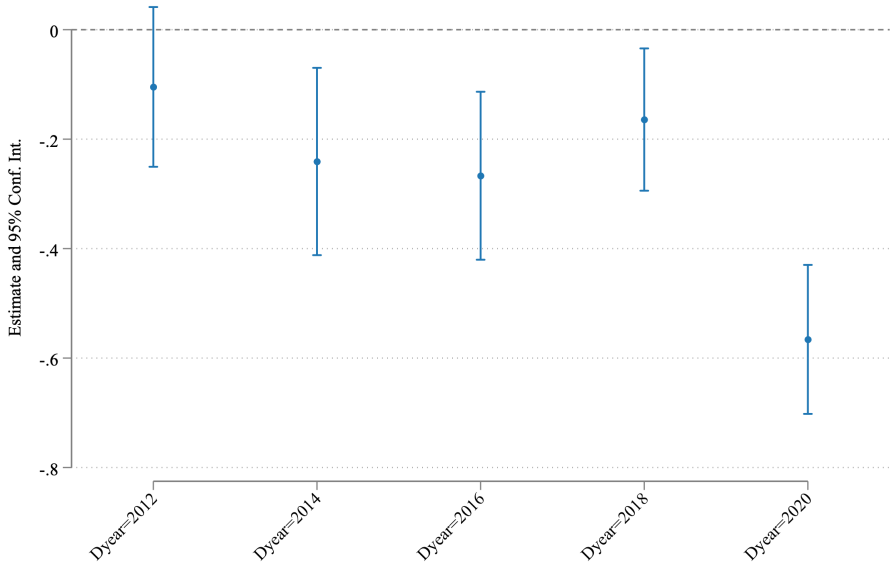

Figure C.3: Event Study: Impact of Eligibility on Wage Over Time

Table C.4: Effects of TCP on Target Females by Age Group

	Age Groups					
	(1) Age 25 or younger	(2) (25, 30]	(3) (30, 35]	(4) (35, 40]	(5) (40, 45]	(6) (45, 50]
$Eligible \times Post_{2014}$	0.429*	-0.270*	-0.410***	-0.400***	-0.263***	-0.124
	(0.236)	(0.160)	(0.115)	(0.110)	(0.093)	(0.148)
Controls	✓	✓	✓	✓	✓	✓
Provincial FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	475	892	1,234	1,644	1,949	1,496
R-squared	0.347	0.399	0.406	0.396	0.400	0.282

Notes: The outcome variable is the logarithm of annual wage income. All regressions control for education years, marital status, log of GRP, sex ratio, and log of family expense. Robust standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.5: Effects of TCP on Target Females by Education Level

	Education Level				
	(1) Illiterate /Semi-literate	(2) Primary school	(3) Junior high school	(4) Senior high school Vocational school	(5) College and above
$Eligible \times Post_{2014}$	-0.259	-0.163	-0.287***	-0.054	0.364**
	(0.179)	(0.111)	(0.079)	(0.137)	(0.147)
Controls	✓	✓	✓	✓	✓
Provincial FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	1,188	1,554	2,579	1,154	1,215
R-squared	0.239	0.251	0.239	0.255	0.329

Notes: The outcome variable is the logarithm of annual wage income. All regressions control for age, marital status, log of GRP, sex ratio, and log of family expense. Robust standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.6: Effects of TCP on Target Females by Job Categories

	Job Categories						
	(1) Leading cadres	(2) Prof. and tech.	(3) Office	(4) Service	(5) Agri.	(6) Prodt.	(7) Other
$Eligible \times Post_{2014}$	0.288	0.089	0.425	0.207***	-0.172	-0.350***	-0.259
	(0.278)	(0.119)	(0.342)	(0.074)	(0.111)	(0.091)	(0.421)
Controls	✓	✓	✓	✓	✓	✓	✓
Prov. FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	259	929	497	2,115	2,165	1,606	118
R-squared	0.397	0.395	0.287	0.220	0.223	0.253	0.492

Notes: The dependent variable is the logarithm of annual wage income. Column (1) reports estimates for the leading cadres subsample; column (2) for professional and technical workers; column (3) for office workers; column (4) for service workers; column (5) for agricultural workers; column (6) for production workers; and column (7) for other occupational categories. All specifications control for age, years of education, marital status, log gross regional product (GRP), sex ratio, and log household expenditure. Robust standard errors clustered at the individual level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C.7: Effects of TCP on Target Females by Urban-Rural Classification

	Urban-Rural Classification	
	(1) Urban	(2) Rural
$Eligible \times Post_{2014}$	-0.007 (0.061)	-0.205*** (0.077)
Controls	✓	✓
Provincial FE	✓	✓
Year FE	✓	✓
Observations	4,182	3,508
R-squared	0.330	0.293

Notes: The outcome variable is the logarithm of annual wage income. All regressions control for age, education years, marital status, log of GRP, sex ratio, and log of family expense. Robust standard errors clustered at the individual level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

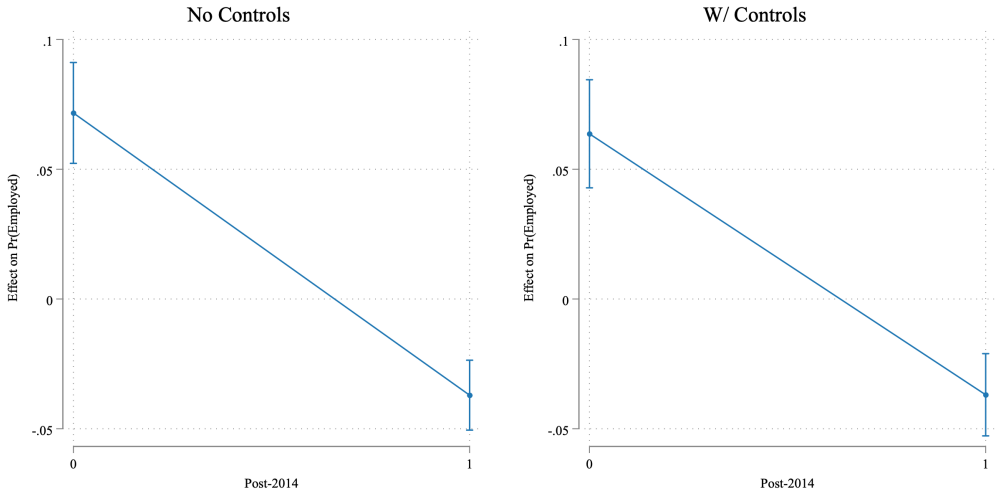

Figure C.4: Marginal Effects of Eligibility on Employment Probability Before and After 2014 (Unbalanced Panel)

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