Justice Under Austerity: The Impacts of Reduced Access to Legal Assistance in England and Wales

Juliet-Nil Uraz *

Preliminary Draft

December 14, 2025

Latest version available here.

Abstract

In 2013, England and Wales implemented a legal aid reform that drastically reduced public funding for civil legal assistance, eliminating early-stage support and restricting aid to last-resort courtroom interventions. This paper examines the broader socioeconomic and public health consequences of this reform, focusing on vulnerable households facing housing, debt, and welfare disputes. Using panel data from 2009–2023 on legal aid provider locations, service volumes, legal aid funding, court filings, and mortality, I quantify the reform's effects on access to justice and downstream outcomes. The empirical strategy combines a difference-in-differences design exploiting spatial and temporal variation in access driven by provider closures, with a Bartik-style instrument based on pre-reform exposure to funding cuts. Leveraging local variation in exposure to austerity, I test whether legal aid reductions amplified the impacts of concurrent welfare retrenchments. Findings suggest that exposed areas experienced increases in eviction filings, court orders, homelessness, and mortality. By narrowing legal aid to specific areas, the reform limited lawyers' ability to address interconnected issues, undermining the integrative role of generalist firms and driving them out of the market. This study highlights the importance of access to justice in social protection and suggests that cutting preventive legal services can shift costs onto local authorities and healthcare systems, revealing unintended long-term consequences of reducing the social safety net through legal reform.

JEL codes: G51, H53, H75, I38, K15, K41

Keywords: Legal Assistance, Eviction, Housing, Homelessness, Mortality, Austerity

*London School of Economics - Opportunity Insights, Harvard.

Corresponding author: j.uraz@lse.ac.uk - julietniluraz@fas.harvard.edu. I am grateful to Varsha Aithala, Catherine Albiston, Marcella Alsan, Elliott Ash, Elisabetta De Cao, Raj Chetty, Brandon Davis, Mary-Alice Doyle, Francisco Ferreira, Hazel Genn, Deven Ghelani, Thiemo Fetzer, Paul Heaton, Nathaniel Hendren, Johann Koehler, Luis F. López-Calva, Alyx Mark, Michael Makowsky, James Organ, Berkay Ozcan, Amaia Palencia-Esteban, Lindsey Poole, Rebecca Sandefur, Marisol Smith, Clara Schaeper, Margaux Suteau, Jo Wilding, the American Bar Foundation Early Career Access to Justice Initiative fellows, as well as many workshop and seminar participants for generously sharing their time and suggestions. I also thank Opportunity Insights for hosting this project upon its completion.

1 Introduction

Legal aid programs subsidize legal services for low-income individuals to improve access to justice. Although historically focused on criminal defence, there is growing recognition that legal assistance can mitigate socioeconomic disadvantage by helping households navigate everyday legal problems such as disputes over housing, debt, and welfare benefits. In the United States, several cities have recently begun funding legal representation for tenants facing eviction through right-to-counsel programs (Benfer et al. (2025)). By contrast, the United Kingdom pioneered an ambitious civil legal aid system in the postwar period.

Initially designed to support veterans seeking divorce, the UK's scheme evolved into a comprehensive, means-tested program covering most areas of civil law and all stages of the legal process. This system lowered informational barriers, reduced administrative frictions, and strengthened households' bargaining position vis-à-vis landlords, creditors, and the welfare state. Once nearly universal, eligibility and scope have since been dramatically narrowed: fewer than 25% of households now qualify based on income, and assistance is restricted to a limited set of legal cases (Pyper et al. (2020)). A series of reforms, culminating in a major retrenchment in 2013, transformed civil legal aid from a holistic, early-stage support system into a last-resort model concentrated on cases involving immediate risks to health or housing security. These changes offer a rare opportunity to study how large, system-wide reductions in civil legal assistance affect both the market for legal services and broader welfare outcomes.

Despite extensive documentation of the pervasiveness of legal problems in global surveys (World Justice Project (2019)), causal evidence on how legal aid affects welfare and health outcomes remains limited, particularly in the context of program retrenchment. The inclusion of Access to Justice in the United Nations Sustainable Development Goals (Goal 16.3) underscores the need for evidence on the welfare consequences of expanding or curtailing these programs (OECD (2019), World Bank (2019)). A growing body of evidence, primarily from the United States, has begun to document the benefits of expanding legal assistance in eviction cases, typically through local or city-level initiatives. We ex-

amine the inverse scenario: what happens when a mature, national civil legal aid system is sharply scaled back across multiple legal domains, and when preventive support is largely withdrawn. Focusing on the 2013 legal aid reform in England and Wales, this study provides new national-scale evidence on the downstream consequences of shifting provision from holistic, early-stage advice to specialist, last-resort defense. Specifically, we study how reduced legal support for welfare benefits, debt, and housing disputes affected (i) the supply and spatial distribution of publicly funded legal services and (ii) population-level outcomes, including eviction, homelessness, and mortality. In doing so, the paper connects to the literature on access to justice with research on social insurance, housing insecurity, and the health consequences of austerity.

The 2013 reform constituted one of the most significant austerity-era changes to the legal aid system in England and Wales. In response to the post-financial crisis fiscal consolidation, the UK government implemented sweeping cuts to public spending across multiple sectors, including justice and local government services. Civil legal aid was particularly affected: the reform drastically reduced funding and restricted eligibility to cases involving immediate risks to health or housing security such as domestic violence and eviction. The reform also eliminated most early-stage legal advice, previously a central component of the system that helped resolve disputes before they escalated to formal proceedings.

This shift transformed civil legal aid from a comprehensive service model into a last-resort regime focused on urgent court representation. The resulting contraction was abrupt and substantial. The number of supported civil cases fell by roughly 70%, and providers specialising in welfare benefits, debt, and housing experienced funding shocks of approximately 80%. Applied uniformly, the policy failed to account for variation in local capacity to absorb the shock. The resulting financial strain caused widespread office closures and increased congestion at surviving providers. The number of legal aid offices was effectively halved, forcing clients to travel farther and endure longer wait times. These disruptions generated widening spatial and temporal disparities in access to subsidized, in-person legal assistance, even for cases that remained eligible under the new rules. This geographic variation in service availability provides a quasi-experimental set-

ting to estimate the welfare impacts of reducing early-stage and multi-domain legal aid on both service provision and population welfare outcomes.

To quantify the dynamic causal effects of reduced access to civil legal aid, we assemble a new longitudinal dataset combining administrative records on legal aid providers with small-area indicators of legal market conditions and population welfare. Our core data come from the Legal Aid Agency's administrative records, which track quarterly activity for all providers holding a civil legal aid contract in England and Wales from 2009 to 2023. These records report provider-level case volumes by legal domain, the type of assistance delivered (legal advice vs. court representation), the costs incurred and charged to the Agency, the network structure linking firms and branch offices, the provider type (private or nonprofit), and the geographic location of each office. From these components, we construct measures of provider capacity, portfolio breadth across more than twenty legal domains, and funding streams at the firm and office level over the panel period. The panel structure allows us to identify the quarter in which an office stops effectively exits the market by ceasing to take new legal aid cases, and to compute precise travel distances to the nearest provider for each census area.

We link these provider measures to small-area-level outcomes capturing both the functioning of the legal services market and downstream welfare conditions. Legal aid market outcomes include the number of active legal aid offices, local service capacity across all legal domains of civil law, and changes in the supply of publicly funded casework over time. Population-level outcomes include eviction court filings and outcomes, homelessness prevention cases reviewed by local governments for emergency assistance, temporary accommodation and shelter occupancy, and age-adjusted mortality rates by cause of death. These measures rely on administrative data from the Ministry of Justice, Local Authority Housing Statistics, and the Office for National Statistics.

Our empirical strategy combines two complementary approaches. First, we implement a generalized difference-in-differences design using changes in distance to the nearest legal aid provider as a continuous treatment variable. This approach captures the *extensive* margin: the direct effect of spatial access disruptions generated by provider exits following the reform. Second, we develop a Bartik-style instrumental variable strategy

based on the pre-reform caseload composition of local providers. Areas with a higher concentration of providers specialising in now-excluded domains experienced larger post-reform contractions in service availability. This instrument isolates plausibly exogenous variation in post-reform access through differential exposure to the funding shock, capturing the *intensive* margin of provider response.

We employ multiple approaches in our difference-in-differences framework. First, we leverage the *spatial* variation induced by the reform's implementation. Specifically, we measure changes in the distance to the nearest legal aid provider over the two-year period from the reform's adoption by Parliament in June 2012 to one financial year post-implementation in March 2014. We estimate two effects separately: (i) the effect of a marginal increase in distance, treating distance change as a continuous variable, and (ii) the relative difference in effects between the most affected areas-those experiencing significant service disruptions-and those with little to no impact, using the quintile distribution of distance changes.

Despite a lack of pre-trends in the decade preceding the reform across all outcomes, we confirm our findings with doubly robust estimators following Sant'Anna and Zhao (2020), to account for treatment effect heterogeneity across groups and strong imbalance in sociodemographic characteristics across treated and control areas. We match on indicators of rurality, housing ownership, income, poverty, demographic composition, ethnic diversity, and baseline characteristics of the local legal aid market in terms of the number of cases and types of providers. This approach combines inverse propensity score weighting with outcome regression to achieve robustness to misspecification of either the treatment assignment or outcome model.

Our results are robust across specifications. Interestingly, the areas most likely to lose their nearest legal aid provider were on average richer, older, and ethnically less diverse, even within similar urbanity clusters. This pattern rules out the concern that providers would strategically leave areas with the worst case prospects or outcomes. We also exploit the *overtime* variation. Specifically, we account for the staggered and dynamic disruptions to legal services across the country, which led to differential timing in the increase in distance to the nearest provider. We estimate the effect of an increase in distance at any

point post-reform, using areas that had not yet experienced an increase as a comparison group and leveraging the richness of quarterly data on legal aid providers activity.

To further rule out concerns about endogeneity in distance changes and spatial autocorrelation, we turn to a Bartik-style instrument to predict changes in access to legal assistance. Following Goldsmith-Pinkham et al. (2020), we adopt a framework that treats shares as quasi-random. Our instrument leverages variation in legal providers' caseload composition to predict their resilience to legal aid cuts. Specifically, using data on case volume, we assess how the reform differentially affected providers' ability to continue delivering legal services based on the types of cases they handled pre-reform, categorized by area of law (e.g., housing vs. debt) and type of assistance (e.g., early vs. late-stage). This enables us to compare areas around surviving providers that adjusted their service levels at the margin instead of exiting the market.

We construct the instrument using providers' baseline caseload portfolios along with Ministry of Justice projections on the expected reform impact by case type. We then borrow estimates of spatial variation in exposure to broader austerity measures from Fetzer (2019) and Fetzer et al. (2023)—including cuts to welfare spending, rental assistance cuts, and recession intensity. This allows us to test whether reductions in legal aid amplified the socioeconomic impact of concurrent shocks, shedding light on the overlooked role of legal aid programs as fiscal multipliers.

In this paper, we explore the impacts of removing a program that targeted households vulnerable to homelessness and over-indebtedness, exploiting the reform-induced change in local access to legal assistance. Our study relies on the assumption that physical access to legal aid providers matters and cannot be perfectly substituted by digital solutions, a claim supported by the sociolegal literature. The legal aid reform impacted three critical dimensions of local access to legal aid: availability, affordability, and proximity. While these dimensions are interconnected, collectively they contributed to a reduction in access to timely, free, in-person legal assistance for local communities. Sociolegal literature highlights the importance of factors such as the timing of legal support, the cost of legal services, and the proximity of legal providers in effectively resolving legal problems for low-income households.

These barriers are particularly consequential for our study population. Legal aid is targeted towards households with severe budget constraints. By increasing the distances to the nearest legal aid providers, the reform imposed additional financial and time travel costs that households were unlikely to afford (Pleasence et al. (2004), Hirsch (2018)). Moreover, administrative boundaries between local authorities created further barriers to accessing providers that remained active, particularly for non-profit providers, which required their clients to meet residency requirements to continue receiving additional local authorities funding (Low Commission (2014)). Additionally, the reform did not introduce perfect substitutes for in-person services. Despite efforts to transition to digital and telephone provision of legal assistance, the reform created online tools that failed to adequately address the legal needs of the most vulnerable populations (Balmer et al. (2012), Smith et al. (2013), Burton (2018)). Critically, frontline practitioners and researchers alike have identified access to early legal advice as a key determinant in individuals' legal journeys, stressing the harmful consequences of delayed interventions (Pleasence et al. (2013), Ipsos-MORI (2017), Law Centre Network (2018)).

Our findings vary depending on the perspective adopted. When examining how the reform reshuffled the geography of services *in the two years* following its adoption, we find that the most exposed areas—those in the top quintile in terms of changes in distance gradually experienced a higher rate of eviction compared to areas that did not experience any change in distance to their nearest provider. This difference holds for both eviction filings and orders, with disparities becoming statistically significant two to three years after the reform adoption. These differences first widen during the pandemic, then gradually diminish during the period when eviction moratoriums were in place, but they do not disappear entirely even ten years post-reform. The doubly robust Average Treatment Effect pooled over the period suggests that the rates of eviction claims and orders per 100,000 inhabitants are higher in the most affected areas by an average of 8.7 and 5.6, respectively, representing a 26% and 13% increase compared to the 2011 mean baseline. These areas saw on average an increase in distance of 13.7 km.

Regarding mortality, the most exposed areas exhibited a small but significantly higher mortality rate 5 to 6 years later across all specifications, representing a 1% increase relative

to the mean baseline. With a continuous measure of change in distance, age-adjusted mortality increases by 2.22 per 100,000 inhabitants for each additional kilometer of increased distance. These mortality effects are concentrated among the elderly: when disaggregated by age cohort, the impacts are strongest for individuals aged 75 and over, consistent with this demographic's heightened vulnerability to in-person services disruptions. Interestingly, the share of legal aid cases handled by generalist providers at baseline serves as a strong protective factor; areas predominantly served by providers working across multiple domains of law prior to the reform fare substantially better than areas with similar changes in distance but different pre-reform market structures.

Importantly, the effects of legal aid cuts are dramatically amplified when combined with other austerity measures. Areas in the top quintile of exposure to both legal aid and rental assistance cuts, as measured by the Bartik IV, experienced seven times more homelessness prevention assessments than those in the bottom quintile of rental assistance exposure. The reform fundamentally restructured the legal aid market itself. Spatial inequality in provision, already present before 2013, was significantly amplified post-reform. The market underwent substantial consolidation, with fewer offices per firm and increased specialization. Offices also covered fewer areas of law on average. Most notably, generalist not-for-profit providers working across housing, debt, and benefits law were disproportionately forced out, experiencing higher exit rates than specialists facing comparable funding cuts. These generalists had specialized in early-stage advice, precisely the preventative services that the reform defunded.

Adopting a more dynamic perspective—considering changes in distance at any point in time, regardless of their magnitude yields statistically significant effects only for mortality rates. Areas that experienced an increase in distance to their nearest legal aid provider at any point between 2013 and 2019 show significant and deepening effects in terms of increased mortality rates, compared to areas that did not experience any change. However, the effects previously observed on eviction court case prevalence and outcomes are not statistically significant. We explain this discrepancy with two factors. First, distance to a provider is likely to matter only after a certain threshold; small changes in distance may be negligible in affecting court outcomes. Second, areas most affected early

in the reform period may have required more time to adjust to the new landscape. Over time, one would expect actors to adjust more quickly to shifting levels of provisions, raising reservations about grouping together areas that experienced any change in distance regardless of timing or intensity.

This paper explores two mechanisms. First, legal aid professionals often serve as navigators of the welfare state, either directly or by teaming up with paralegals and social workers. They identify client needs critical to both legal resolution and welfare, such as unclaimed benefits or untreated mental health conditions, which can activate legal protection clauses and strengthen legal arguments. By restricting legal aid funding to narrowly defined cases, the reform limited lawyers' ability to address these interconnected issues. For instance, while legal representation in eviction proceedings remained covered, assistance with the underlying benefits claims that led to rent arrears was no longer funded. To test this mechanism, in a side paper, we link administrative welfare records to legal assistance data in a pilot area, tracking clients' financial outcomes over two years and surveying them about how legal support shaped their access to benefits, housing, and other non-legal services (Uraz (2025)).

Second, the reform distorted the legal aid provider market by crowding out non-profits and generalist firms that worked across multiple legal domains. While the policy aimed to consolidate the sector, it hollowed out the legal aid landscape, weakening the supply of in-person legal help provided within hubs of legal services. These hubs functioned like a "hospital of legal aid" or a one-stop-shop, with several teams of lawyers specialising in different areas of law hosted under the same umbrella organisation. By contrast, surviving providers tended to specialize in only a few areas of law, making them ill-equipped to assist clients with complex, multi-faceted problems, such as eviction intertwined with domestic violence or immigration issues, legal domains requiring different legal practices and expertise.

These structural market shifts undermined the integrative function that generalist hubs previously served, including referrals across the legal and social support system. To test this mechanism, we classify providers based on their pre-reform portfolio as either generalist hubs of service or isolated specialists across all domains of civil law. While

our main analysis focuses on housing, welfare, and debt legal aid provision, we expand this classification to include family and civil law to capture the full scope of providers' portfolios. We then spatially map the overlap of areas of legal aid expertise to measure the cumulative provision across legal domains, and explore the heterogeneity of treatment effects based on the type of the nearest provider at baseline.

Our findings contribute to several strands of literature. This paper provides the first national-scale evidence outside the U.S. of a *negative* supply shock in legal assistance provision. Unlike existing studies that focus on introducing legal aid or examining schemes tailored to single case types such as eviction proceedings (Cassidy and Curie (2022)), disability claims (Hoynes et al. (2022)), or foster care (Cooper et al. (2025)), we offer a comprehensive perspective on the effects of simultaneously removing legal assistance across multiple legal domains of civil law. This study quantifies the multifaceted impacts of both transforming legal aid into a last-resort scheme and weakening hubs of legal services, with direct policy relevance for the design of legal aid programs. By evaluating legal aid as a social policy, we further examine how withdrawing legal assistance amplifies the impacts of concurrent austerity measures on homelessness, health outcomes, and mortality.

Our study addresses the limited evidence on the welfare costs and benefits of legal assistance programs from a value-for-money perspective (Abel and Vignola (2010), Stout Risius Ross, Inc. (2016)). While previous US-based welfare analyses by Abramson (2022) and van Dijk et al. (2024) focus on frictional costs in the housing market, our analysis extends this framework to examine broader welfare impacts, including homelessness and health outcomes, with mortality as an extreme manifestation of these costs.

Our research also contributes to the growing literature on legal counsel's effects in housing disputes. This literature, primarily US-centered, has documented the impacts of intensive forms of legal assistance, such as legal representation, in preventing eviction orders. Early randomized controlled trials in NYC (Seron et al. (2001)) and Massachusetts courts (Greiner and Pattanayak (2011), Greiner et al. (2013)) demonstrated that personalised legal assistance improved court outcomes compared to generic information provision. Subsequent evaluations of NYC's right-to-counsel initiative (Ellen et al. (2021),

Cassidy and Curie (2022)) found that legal representation reduced evictions and monetary judgments. To our knowledge, the only other quasi-experimental study taking a broader perspective by Buenaventura and Heaton (2025) shows that households near income eligibility cut-offs for federally funded legal aid demonstrate higher housing stability, especially for seniors.

Our findings align with this pattern: the areas most exposed to legal aid cuts have higher shares of pensioners, the demographic most affected in our mortality analysis. By examining mortality effects, we connect to the sociolegal and public health literatures linking unmet legal needs, the experience of not resolving a legal problem, to health status. Studies of medico-legal partnerships have evidenced how legal assistance integrated within healthcare systems can improve physical and mental health outcomes for vulnerable households facing accumulating legal problems (Tobin-Tyler et al. (2011)). International comparisons have stressed the importance of collaborations between healthcare providers and legal advisors to reach health equity (Tobin-Tyler et al. (2023)). While Genn (2019) has theorized the bidirectional relationship between unresolved legal problem and deteriorating health, Leifheit et al. (2024) have recently shown that living in areas eligible for right-to-counsel reduced adverse birth outcomes among Medicaid-insured parents in NYC, underscoring the importance of breaking down the analysis by demographic subgroups.

Finally, our findings speak to broader questions about legal aid as social infrastructure and its role in overcoming barriers to benefits take-up. Joint models of legal provision and benefits support are particularly common for housing cases (Newton et al. (2020)) and are found to be more effective when integrated with complementary services delivered by both lawyers and non-lawyers (Sandefur and Clarke (2017)). Caspi and Rafkin (2024) shows that legal representation for eviction may only be effective when paired with rental assistance, with elicitation surveys revealing that beneficiaries attribute a substantial stress-reduction benefit to the legal support itself. This aligns with our mortality findings and parallels documented impacts of rental assistance on self-rated health (Keene et al. (2020)), mental health (Denary et al. (2021)), and hemoglobin levels (Fenelon et al. (2022)), a biomarker of stress-related physiological strain.

This evidence contributes to a wider view that access to justice programs, including legal assistance schemes, can generate long-run impacts on welfare participation and the effectiveness of social protection, as shown in the US (Cunningham and Goodman-Bacon (2025)), and suggested in developing country settings (Britto et al. (2025)). Our findings also connect to studies highlighting the critical importance of personalised assistance in navigating housing search (Bergman et al. (2025)) and in claiming benefits beyond the provision of information alone (Castell et al. (2025)), particularly for older adults (Finkelstein and Notowidigo (2019)). Together, this literature underscores the need to understand how legal aid lawyers complement existing navigators, such as social workers and counsellors, in helping individuals access entitlements and maintain basic welfare. This perspective reframes the role of legal assistance within the poverty-eviction cycle documented by Desmond (2016) and quantified by Collinson et al. (2024), while also revealing how the withdrawal of legal aid can amplify austerity's documented effects on voting behaviour (Fetzer (2019)), homelessness (Fetzer et al. (2023)), and mortality (Berman and Hovland (2024)).

This paper is structured as follows. Section 2 provides the institutional context behind the provision of legal aid services in England and Wales and the changes introduced by the 2012 reform. Section 3 presents the set of data used and the computation procedure to dynamically map the activity of legal aid services across the country. Section 4 presents the different empirical strategies that successively exploit the spatial and time variation triggered by the reform in terms of access to local assistance. Section 5 presents the findings that are discussed in section 6. Section 7 concludes.

2 Institutional Context

2.1 The provision of legal aid for social welfare cases in England and Wales

In England and Wales, legal aid is a funding scheme provided by the central government to cover the costs of legal services for eligible households. To qualify for assistance, the legal problem must fall within the scheme's scope. The specific nature of the problem determines the *type* of assistance the household can receive, between legal help and/or legal representation, and the *timing* at which legal aid is available. Legal help encompasses advice and assistance about a legal problem but does not include representation or advocacy in proceedings. Legal representation entails representation by lawyers in court.

The legal aid reform, commonly referred to as LASPO [Legal Aid, Sentencing and Punishment of Offenders Act], affected both the scope of cases eligible for assistance and the timing at which legal services would begin to be subsidized. While some legal problems remained within scope, such as eviction cases, free legal assistance would no longer be available until an eviction threat materialized into a court action. For instance, before the reform, households facing rent arrears could receive subsidized legal help before being instructed to vacate their homes. In such cases, tenants were at an early stage: not yet facing a court hearing, often without any formal eviction filing, and not requiring legal representation.

After the reform, the same household would have had to wait until the formal eviction process commenced before qualifying for legal aid, meaning once their landlord had filed a claim in court, initiating the formal process and starting the notice period of a minimum two months. Similarly, while an eviction case may originate from a benefit entitlement issue, either for rental assistance or for non-housing-related support, the household would no longer be able to access benefit advice now outside the scope of legal aid, preventing them from addressing the underlying causes of the eviction through the legal aid scheme.

The exact content of legal help sessions pre-reform remains a black box. Statistics from

the Ministry of Justice give some indications on the types of issues dealt with, but there are no details of the types of actions undertaken by lawyers in these sessions. Qualitative works, such as Moorhead and Robinson (2006) and Smith et al. (2013), suggest that the advisor could help clients with paperwork, correspondence with other parties such as their landlord, or refer them to other social care services. This assistance could involve negotiating repayment plans with landlords, helping with benefits claims, or addressing issues like disrepair that contributed to arrears.

Overall, the reform primarily removed funding streams for early legal advice. Legal assistance was no longer available for confronting a wide range of everyday legal issues, from housing disrepair to benefit entitlements. Instead, it became a last-resort service only accessible for cases involving court actions and posing a direct threat to household security. Two main policy justifications underpinned the funding cuts beyond generating savings. The reform arguably aimed at "discouraging unnecessary litigation" and "targeting the scheme to those most in need" (Ministry of Justice of the United Kingdom (2019)). The underlying rationale was that subsidizing lawyer fees was creating misplaced incentives for both the represented party and the lawyer to seek formal resolution of disputes through the court system instead of prioritising cheaper, alternative dispute resolution and mediation.

This shift dramatically changed the type of assistance effectively covered by the scheme. Figure 1 shows how it translated into the number of legal help cases being reduced by fivefold. In Appendix, Figure 69 illustrates the dramatic restructuring of the scheme across areas of law between baseline (2009-10) and the first financial year post-implementation (2013-14). While the areas of "Housing" (primarily eviction cases), "Family," and "Mental Health" were partially preserved, the poverty law areas of "Debt" and "Welfare Benefits" virtually disappeared from legal aid provision, reduced by a factor of over 10.Provision for "Immigration" legal aid, which also faced dramatic cuts, falls outside the direct scope of this study but has been extensively documented by Wilding (2021).

2.2 Austerity cuts and interruption of legal aid services

The legal aid reform was uniformly implemented and acted as a supply shock. The cuts surpassed expectations and amounted to a £300 million spending reduction per year, approximately a 30% reduction in the total civil legal aid budget of 2011-2012 (National Audit Office (2014), National Audit Office (2024)). This represented an 80% reduction in funding for cases in social welfare laws (Figure 2). While all areas in England and Wales were affected by the reform at the same time, some areas ended up being more or less affected in terms of the interruption they experienced in their legal aid services. Figure 3 shows that the number of providers taking on legal aid work was halved between 2011 and 2019. Already weakened by a low level of legal aid fees, many providers either stopped taking on legal aid cases or ceased their work altogether. The English and Welsh legal aid system had undergone several cost-containing reforms since the 1999 Access to Justice Act, with a staggered introduction of fixed-fee scheme (in 2004, 2007 and 2010). Civil legal aid fees had also been frozen since 1996 and further reduced by %10 in 2010. The LASPO Act however stands out over a longer-term and comparative perspective by its scope and magnitude.

In addition, as time passed, the legal aid reform increased the workload of providers that remained active to an unprecedented level due to the displacement of unmet demand. Evidence suggests that the reform created a snowball effect of closures and congestion across the country, which translated into a spatially uneven inability to meet the demand for services over time (for a case study see: Organ and Sigafoos (2017); for a review see: House of Commons Justice Committee (2015), The Law Society (2017), The Law Society (2021)). Local access to legal assistance was thus restricted differently depending on the resilience of existing providers. Figure 4 shows the steady increase in distance to the nearest legal aid provider over time, with two acceleration periods: from Q3 2011 to Q1 2013, and from Q1 2013 to Q2 2014.

Legal aid had historically been provided by two types of providers with different business models: solicitors and not-for-profit organisations. Solicitors derive funding from two sources: their private (chargeable) clients and legal aid-subsidized clients. They

can also take on cases for free, outside of legal aid schemes, a practice known as *pro bono* work. However, evidence suggests that *pro bono* work tends to concentrate on areas of law outside the scope of this study. For instance, according to the Trust Law Index, less than 15% of *pro bono* work focused on housing disputes over the past 10 years[1]. Moreover, *pro bono* work on social welfare problems remains marginal outside of not-for-profit structures. In this context, they are a limited substitute for legal aid.

On the other hand, not-for-profit organisations offer legal services on a charitable basis, including outside of the legal aid scheme. Typically, they used to take on legal help cases and then refer clients eligible for legal aid to solicitors for further assistance and representation. However, since the reform severely reduced the number of legal help cases eligible for legal aid, most organisations had to scale back their services and cease offering legal aid work. Figure **??** shows the distribution of legal aid work between solicitors and not-for-profits. Not-for-profit includes national networks covering broad ranges of legal problems, such as Citizens Advice bureaux or law centres, as well as more specialised units focused on specific areas like homelessness prevention.

Not-for-profit organisations rely on funding from various sources, including funding from local authorities, governmental grants and private donors, and legal aid. Therefore, even if they nearly stopped receiving legal aid funding after LASPO, many organisations may have remained active in providing alternative forms of legal support. In the context of this paper, we consider the work not-for-profit organisations continue to provide after the reform as an imperfect substitute for the personalized, intensive form of assistance that was previously available through legal aid. We are currently mapping the activity of not-for-profit organisations to better understand their role and impact in the post-reform landscape. Whenever this data is available, we will control for the location of not-for-profit providers in our empirical strategy.

[1]See the Thomson Reuters Foundation Trust Law Index available online at: pbi.trust.org/analysis hyperlink accessed February 12, 2024

3 Data and Descriptive Evidence

This section describes the data mobilized for this paper and the construction of the market-level measures, including the Bartik shift-share instrument.

3.1 Legal Aid Providers Activity in England and Wales

3.1.1 Mapping distance to the nearest legal aid provider

To measure changes in access to legal assistance, we begin by calculating the distance to the nearest legal aid provider over time. We use the Ministry of Justice's legal aid statistics, which report the number of cases opened and completed each quarter by providers contracted to deliver legal aid. This open-source dataset, released annually, covers providers that have undertaken legal aid work at some point since 2009. However, it does not represent the full universe of organisations offering legal support. In particular, it excludes not-for-profit organisations that have not engaged in legal aid work since 2009, even if they have continued to provide other forms of legal services to low-income households outside of the government scheme. Additionally, it omits organisations that stopped providing legal aid after the implementation of LASPO but remained active *pro bono*, meaning without state subsidies.

At the time of writing, no comprehensive time series exist for the activities of legal advice providers operating outside the legal aid scheme. Existing literature (The Law Society (2017); Wilding (2021)) relies exclusively on the Ministry of Justice dataset and is therefore limited to providers within legal aid. To address this gap, we are currently constructing a new database to track the locations and operational periods of all organisations delivering legal support for housing, debt and benefits disputes to low-income households. This effort draws on data from the Charity Commission, Companies House, and civil society actors, and focuses on identifying advice providers that meet recognised quality standards.

As a first step, we acknowledge this limitation but proceed under the assumption that providers not engaged in legal aid work are imperfect substitutes for the type of

assistance previously available under the legal aid scheme—namely, personalised, high-intensity legal support delivered by paid professionals. This contrasts with more generic, lower-intensity forms of advice typically offered by volunteers or paralegals in the charity sector. To further mitigate this concern, we test the robustness of our findings at the local authority level (see Section 4).

The Ministry of Justice legal aid statistics include information on the postcodes of legal aid providers and the areas of law in which they are contracted to deliver services. We geolocate all legal aid providers in England and Wales and assign them to Middle Layer Super Output Areas (MSOAs) using 2021 Census boundaries—the only scale at which court outcome data are available. When working with data based on 2001 or 2011 Census boundaries, we use the Office for National Statistics' official lookup files to reconcile spatial units. In cases where several boundaries overlap (e.g., multiple 2021 MSOAs correspond to a single 2011 MSOA), we take the average value across the overlapping units. There are 7,264 MSOAs in England and Wales under the 2021 Census and 7,201 under the 2011 boundaries.

For each provider, we define the point of exit from legal aid work as the last quarter in which they take on any new legal aid cases. A provider is considered inactive from time t onward if no new cases are recorded in any subsequent quarter. Our main sample is restricted to providers handling at least one case in any of the following areas of **social welfare law**: housing, debt, or welfare benefits. In the Appendix, we explore alternative definitions, including providers that conduct at least one case in any **non-family law** area, and providers that handle **housing law** cases specifically. Providers classified under a given area of law (e.g. housing) may also cover other areas of law, but will have undertaken at least one housing case while active. The non-family law category includes: housing, debt, welfare benefits, clinical negligence, education, consumer, community care, personal injury, public law, mental health, employment, and discrimination. As shown in Table 1, the treatment variables defined in Section 4 are especially sensitive to how broadly legal aid provision is defined. We further explore this portfolio dimension within the context of the Bartik shift-share instrument.

To measure distance, we use the *st_distance* function from the *sf* R package, which

calculates straight-line (Euclidean) distances between points in Cartesian space. Specifically, we compute the distance between each MSOA's population-weighted centroid and the nearest legal aid provider. We allow the closest provider to lie outside the MSOA but require to be located within the same local authority, to reflect residency rules and local dynamics of service provision. If no provider is active within the same local authority, we default to the nearest provider regardless of administrative boundaries. As a robustness check, we plan to implement the *georoute* module developed by Weber and Peclat (2016), which estimates road distance and travel time between two addresses and control for the ONS rural-urban classification using 2011 Census boundaries.

3.1.2 Mapping overall provision of legal aid across areas of law

Using changes in distance as a proxy for changes in access to legal aid poses several challenges. One key limitation is that access for a specific legal problem is conditional on the provider's specialisation. Beneficiaries can only receive appropriate assistance if a nearby provider is both qualified and contracted by the Ministry of Justice to deliver services in the relevant area of law. At the same time, providers specialising in areas of law that were most affected by the reform are likely to have experienced more substantial operational impacts. As discussed in Section 2.2, the structure and funding models of legal aid providers differ substantially. Private firms often operate with mixed client portfolios —balancing legal aid and privately funded work, whereas not-for-profit organisations typically rely on a combination of government, local authority, and philanthropic funding for services not covered by legal aid. These structural differences are likely to produce **heterogeneous** responses to the reform.

Overall, we expect providers' responses to the reform to vary along several dimensions: their *legal structure* (private firm vs. not-for-profit), *size* and *geographical outreach* (e.g., single-office vs. multi-branch firms), *degree of specialisation* (generalist vs. specialist), and their *baseline exposure to legal aid* (measured by volume of cases and revenue share). These characteristics shape both their vulnerability to legal aid cuts and their strategic capacity to adapt. Providers may respond at both the **intensive** and **extensive** margins. At the intensive margin, organisations might adjust the scope or focus of their services —for

instance, by diversifying into new areas of law, consolidating services by closing some offices, or expanding to new locations. At the extensive margin, they may exit the market altogether, remain but scale back services, or even enter the market if new opportunities arise under the revised funding environment.

To better understand how the legal aid landscape responded to these reforms, and how this reshaping may have driven heterogeneous effects across different areas, we present descriptive evidence on sectoral trends. Our analysis confirms that many providers anticipated the reform and exited the market prior to the LASPO Act's implementation. However, we also observe that some firms entered the market during this period, contributing to a substantial reshuffling to the spatial distribution of legal aid provision. While a stock analysis reveals a general decline in the number of active providers (Figure 45) and a gradual increase in average distance to the nearest provider (Figure 4), a more granular analysis shows that some areas actually experienced a decline in distance due to high turnover, encouraged in part by the competitive tendering process for legal aid contracts (Figure 45).

Two parallel trends are particularly noteworthy. First, the sector experienced growing **specialisation**, with an increasing share of firms focusing on fewer areas of law post-reform (Figures 5). While over half of providers covered four or more areas of law in 2009-10, only 25% did so in 2013-14. Second, we observe a trend toward **consolidation**, with more firms operating multiple offices. In 2009–10, over 60% of firms had a single office; by 2013–14, more than 50% had two or more (Figure 6). Meanwhile, the largest providers reduced their footprint: while some firms operated more than 30 offices in 2009–10, none did so by 2013–14 (see Appendix Figures 65 and 66 for the distribution).

These structural shifts motivate the construction of a second measure of access at the MSOA level. While we observe the number of cases at the provider level, we do not have data on the geographic distribution of legal aid beneficiaries, and there are no strictly enforced catchment areas to assign MSOAs to a set of providers. To address this, we proxy the flows of legal aid services into each MSOA using a gravity model framework, following the standard assumption that service flows decay with distance.

For each MSOA, we calculate the distance from its population-weighted centroid to

each provider p operating in England and Wales. For each provider, we take the volume of legal aid work in a given area of law a at time t in terms of number of cases opened during that period: $Volume^{a}_{p,t}$. To create our measure of flows between an MSOA and a provider p, we construct the inverse-distance-weighted product of the provider capacity, $Volume^{a}_{p,t}$ with the size of the population in each MSOA, $Population_{MSOA,t}$, which serves as a proxy for legal need or "market size". We then sum over all providers, with the inverse of distance giving less weight to providers that are further away from the MSOA centroid.

Compared to the distance to the nearest provider, this metric adjusts for spatial clustering of providers, particularly in metropolitan areas, and allows us to rank MSOAs by their relative level of access (or distance-weighted provision per capita) in one or several areas of law a. In our instrumental variable strategy, we use the flow measure described here not only as a first-stage outcome to test the relevance of our Bartik-style instruments, but also as an indicator of market access to map out spatial inequality in provision. All our measures are scaled per 100,000 inhabitants.

$$Access^{a}_{MSOA,t} = \sum_{p} Access^{a}_{MSOA,p,t} = \sum_{p} \frac{1}{Distance_{MSOA,p}} \times Volume^{a}_{p} \times \frac{1}{Population_{MSOA,t}}$$

To illustrate the spatial inequality in legal aid provision that existed even before the reform, Figure 52 maps the cumulative distance-weighted provision across all areas of law (left panel) and specifically for welfare law areas—housing, debt, and welfare benefits (right panel). The stark geographic disparities are evident: while London and other major urban centers show high provision levels (top 20th percentile), vast swaths of rural England and Wales fall into the bottom quintile of provision. This pattern is even more pronounced for welfare law provision, where the concentration in urban areas leaves rural communities with minimal access.

In the Appendix, Figure 54 quantifies these disparities more precisely. Using k-means clustering, we classify MSOAs into five tiers based based on their baseline provision levels across all 15 areas of law. The heatmap reveals dramatic gaps in provision: the bottom

tier have 2.5 times less overall provision than the top tier, but the disparities are far more extreme for specialized services—7.9 times less for mental health services, 5.8 times less for housing assistance, and 4.8 times less for employment law. Even areas classified as "Mixed Access," representing nearly 40% of all MSOAs, have approximately half the provision levels of the top tier across most services. These pre-existing inequalities meant that when the reform hit, already underserved areas had minimal buffers against further service reductions, while well-served urban areas could potentially absorb some of the shock through provider consolidation and adaptation. The Appendix section E.3 provides additional evidence on the spatial inequality of provision.

3.1.3 Bartik Instrument

Using area-specific predictions of the fall in volume, along with the provider-level data on case volumes and revenues from legal aid, we compute a shift-share instrument as described in Section 4.

- *Shock*

  Prior to the reform, the Ministry of Justice conducted an Equality Impact Assessment (EIA) estimating the expected reduction in legal aid workload and income across areas of law and provider types (Ministry of Justice of the United Kingdom (2011)). Table E.2.2 in the Appendix reports the estimated percentage impact by area of law, as outlined in the EIA. To build the instrument, we use the predicted shocks to volume because shocks to income were, in practice, derived from these estimates.

- *Share*

  To create our share measures, we take two metrics at baseline: the provider reliance on legal aid funding for that area of law and its level of specialisation. To measure the dependence of providers on the legal aid income stream and to account for the gaps between billing and opening of cases, we average the total funding received by a provider as part of the legal aid scheme for cases handled in that area of law over the two financial years pre-reform 2008-09 and 2009-10.

  For providers with missing billing information, we impute their average baseline

funding stream using the volume of cases opened in 2009-10 and the average cost per case computed across all firms for that specific area of law and specific scheme (Civil Representation vs. Legal Help) between 2008-09 and 2009-10. For the specialisation factor, we take the share of cases handled in 2009-10 in that area of law relative to the total number of legal aid cases handled in that financial year across all areas of law.

3.2 Court Outcomes

We consider the occurrences of eviction and debt court cases as proxies to measure breaking points, or breakdowns, from the perspective of households facing legal problems. Breakdown is defined as a condition that prevents an individual from living, working and consuming as they usually would, exposing them to the risk of falling into a more precarious socioeconomic situation than that experienced before the so-called breaking point. Our first outcome is the number of court cases per inhabitant in a given area, and we look at eviction proceedings and over-indebtedness judgments. We consider these court actions as a sign of a crisis and interpret a higher number of cases in an area as a higher prevalence of potential breakdowns among residents. The implicit assumption is that access to timely legal advice could have either helped settle the dispute outside of court or disincentivised the other party to bring the matter to court.

Our second outcome is the population-weighted number of unfavourable court decisions for the above-mentioned cases, ie. an eviction order and a debt judgment forthwith. We consider obtaining an unfavourable court ruling as another sign of breakdown, as such rulings are likely to affect the financial resilience of households. An eviction order can trigger a forced relocation outside the borough where a household has its professional and support network and lead to homelessness. A debt judgment without installment is more difficult to settle on time and can lead to a listing in a public directory, which deteriorates the household's credit score and ability to take out new loans. Therefore, we interpret a higher proportion of adverse court decisions as a sign of a higher risk of acute vulnerability among residents.

Both our outcomes are scaled per 100,000 inhabitants. When defining an outcome as favourable, we implicitly assume that the beneficiary of legal aid would be the tenant and exclude from our sample mortgages possessions proceedings.

Eviction proceedings: Eviction data were obtained from the Ministry of Justice through Freedom of Information requests. To measure local prevalence, we take the number of possession claims. To measure the share of unfavourable outcomes, we use data on the number of eviction orders that can be decomposed between Suspended Possession Orders and Outright Possession Orders.

The flowchart in Appendix 49 illustrates the sequence of an eviction case in England. A claim is a formal filing made by the homeowner against the tenant to instruct them to leave the property. At a court hearing, a judge decides whether an eviction order is issued based on this claim. The case can also be adjourned or dismissed, but this data is not currently available. There are two types of eviction orders: outright orders which take effect in the immediate aftermath and suspended possession orders where the order is conditional on the tenant not satisfying a list of conditions set out at the judgment. A suspended possession order is a better outcome from the tenant's perspective than an outright order.

The data is at the MSOA level, based on 2021 Census boundaries, every quarter between 2000 and 2019. Table 16 reports the summary statistics of eviction proceedings action over time.

Debt judgments: Debt judgments data were accessed via the County Court Judgments database of the Consumer Data Research Center. To measure local prevalence, we take the number of personal debt judgments. To measure the share of favourable outcomes, we compute the ratio of the number of judgments with installments over the total number of judgments. The data is available at the MSOA level every year between 2001 and 2019. Debt data includes claims up to £100,000, as well as the incidence of cases for claims below £250, £500 and £1000. [2]

[2]Forthcoming

3.3 Mortality Outcomes

Mortality rate: Mortality data from the Mortality Statistics for England and Wales supplies yearly death counts by age and sex at the Lower Super Output Area (LSOA) level for analysis spanning 2000 to 2021, based on the 2011 Census. We compute the age-adjusted mortality rate per 100,000 inhabitants at the MSOA level.

Avoidable mortality: [3] In addition, cause of death data and avoidable mortality data from the Office for National Statistics (ONS) capture preventable and treatable deaths, providing a supplementary health metric at the MSOA level. This includes the number of deaths and age-standardised avoidable, treatable and preventable mortality rates by sex. Figures on cause of death are available yearly at the MSOA level, those on avoidable death are aggregated over a five-year calendar period, between 2008 and 2012, and between 2013 and 2017. Note that the latter estimates are missing for Wales.

3.4 Healthcare services Outcomes

Attendance and Emergency weekly admissions:[4] This project will next look at attendance and emergency weekly admissions in the 316 emergency services of England (data available from 2010).

Bed hospitals occupancy: This project will next look at the occupancy of hospital beds at the 204 National Health Service organisations level (data available from 2000).

3.5 Housing Market Tensions Indices

The following indices reflect housing market tensions, capturing both housing affordability and the risk of homelessness.

Housing affordability: Housing affordability is proxied by houses prices related to the share of low-income households in the area. Average yearly house prices by MSOA are sourced from the House Price Statistics for Small Areas. This project aims to next

[3] Forthcoming
[4] Forthcoming

include rental transactions data from property search website. [5]

Homelessness risk: To capture homelessness risk, this project relies on the comprehensive Statutory Homelessness statistics reported by local authorities to the central government. This data is available only for the 348 local authorities of England and includes annual counts of households on local authority housing waiting lists based on Table 600, counts of households assessed for homelessness relief or prevention and those in temporary accommodations.

3.6 What are the areas that lose access to legal aid

Table 2 reports the summary statistics between the treated and control areas, using our preferred static DiD specification comparing areas with the highest change in distance to the nearest provider between Q2 2012 and Q1 2014 with areas with no change in distance. Using the 2011 Census and open-source data available at the MSOA level, we draw an extensive comparison of covariates at baseline across several broad categories: demographics, ethnicity, education, employment, housing characteristics, language proficiency, car ownership, family structure, deprivation indices, health and disability, migration patterns, benefits and poverty measures, economic conditions, mortality, housing stress, financial distress, eviction proceedings, and legal aid provision.

To examine whether observed differences in sociodemographic composition and income levels between treated and control areas persist across different geographic settings, we stratify the analysis using the ONS Rural-Urban Classification. We compare balance across three geographic clusters: rural areas (including rural towns, villages, and dispersed settlements), urban areas (towns and cities), and major cities (conurbations).

Across all geographic settings, treated areas consistently exhibit three key characteristics: they have significantly older populations, are substantially less ethnically diverse, and are more affluent overall. Specifically, treated areas have a higher share of pensioners aged above 65 (18.8% versus 15.9%). They are also substantially less ethnically diverse, with 93% white residents in treated areas—a 10 percentage point difference. This reflects

[5] Forthcoming

lower migration patterns: the share of households for whom English is not the first language is half that of control areas (4% versus 8%), and the share of UK-born residents is 8 percentage points higher, reaching 92%. Greater affluence is reflected in higher car ownership (though this difference is insignificant within rural areas), higher net income before housing costs, and a lower share of people below the poverty line—except in rural areas, where the pattern reverses. Regarding housing conditions, differences are primarily driven by disparities within cities, where treated areas exhibit better conditions with lower overcrowding rates, a higher share of owner-occupied housing, and lower baseline eviction rates.

Two patterns stand out as particularly striking. First, despite greater overall affluence, financial distress indicators show much higher bankruptcy and insolvency rates in treated local authorities regardless of geographic clustering. Similarly, fuel poverty rates are significantly higher in treated areas (8,250 versus 7,161 per 100,000). These areas also score worse on the Index of Multiple Deprivation's geographic barriers domain, indicating systematically poorer access to essential services such as GP surgeries, primary schools, post offices, and supermarkets, a pattern that points to hidden vulnerabilities within ostensibly affluent communities. Second, baseline legal aid provision is substantially lower across all areas of law in treated areas, suggesting these areas were already approaching underserved "legal deserts" and that the reform may have precipitated complete withdrawal by closing the only nearby provider.

4 Empirical Strategy

Our identification strategy exploits the legal aid reform as a quasi-experimental source of variation in the provision of legal aid services with both a spatial intensity and a time component. We adopt several empirical strategies to exploit each source of variation separately.

4.1 Spatial Source of Variation: Static Treatment

4.1.1 Design

First, we exploit the *spatial intensity* of variation in access, defining areas that experienced little to no change in access after the reform as control groups and those where provision was most reduced as treated groups. We follow Lindo et al. (2020) and restrict the timeline to measure the change in access experienced in the two years after the reform. Our target causal parameter is a relative average treatment effect on the treated (ATT): the average treatment effect of a negative shock in legal assistance provision on areas that were the most affected compared to areas that were the least affected between 2012 and 2014. We implement this strategy with a Two-Way Fixed Effects dynamic procedure and benchmark our estimates with heterogeneity-robust estimators following Callaway and Sant'Anna (2021).

In this strategy, our treatment definition is **static**: areas are treated or untreated based on the relative change in the distance they experienced over a given time frame. To classify areas between most affected and least affected, we first use the quintile distribution of the treatment when measuring the change in distance between Q2 2012 and Q1 2014. Table 3 shows that the first quintile is the most significantly affected by the reform. In that group, the change in distance is an average of 13.7 km. Since some areas experienced a decrease in distance, we exclude them from our sample and, instead, define untreated areas as areas that did not experience any change in distance between Q2 2012 and Q1 2014. We will further investigate how to better account for areas that experience a decrease in distance in our Shift-Share IV approach. Table 4 shows the distribution of the change in distance variable among our favourite specification of treatment intensity. Table 21 and 22 show the distribution of the change in distance variable when considering an alternative sample of legal aid providers.

Alternative time windows could have been chosen to measure changes in distance. Figure 4 suggests that one could take Q3 2011-Q1 2013 and Q1 2013-Q2 2014 to capture the periods where distance increases at the most sustained pace. In the Appendix, we

test the sensitivity of our results to these alternative definitions of the treatment [6]. The reason for choosing Q2 2012 – Q1 2014 is to capture the period from the date the reform received Royal Assent — when the Bill had passed all stages of parliamentary discussion and its final version became known to the public — until a full financial year after its implementation. The reform came into force on April 1, 2013, and in the UK, the financial year runs until March 31. This approach helps account for both anticipatory behaviors and the non-immediate adjustments of legal aid providers to the post-reform landscape.

We leverage the data granularity to exploit two levels of spatial variation. We conduct the analysis at the level of the Middle Layer Super Output Areas (MSOA) and cluster it at the level of local authorities. We use MSOAs because this is the smallest census unit available for our outcomes, MSOAs are constructed to have on average 8,000 residents. The homogeneity of MSOAs increases the relevance of grouping areas with similar changes in access in terms of congestion of services. We cluster the analysis at the level of local authorities and further add local-authority fixed effects in a refined specification since local authorities administrative borders hold significance for the local provision of advice outside of the legal aid scheme. Some not-for-profit providers that operate outside of legal aid employ a residency test at the local authority level to screen households eligible for their services. Some also receive supplementary funding from local authorities, which affects their resilience to the reform.

4.1.2 Estimation procedure

First, we implement our Difference-in-Differences design with an event study, using a dynamic two-way fixed effects (TWFE) regression clustered at the level of local authorities. The simplest form of the estimating equation is as follows, with i the observation measured at the MSOA level, g the respective local authority and, t the time period covering thirteen years before the reform and six year after, meaning from 2000 to 2019. We stop the analysis in 2019 because new funding for legal aid was introduced at that time. Since we encounter areas with zero court cases, we operationalise the estimation of the TWFE

[6] Forthcoming

with a Poisson model.

$$\text{Equation (1): } Y_{ig,t} = \sum_{q=t}^{T} \mu_q Post_q + \sum_{q=t}^{T} \beta_q^{TWFE}(Access_{ig,q} \times Post_q) + \theta_t + \delta_i + \epsilon_{ig,t}$$

$$\text{for } q = t, ..., T \text{ and } t < \text{Reform}, \; Post_q = 0$$

$$\text{for } q = t, ..., T \text{ and } t > \text{Reform, with } Access_{ig,q} = D_{ig} \times Post_q$$

$$\text{with } Post_q \text{ a set of time indicators that equal to 1 if } q = t, \text{ 0 otherwise}$$

$Y_{ig,t}$ denotes the population-weighted outcomes measured at the MSOA level i of the local authority g in time t.

μ_q captures the effect of time passing on the outcomes, meaning any changes in the outcomes that are unrelated to the treatment but that may be associated with the given time period. It provides a baseline estimate that can be compared to the coefficient β_q^{TWFE} which captures the effect of the treatment in time period q.

$Access_{ig,q}$ captures the treatment: the change in access to legal services in the area i, g at time q. This can be decomposed into D_{ig}, the treatment indicator, and $Post_q$, a time dummy that switches on and off for each year following the reform. D_{ig} measures the change in distance to the nearest provider within the two years following the reform's adoption. It can either be a continuous variable or a binary variable, depending on whether we are considering the effect of a marginal increase in one unit in distance, or comparing effects between the most affected areas (quintile of the distribution) and the least affected areas (areas with no change in distance).

θ_t are time-fixed effects and δ_i are MSOA fixed effects. To rule out concerns about within-local authority serial correlation and overdispersion, we cluster our error terms $\epsilon_{i,g,t}$ at the local authority g level (Wooldridge, 1999).

Our setting, akin to a conventional difference-in-differences model, requires the parallel trend assumption to hold. We discuss the plausibility of this assumption in the next sub-section 4.4.2.

4.2 Overtime Source of Variation: Staggered Treatment

4.2.1 Design

Second, we exploit the *overtime* variation in access triggered by the reform and adopt a difference-in-differences approach with a staggered definition of my treatment. Even if the 2013 reform was not staggered in its implementation, its effects on the evolution of access had a dynamic component: the number of legal aid providers kept falling steadily at the national level. As a consequence, we expect the effects of the reform to be heterogeneous over time. The further one moves away from the reform, the more new local substitutes for legal aid may appear, triggering potentially different effects of the reform on the set of outcomes over time. Accounting for the recent development of the literature that casts doubts on the reliability of Two-Way Fixed Effect estimators when treatment effects are heterogeneous across units and periods (for a review see Roth et al. (2023)), we compute imputation-based estimators following Borusyak et al. (2021).

In this setting, we define treated units as areas which experience an increase in distance to their nearest legal aid provider *at any point of time* after the reform. Compared to the first approach, this empirical strategy discards the magnitude of the change in distance experienced and instead considers any positive, non-null change in distance as treated.[7] Figure 80 shows the geographical distribution of treatment status across England and Wales. The analysis reveals the need to have a more complex definition of the treatment status. 1,132 out of 7,264 MSOAs experience several increases in the distance to their nearest legal aid provider over the period Q2 2013 - Q4 2019, compared to the baseline distance measured in Q1 2013. These areas are classified as "Treated several times" on the map. A few, 332 out of 7,264 MSOAs, experience one or several decreases in distance. 1,367 others experience alternatively at least one increase and one decrease in the distance, we classify them as "Switchers.

[7]For instance, areas that were classified as "Small Change" and were not considered as "Treated" in the previous strategy would be classified as "Treated" in this one. We are currently exploring new avenues to incorporate the intensity of the treatment in the staggered definition. The literature is not conclusive regarding the best way to do so. At the moment, we are considering (i) using a continuous definition of the treatment, taking the overall change in distance between 2013 and 2019, (ii) imposing a strictly positive threshold above which the change-in-distance should be for an area to be classified as Treated. In the current design, the threshold is implicitly zero. We are investigating data-driven avenues to set this threshold.

We restrict our sample to apply the imputation approach of Borusyak et al. (2021) confidently. Note that these modifications introduce an imbalance in the panel data, and the number of observations varies for each time period. First, we strictly exclude all areas that experience one or several decreases in distance. In this setting, we only consider positive values of the treatment, meaning an increase in distance. Next, we exclude areas defined as "Switchers" once they experience a decrease in distance. This means that if they first experience an increase, they are considered as treated. They are also considered as controls as long as their distance is stable over time.

Keeping "Switchers" as long as they behave alike treated or controls allows us to boost the sample size and ensures that we have control and treated units in as many local authorities as possible. However, we exclude them as soon as they switch off to not rely on additional assumptions. Following Borusyak et al. (2021), including areas with on and off treatment would require to additionally assume that there are no within-unit spillovers from the periods of treatment to the future untreated periods. In other words, this would require the treatment effect to be assumed not to be dynamic, that the potential outcome in time t does not depend on the treatment status of time $t - 1$.. Last, to further simplify the analysis, we exclude from the sample "Treated several times" units after they experience a further increase in distance. We are currently investigating how to account for units that experience an additional "dose" in the treatment. As a first approach, we focus on the first increase in distance only.

4.2.2 Estimation procedure

The estimation is similar to Equation 4.1.2 but the treatment definition is not static. We introduce F_i: the unit-specific time of treatment. This is a date for treated units, and missing for never treated units.

Equation (2): $$Y_{ig,t} = \sum_{q=t}^{T} \mu_q Post_{i,q} + \sum_{q=t}^{T} \beta_q^{TWFE}(Access_{ig,q} \times Post_{i,q}) + \theta_t + \delta_{ig} + \epsilon_{ig,t}$$

for $q = t, ..., T$ and $t < F_i Post_{i,q} = 0$

for $q = t, ..., T$ and $t > F_i$ with $Access_{ig,q} = D_{ig,q} \times Post_{i,q}$

with $Post_{i,q}$ a set of time indicators that equal to 1 if $F_q = F_t$, 0 otherwise

Estimating Equation 4.2.2 through a Two-Way Fixed Effect procedure would create concerns in the presence of heterogeneous treatment effects. Especially, such estimations rely on using damaging pair-wise comparisons between "already treated" and "not-yet-treated" units. This issue has been widely discussed and recognized in prior research (see de Chaisemartin and D'Haultfœuille (2020), Borusyak et al. (2021), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), Sun and Abraham (2021) and for a synthesis of the literature, Roth et al. (2023)). To address this concern, we rely on the imputation-based estimators of Borusyak et al. (2021) using the R package *did_imputation*. We prefer this estimator for three main reasons. First, compared to alternative robust estimators, it uses all the data available. This is an especially attractive feature since several units experience treatment in each period after the reform in my setting. This creates a large pool of "not-yet-treated" units that would be under-used in models relying on defining controls as "never-treated" units. Second, the testing of parallel trends is done separately from estimation which the authors claim mitigates the pre-testing problem identified by Roth (2019). Third, the approach allows more flexibility to perform heterogeneity analysis by enabling to breakdown cohort-average treatment effects along covariates (*upcoming*).

4.3 Instrumental Variable Strategy: Shift-Share IV Using Caseload Composition

4.3.1 Instrument construction

To address concerns about endogeneity in distance changes and spatial autocorrelation, the second identification strategy employs a Bartik-style Instrumental Variables (IV) approach, following Goldsmith-Pinkham et al. (2020). This strategy leverages quasi-exogenous variation in pre-reform exposure to different types of legal aid services across areas, based on the composition of providers' caseloads and their spatial distribution.

The key insight is that providers varied in the extent to which their activity was concentrated in areas of law that were more or less severely affected by the 2012 reform. The Ministry of Justice's Equality Impact Assessment (EIA) predicted the extent of anticipated volume reductions and funding losses for each area of law (e.g., housing, debt, etc.) and type of assistance (e.g., early intervention vs. full representation). Using these domain-level shocks, we construct an MSOA-level instrument that captures each area's predicted exposure to legal aid retrenchment.

This Bartik IV strategy isolates variation in post-reform legal aid access driven by supply-side provider exposure rather than endogenous shifts in local demand. By drawing on the entire network of providers to define changes in access, this approach also accounts for the clustering of legal services, which creates differential access patterns between rural and urban areas. The instrument, computed for each MSOA, captures the degree of exposure to legal aid cuts by reflecting both providers' resilience and their spatial distribution.

Specifically, we first construct a provider-level measure of baseline vulnerability in each area of law a. This is defined as the product of the stream of funding coming from legal aid (Funding_p^a), and the provider's degree of specialisation in that area of law ($\text{Specialisation}_p^a$). This metric is then spatially weighted by the inverse distance from the MSOA's population-weighted centroid to the provider's location, and scaled by the size of the MSOA population (Population_{MSOA}). Aggregating over providers yields an MSOA-level measure of vulnerability to legal aid cuts for an area of law a.

Figure 61 reveals that three distinct patterns emerge in terms of specialisation profile at baseline. Highly specialized areas where providers focus almost exclusively on one type (Family law, Clinical negligence, and Immigration with medians above 40%); moderately specialized areas (Welfare benefits, Housing, Debt and Mental Health with medians between 5-20%); and areas characterized by low specialization where most providers dedicate less than 3% of their practice (remaining eight areas).

$$W_i^a = \sum_p \frac{1}{Distance_{i,p}} \cdot Funding_p^a \cdot Specialisation_p^a \cdot \frac{1}{Population_i}$$

These weights W_i^a reflects how strongly a MSOA i was connected to legal aid provision in an area of law a prior to the reform, as a function of providers' dependence on legal aid funding, distance decay, and local market size. To construct the instrument, we interact the MSOA-level vulnerability weights with the predicted shocks to each area of law. When analyzing outcomes at the local authority level, we aggregate the instrument accordingly, using distances from providers to the Local Authority's population-weighted centroid rather than MSOA centroids.

$$\text{Instrument}_i = \sum_a W_{i,a} \cdot Shock_a$$

This Bartik-style instrument varies across MSOAs based on their pre-reform exposure to legal aid provision in more versus less affected areas of law, while the shocks themselves are common across space. The first stage estimates how this instrument predicts post-reform legal aid access $Access_{i,t}$ in MSOA i at time t. The second stage then estimates the effect of access on outcomes $Y_{i,LA,t}$, with fixed effects and clustering at the local authority level:

$$\text{First Stage (1):} \quad Access_{i,t} = \alpha + \gamma \cdot Instrument_i + \epsilon_i$$

$$\text{Second Stage (2):} \quad Y_{i,LA,t} = \sum_{q=t}^{T} \mu_q Post_q + \sum_{q=t}^{T} \beta_q (\widehat{Access}_{i,LA,q} \times Post_q) + \lambda_t^{LA} + \phi X_i + \epsilon_{i,LA,t}$$

where $Post_q$ is an indicator equal to 1 for post-reform years, X_i captures time-invariant MSOA characteristics, and λ_t^{LA} absorbs year-specific shocks at the local authority level. This two-stage framework allows us to trace the impact of changes in access to legal aid services on local outcomes while leveraging plausibly exogenous variation in exposure generated by provider specialization and legal-domain shocks.

4.3.2 First Stage

To assess the relevance of the instrument, we regress it on three MSOA-level measures of change in access: (i) change in distance to the nearest provider (Figure 36), (ii) change in the distance-weighted provision (Figure 38), (iii) change in the number of providers within a distance radius (Figure 37. The resulting R^2 values, presented graphically, provide evidence of the instrument's explanatory strength (see Figure 39).

We also examine the instrument's predictive power for provider-level outcomes by testing whether providers' exposure to the reform predicts their exit from the legal aid market. Regressing provider exit on provider-specific instrument values yields statistically significant coefficients in key areas affected by the reform, including housing, debt, and welfare benefits (see Figure 40).

We further examine the characteristics of providers that exited the legal aid market following the reform—those who either left entirely or maintained dormant contracts without taking new clients. This analysis reveals important heterogeneity in exit patterns based on both organizational type (solicitors versus not-for-profits) and specialization profile. We classify providers as "generalists" if they serve three or more areas of law with at least 5% specialization, while "specialists" focus on fewer areas.

The exit patterns reveal striking differences across provider types and practice areas. Among not-for-profit organizations specializing in poverty law areas (housing, debt, and welfare benefits), exit rates were dramatically higher than for solicitors serving the same areas. Not-for-profit providers, with partial interconnection across poverty law areas, showed an 84% exit rate compared to just 34% for similar solicitors—a 50 percentage point difference.

This gap persisted across all poverty law configurations: single poverty law special-

ists (85% vs. 54%), and those serving all three poverty law areas (62% vs. 24%). The pattern reversed only for family law specialists, where solicitors had higher exit rates than not-for-profits (42% vs. 13%). These patterns, detailed in Table 15 and Figure 44, suggest fundamentally different business models and outside options. Solicitor specialists, particularly in family law, likely pivoted to private clients when legal aid funding was cut, explaining their higher exit rates in this area. Not-for-profit organizations, however, appeared more dependent on legal aid funding for poverty law services, lacking similar outside options.

This distinction proves especially important for understanding the mechanisms behind our welfare effects. The hypothesis we test is whether welfare outcomes deteriorate more severely when not-for-profit providers exit compared to private firms, driven by two potential channels. First, not-for-profit providers often provide holistic support beyond strictly legal services, leveraging additional funding streams for social support work. Second, and perhaps more fundamentally, there may be genuine added value in the generalist, interconnected approach that not-for-profit providers take to poverty law.

Given the well-documented clustering of legal problems across housing, debt, and welfare benefits among low-income households (Pleasence et al. (2013)), providers offering integrated services across these domains may be particularly effective at addressing the complex, interrelated challenges their clients face. The dramatic differential exit rates in poverty law areas, with not-for-profit providers 30-50 percentage points more likely to exit than solicitors, thus represent not just a loss of legal services, but potentially the elimination of a distinct and valuable model of legal assistance for vulnerable populations.

To test this mechanism empirically, we construct an additional instrument that separates the extensive margin response (providers exiting the market) from the intensive margin response (changes in volume among surviving providers). While our main difference-in-differences specification captures overall changes in distance to providers, this decomposition allows us to isolate the welfare effects specifically attributable to provider exits versus volume reductions. We create an "exit vulnerability" instrument that captures factors predicting acute vulnerability to market exit. The instrument combines three dimensions of vulnerability: (1) inverse baseline provider size, as smaller providers have less

capacity to absorb funding cuts; (2) specialization in poverty law areas (housing, debt, and welfare benefits), which faced the deepest cuts; and (3) not-for-profit status, given these organizations' lack of alternative revenue sources.

Formally, for each LSOA i, we construct:

$$\text{ExitVulnerability}_i = \frac{1}{\text{Pop}_i} \sum_j \left(\frac{1}{d_{ij,2009}} \times \frac{1}{\text{Size}_{j,2009}} \times (1 + \mathbb{1}_{\text{NFP},j} + \mathbb{1}_{\text{PovLaw},j}) \times \text{Shock}_j \right)$$

where $d_{ij,2009}$ is the baseline distance from LSOA i to provider j, $\text{Size}_{j,2009}$ is the provider's baseline volume across all areas, $\mathbb{1}_{\text{NFP},j}$ indicates not-for-profit status, $\mathbb{1}_{\text{PovLaw},j}$ indicates specialization in poverty law (>50% of work in housing, debt, or welfare benefits), and Shock_j captures the area-specific funding cuts. The instrument thus assigns higher vulnerability weights to LSOAs served primarily by small, poverty law-focused not-for-profit providers, precisely the providers our descriptive analysis shows were most likely to exit.

4.4 Identifying Assumptions and Threats to Identification

4.4.1 Anticipation

Identifying the average treatment effects requires assuming that the reform had no causal effect before its implementation. Coefficients should be insignificantly different from zero for all periods before the reform.

The legal aid reform was first presented to the UK Parliament almost two years before it was enacted and was adopted one year before it came into force (see the Timeline in Appendix Section E.2.1). As a consequence, providers may have anticipated the reform and begun diversifying their funding sources to increase their resilience as early as its initial discussion in the media following the formation of the Coalition government in May 2010. This is particularly relevant in light of the slight pre-trends observed 10 to 12 quarters prior to Q2 2013 in the event studies generated using the staggered treatment definition in Section 5.2.

Our analysis of entry and exit flows in the legal aid sector supports this intuition, revealing a clear trend of firms leaving the market ahead of April 2013 (see Figure 46), particularly those specialising in family, housing, debt, or welfare benefits law (see Appendix Figures 63 and 64). Breaking down the data between providers starting work and those billing also reveals clearer evidence of anticipation, showing that the number of new cases opened was reduced much earlier while billing was sustained until implementation (see Figure 45).

Next steps: To address concerns about anticipation effects, we measure changes in outcomes from the time the reform was introduced in Parliament, rather than from its date of implementation, extending the analysis up to one financial year after the reform came into effect. To determine the most appropriate reference point, we test the sensitivity of our results to different definitions of the pre- and post-reform periods. The event study graphs generated using the staggered treatment definition suggest that the reform's announcement date may more accurately capture providers' strategic responses than its formal adoption date. For the Bartik instrument, we use baseline observations from the 2009–2010 financial year. In the UK, financial years run from April 1st to March 31st of the following calendar year.

4.4.2 Parallel trends

The more serious threats to our identification strategy lie in the potential violation of the parallel trend assumption. The parallel trend assumption can be formulated as follows: in the absence of changes in access to legal assistance, the treated and control areas would have experienced the same trends in their mean outcomes. Stated differently, in the absence of the reform, the outcomes would have continued on similar trajectories in all areas.

For this assumption to hold, variation in access to legal assistance providers should not be correlated with other unobserved, area-specific, time-varying that affect the trend in my outcomes. Otherwise, we will not be able to determine whether the effect we are measuring causally results from the changes in access or another coinciding factor. Formally, in equation (1) this means that the treatment variable $D_{ig,t}$ is mean-independent of

the error term $\epsilon_{ig,t}$ It is worth noting that in a TWFE setting, the parallel trend assumption is conditional on the fixed effects terms which allow for different levels in pre-reform trends. $\forall t, D_{ig,t}|(\theta_t, \delta_{ig}, t) \perp \epsilon_{ig,t}$.

No confounding contemporaneous shock

Another way to express the parallel trends assumption is as the absence of confounding contemporaneous shocks—such as policy changes—that would differentially affect treated and control areas, thereby influencing the outcomes. In our setting, the main concerns stem from the implementation of various austerity measures during the 2010–2013 period, particularly cuts to the welfare sector, local authority funding, and rental allowances, all of which have been shown to have spatially heterogeneous effects (Beatty and Fothergill (2013), Beatty and Fothergill (2017), Fetzer (2019), Fetzer et al. (2023)). In Section 6.5, we outline the robustness checks conducted to ensure that we are isolating the specific effect of the legal aid reform. We also explain how this unique context enables us to examine the amplifying effect of cutting legal aid in tandem with reductions in other welfare provisions.

Additional concerns include the broader overhaul of the welfare benefits system, specifically the replacement of six legacy benefits and tax credits with a single scheme, Universal Credit (UC).The transition to UC, which aimed to simplify the benefits system by consolidating several schemes into one, was initially piloted in 2013 in a small number of areas and gradually rolled out nationwide starting in February 2015. The introduction of UC may have led to increased local demand for legal assistance with benefits-related issues. Simultaneously, local legal service provision may have adapted in response—for example, some local authorities may have increased funding to advice services in anticipation of heightened demand. We are currently testing whether early adoption of UC predicts changes in legal assistance provision in the areas where it was first introduced. If this proves to be a significant confounding factor, we will assess the sensitivity of our estimates by excluding UC pilot areas from the sample.[8]

[8]Forthcoming

No omitted variable

In a setting with continuous treatment, Callaway and Sant'Anna (2021) point out that the parallel trends assumption is even stronger to retrieve the average treatment effect: we need to assume that the treatment effect is homogeneous across groups, which means that the impact on outcomes for highly affected areas should be equal to the impact that would have been observed in little affected areas if they had been as much affected. Otherwise, TWFE estimates are biased. The use of distance to measure changes in access is reassuring to make such an assumption. Following Cunningham (2021), we argue that the timing and intensity of changes in access across areas appear arbitrary and unrelated to the impact they have had on residents.

In a typical DiD model, the parallel trend assumption is never directly testable since counterfactuals are never observed. The literature usually relies on visually observing pre-reform trends in outcomes to support its credibility and on further conditioning the parallel trends assumptions when covariates seem too unbalanced between treated and control groups. In our setting, there does not appear to be evidence against its plausibility. In the figures presented in section 5, the event-study coefficients in the periods leading up to the reform are not statistically different from 0. For the static definition of the treatment, the confidence bands cover 0 in all pre-treatment periods except when considering house prices. The pattern of the coefficients however suggests cyclical fluctuations for this outcome. Adding local authorities by year fixed effects to the specification mitigates this concern.[9] In addition, Callaway and Sant'Anna (2021) *did* package includes a Wald test to test the common trends assumption which one fails to reject for all outcomes.[10]

Local level of needs for legal assistance

[9] Adding local authority-specific time trends is not our favourite option. Although it captures all pre-existing differential trends between treated and untreated groups that may affect the outcome, it is also a highly data-demanding specification that may cause incidental issues. Moreover, linearly adding area-specific time trends has two main downsides. First, it may lead to over-controlling and downwardly biasing the estimate when the treatment has a dynamic effect. Second, it imposes a parametrical form that may not be the most suited to capture pre-existing trends

[10] To estimate the robustness of our results, we will closely follow Rambachan and Roth (2023) and use their Stata package HonestDiD to report the authors 'confidence sets' and show how our estimates of the treatment effect vary according to how different we assume the post-reform violations of parallel trends to be from the pre-reform differences in trends we observe.

The reform was imposed everywhere simultaneously, regardless of local levels of pre-reform provision and dynamics of needs. However, one may be worried about how initial differences in demand for legal assistance affected the interruption of legal services. There was already a variety of provision levels across the country as revealed by a spatial mapping at baseline (Figure 52 in Appendix). These differences are likely to reflect different needs based on local characteristics. For instance, one would expect to find more provision of legal aid in more deprived areas.

It seems reasonable that the parallel trend assumption may be violated if area-specific differences between treated and control groups are too important. Little affected and highly affected areas are very likely to differ, depending on the level and nature of legal assistance provision before the reform, but also depending on the dynamics of local needs for legal assistance. However, this is only a problem if differing area characteristics affect the evolution of local outcomes *to a different extent* over time. Interestingly, Table 2 reveals that areas do not differ in terms of the share of households in poverty, which we take as a proxy for the share of households eligible for legal aid. Areas experiencing the greatest change in distance to the nearest provider are also not worse off in terms of income, multiple deprivation (except for the geographical dimension), and different measures of poverty and housing insecurity, ruling out that these areas would experience a closure due to their expected worse outcomes. On the contrary, strategic consolidation of firms seems to have maintained provision in areas the most likely to display greater needs for legal assistance.[11]

In addition, we investigate the extent to which the gap between needs and supply differ across areas, before and after the reform. To compare areas at similar levels of needs, we adopt a methodology inspired by Pleasence et al. (2001) and construct proxies of local demand for housing legal aid. We compute the average number of households facing eviction proceedings, those in fuel poverty and those assessed for homelessness prevention or relief by their local authority. We then compare this estimated number of households likely to encounter housing legal issues with the count of active housing

[11]To investigate this further, we are testing the extent to which the list of covariates included in Table 2 predict changes in distance in 2013.

providers in the region.

Our estimates suggest that, at constant needs, the ratio of clients per provider ranged from 1,000 to 3,000 in 2011, and expanded from 1,000 to 20,000 in 2021 as shown in Figure 67. This supports the claim that the reform changed the geography of the provision of assistance, to a large extent irrespective of local needs. Figure 68 shows that needs, measured based on this definition, have relatively evolved at the same pace across the country, except in few localised areas. As robustness checks, we are testing the sensitivity of our estimates when excluding areas which experienced the greatest change in needs.

5 Results

5.1 Static Treatment: change in distance in the two years after the reform

When examining how the reform reshuffled the geography of services in the two years following its adoption, we find that the most affected areas —those in the top quintile in terms of changes in distance— gradually experienced a higher rate of eviction compared to areas that did not experience any change in distance to their nearest provider. This difference applies to both eviction filings and orders, with the disparities becoming statistically significant after three years. These differences gradually decrease during the pandemic, as eviction moratoriums were in place, but do not fade entirely 10 years post-reform.

The doubly robust Average Treatment Effect pooled over the period suggests that the rate of eviction claims and orders per 100,000 inhabitants is higher in the most affected areas by an average of 8.7 and 5.6, respectively, representing a 26% and 13% increase compared to the 2012 mean baseline. These areas saw on average an increase in distance of 13.7 km. Additionally, these areas exhibited a significantly higher mortality rate 6 years later, with the pooled average treatment effect being insignificant over the entire period. Last, these areas strikingly display a 8.5% decrease in mean price of houses sold, with an effect increasingly more pronounced over time, compared to least affected areas.

Figures 12, 13, 14, 15, and 34 display event-study graphs with a static definition of the treatment. This treatment is defined as being highly affected by the legal aid reform in terms of the top quintile change in distance to the nearest legal aid provider between Q2 2012 and Q1 2014 (a change greater than 1.9 km), compared to areas that experience no change in distance over the same period.

Figures 18, 19, and 20 display event-study graphs with a static definition of the treatment, grouped into several intensity categories: (i) the most affected areas, which are the top quintile in change in distance to the nearest legal aid provider between Q2 2012 and Q1 2014 (a change greater than 1.9 km), (ii) the slightly affected areas, which are those that experience a strictly positive change in distance but do not belong to the top quintile of the distribution, and (iii) the negatively affected areas, which are those where the change in distance is negative over the same time period. For all these groups, the baseline reference (control) consists of areas that experience no change in distance over the same period. For a distribution of the variable, refer to Table 4.

Figures 21, 22, 23, and 24 display event-study graphs with a static definition of the treatment, where a continuous definition of the change in distance variable between Q2 2012 and Q1 2014 is used.

Tables 7, 8, 9, 10 report the overall Average Treatment Effect on the Treated. Note that in a setting without staggered treatment, the weighted average of all Group-Time ATTs reported by Callaway and Sant'Anna (2021) is equal to the Average Treatment Effect on the (unique) Treated Group. Mean baselines are reported in Table 2. Tables 11, 12, 13 and 14 report time-specific average treatment effect of being treated after exactly t period.

5.2 Staggered Treatment: overtime change in distance between 2013 and 2019

When adopting a more dynamic perspective, regardless of the intensity of changes in the reform aftermath, effects are only confirmed for mortality rates and house prices. Areas that experienced an increase in distance to their nearest legal aid provider at any point between 2013 and 2019 observed significant and deepening effects in terms of increased

mortality rates and decreased house prices. However, effects previously observed on court cases prevalence and outcomes are not statistically significant. We explain this discrepancy by two facts.

First, distance to a provider is likely to matter only after a certain threshold. Small changes in distance are likely to be negligible in affecting court outcomes. Second, areas that were most affected right after the reform may have taken longer to adjust to the new landscape. Over time, one may expect actors to adjust more quickly to changes in provisions, which would raise reservations about grouping together areas that experience a change in distance at any point in time. Please note that we are in the process of retrieving cohort-specific treatment effects to test this hypothesis.

Figures 29, 30, 33 and 34 display event-studies graph with a staggered definition of the treatment, meaning experiencing an increase in the distance to the nearest legal aid provider at some point of time after the 2013 reform.

In an event-study setting, the estimates reported in a graphic format are the average effect h periods since treatment for a given horizon h. With a staggered design, each coefficient captures the Average Treatment on the Treated for the cohorts of units that have been exposed to treatment for a given duration.

5.3 Bartik IV

To examine whether the impact of legal aid cuts was amplified in areas experiencing concurrent welfare reforms, we interact our legal aid instrument with the intensity of housing benefit cuts at the local authority level, borrowing the combined shock exposure derived by (Fetzer et al. (2023)). Figure 41 presents event study estimates for areas at the 20th and 80th percentiles of housing benefit reductions. Both groups show parallel pre-trends, validating our identification strategy. However, post-reform trajectories diverge dramatically: areas with minimal housing benefit cuts (20th percentile) experience a modest increase in homelessness decisions of approximately 20 per 100,000 population, while areas facing substantial benefit reductions (80th percentile) see increases exceeding 100 per 100,000 by year seven—a five-fold difference.

Figure 42 decomposes this interaction effect across the full distribution of housing benefit exposure. The results reveal a stark non-linearity: while the bottom decile experiences an average post-treatment effect of 13.6 additional homelessness decisions per 100,000, this rises to 216.4 for the top decile—a 16-fold amplification. Crucially, the additional burden from housing benefit cuts (shown in red) dominates the baseline legal aid effect for areas above the median, accounting for over 80% of the total impact in the most affected areas. This complementarity between legal aid access and welfare support suggests that legal assistance may be particularly vital for navigating benefit systems and preventing housing crises when social safety nets are weakened. The findings indicate that the welfare costs of legal aid cuts cannot be evaluated in isolation but must account for the broader policy environment in which vulnerable populations operate.

6 Discussion

6.1 Why would access to legal aid matter for court outcomes?

Several mechanisms could explain how the reduction in access to legal assistance has led to an increase in eviction claims and orders in the most affected areas. First, the rise in the number of eviction claims may indicate a decline in the number of eviction cases settled outside of courts, contrary to policymakers' expectations. The reform was implemented under the belief that legal assistance often led to unnecessary litigation, anticipating that legal aid lawyers might push for court proceedings incentivised by the prospect of higher remuneration for longer-lasting cases. However, the higher number of claims observed in the most affected areas after the reform suggests that access to legal assistance may actually facilitate compromises or help tenants move out without resorting to formal eviction processes, before landlords file claims with the court. Conversely, landlords may be incentivized to directly use the formal process when they anticipate that tenants will have limited access to legal representation, using the court system as a deterrent when tenants are less likely to represent themselves.

Second, the increased number of eviction orders issued could be attributed to fewer

cases in which tenants had legal representation, or because tenants accessed legal representation too late in the process to avoid eviction orders. This is exemplified by the transformation of legal aid into a last-resort defense mechanism, such as the duty scheme at court, where lawyers meet legal aid clients on the spot and have very limited time and information to build a defense. Our fieldwork suggests that the outcomes obtained through the duty scheme are often perceived as unfavourable from a tenant's perspective, with Suspended Possession Orders being commonly seen as the 'least bad' outcome. These orders allow tenants to stay in the property as long as they meet certain conditions. However, tenants face the risk of facilitated eviction if they fail to comply. The event study 31 suggests an increase in the number of Suspended Possession Orders in areas most affected by the reform, even if the effects are not statistically significant, potentially signaling the role played by duty scheme lawyers in securing this outcome in areas where they are the only representatives of legal aid left.

To further investigate the mechanisms driving our eviction results, we examine heterogeneity based on the actual exit patterns of generalist providers. We calculate the generalist exit rate for each area—the proportion of baseline generalist providers (those serving 3+ areas of law) that exited by 2013—and split areas into deciles based on this measure. Figure 43 presents difference-in-differences estimates for each decile, controlling for baseline total provision.

The results suggest that areas experiencing higher rates of generalist exit (deciles 1-6) show larger positive treatment effects on eviction claims, ranging from 7-20 additional claims per 100,000 population. The effect is however non linear suggesting a potential threshold or compositional effect. The generally positive relationship between generalist exit rates and eviction increases supports our hypothesis that the loss of these multi-service providers—who integrated housing, debt, and welfare benefits assistance—left gaps in the support system that remaining specialists could not fill. Areas that lost more generalist providers seem to experience on average larger increases in evictions.

6.2 Why would access to legal assistance matter for mortality?

A decrease in access to legal assistance may lead to a higher mortality rate in the most affected areas through several potential mechanisms. First, the increased number of evicted households indicates a rise in housing insecurity. Formal eviction proceedings represent only a conservative estimate of the total number of people facing eviction prospects; many individuals move out before court proceedings take place. Reduced access to legal assistance also means a higher number of households navigating the legal journey alone, leading to increased stress especially for the most vulnerable population.

There is a body of evidence linking the experience of unresolved legal problems to poor physical and mental health outcomes. Studies indicate a bidirectional relationship between legal problems and health outcomes, with legal problems exacerbating health conditions and vice versa. The World Justice Project estimated in 2019 that one in three people suffered from physical or mental health issues as a consequence of their legal problems (World Justice Project (2019)). Legal needs surveys report a high level of stress, anxiety, depression, and physical health difficulties among households with social welfare legal problems (YouGov (2019), YouGov (2024)). Moreover, the role of legal problems in exacerbating health inequalities has largely been theorized in the sociolegal literature (Genn (2019)) and explored in the public health literature (see for example Leifheit et al. (2024)).

Such a causal relationship between legal problems and morbidity is especially acute when it comes to housing quality and security, domestic violence, or immigration (Tobin-Tyler et al. (2011)). Repeated and sustained exposure to stress has direct effects on high blood pressure, development of diabetes and ischemic heart disease (McEwen, 2000), and indirect effects on health status through its influence on risky health behaviours (Adler and Newman, 2002; Mullainathan and Shafir, 2013). Legal problems triggering stress-related ill-health have also been shown to lead to a higher number of consultations with healthcare professionals (Balmer and Pleasence, 2018). Legal assistance, by contrast, can alleviate stress and improve overall well-being by directly or indirectly improving health outcomes. Furthermore, the absence of legal assistance results in more households lack-

ing holistic support.

Preliminary evidence drawn from a side-project tracking households after they receive legal assistance in administrative benefits data suggests that, when assisting a tenant with an eviction case, lawyers' intervention often extend beyond providing strictly housing-related legal advice (Uraz (2025)). Lawyers may refer their clients to healthcare providers, particularly when identifying disabilities or mental health issues that could serve as evidence to adjourn eviction cases. Additionally, they may assess clients' eligibility for public benefits or local welfare schemes when suspecting non-take-up, or when helping to establish credible plans to address rent arrears and persuade a judge not to issue an eviction order— all of which can have spillover effects on prospects for better health. Caspi and Rafkin (2024) provide evidence from the US context that legal assistance is most effective when rental assistance programs are available and actively pursued by lawyers.

Preliminary findings suggest that mortality effects are concentrated among the elderly, although this is still under investigation (16, 24). This finding would be consistent with the most affected households being those most vulnerable to digital exclusion and the lack of in-person substitutes for the holistic assistance that legal aid could provide. We are currently investigating the impact of the legal aid reform on hospital bed occupancy and the use of emergency services to better understand the potential healthcare services channel.

6.3 The role of generalist not-for-profit providers in the legal aid ecosystem

The legal aid cuts were premised on the assumption that the charity sector could absorb the shock of reduced legal aid provision, particularly for early intervention services that might prevent cases from reaching court. Policymakers envisioned a division of labour where charities would handle initial advice and triage, reserving costly legal representation for courtroom proceedings. However, our findings suggest this view misunderstood the integrated nature of legal aid provision and the unique complementary role of not-

for-profit providers within this ecosystem.

Our analysis reveals that the reform disproportionately pushed not-for-profit providers out of the legal aid market, particularly those operating as generalists across poverty law areas (Figures 15 and 44). These organizations, including law centres, Citizens Advice Bureaux with legal aid contracts, and specialized legal charities, played a distinctive role that went beyond simple advice provision. They functioned as legal-informed navigators of the welfare state, bridging the gap between identifying a problem and accessing appropriate legal remedies.

The distinction between general advice and legal aid might prove to be crucial. While charities can distribute information about rights (such as handing out leaflets explaining tenant protections), legal aid providers engage in a more intense form of work: sitting with vulnerable clients to translate their lived experiences into what Genn (1999) describes as "justiciable problems"— problems for which legal remedies exist within the civil justice system. This process involves identifying available legal remedies, completing the necessary paperwork, and connecting clients to specialist solicitors when needed. Transforming everyday "problems" into actionable "legal problems" requires both legal expertise and *time* for personalized assessment—something that generic advice services cannot provide. Before the reform, such personalized advice sessions were covered by legal aid as part of the "legal help" scheme. These early interventions were among the most dramatically cut.

Moreover, not-for-profit providers served as critical nodes in referral networks. When operating within the legal aid scheme, they could screen and sort cases, ensuring appropriate allocation to specialist solicitors while handling early intervention themselves. The reform's decimation of these providers not only reduced capacity but also fragmented these referral pathways, leaving vulnerable populations without clear routes to appropriate assistance as documented by Organ and Sigafoos (2018).

To better understand the mechanisms underlying our mortality findings, we employed two complementary approaches. First, we linked monthly administrative benefits data with law centre client records to trace households' financial trajectories before and after legal assistance (Uraz (2025)). This analysis reveals a significant increase in rental

assistance take-up among treated households compared to matched controls, providing correlational evidence that legal aid providers actively address benefit non-take-up.

Second, we conducted a survey among past clients of law centres, a typical form of generalist not-for-profit specialized in poverty areas of law, asking respondents what happened after seeing a lawyer and whether the lawyer connected them to other services. Of 163 respondents, 53% reported receiving support beyond legal services, with 37% receiving help across multiple domains. This additional support clustered in three critical areas: income-related services (44% helped accessing benefits or financial assistance), health-related services (39% connected to NHS care or mental health services), and housing-related services (39% receiving accommodation support).

Together with the administrative data showing increased income from benefits, these findings illuminate the "legal-plus" model that distinguished not-for-profit providers, where legal assistance serves as an entry point for addressing interconnected social, health, and economic challenges. This approach appears particularly well-suited to the clustered nature of problems faced by low-income households, who often experience cascading crises across multiple domains simultaneously. Law centres and similar not-for-profit providers functioned as comprehensive welfare hubs, using legal problems as diagnostic moments to identify and address broader vulnerabilities.

The loss of such providers through funding cuts thus represents not just a reduction in legal capacity, but the dismantling of integrated support systems that address the complex, multidimensional nature of poverty-related legal needs. Neither private firms focused on discrete legal matters, nor generic advice charities possess the combination of legal expertise, holistic approach, and embedded referral networks that characterized these not-for-profit providers. Their exit from the market may help explain why areas most affected by the reform experienced not only worse legal outcomes but broader deteriorations in population health and welfare.

6.4 Why would local provision of legal aid matter for house price dynamics?

Exploring potential mechanisms connecting reduced access to legal assistance in an area and the decline in house prices in that area reveals several interconnected factors. The reduced access to legal assistance may signify a broader issue of area deprivation. As the area becomes increasingly deprived, characterized by higher rates of eviction and mortality, affluent individuals may opt to move out in search of more desirable living conditions. This exodus of wealthy residents could lead to a decrease in demand for housing, thereby exerting downward pressure on property prices. Additionally, we are exploring if this trend correlates with other changes in public facilities access, such as healthcare deserts or school closures, further exacerbating the economic decline in the area and impacting property values negatively. In this context, this systemic phenomenon not only affects housing values but may also elucidate the mechanism behind the increased mortality rate. Especially, we are testing the sensitivity of our results to the degree of exposure to austerity cuts following Fetzer (2019).

Moreover, there may be an underlying debt story linked to the decrease in legal assistance. The legal aid reform did not only affect the provision of legal assistance to deal with housing issues but also with debt and financial legal problems. The absence of adequate debt advice services may lead to more debt judgments against residents. These judgments can have a detrimental effect on credit scores, making it more difficult for individuals to secure mortgages. This difficulty is compounded by the implementation of macroprudential measures such as the loan-to-income ratio in 2014, which restrict access to mortgages based on income levels and could further suppress housing demand in the area. We are in the process of testing these mechanisms by looking at the impact of the reform on the prevalence and outcomes of debt court cases.

In summary, the decline in house prices in areas with reduced access to legal assistance may stem from a combination of factors, including affluent residents moving out, broader area deprivation issues, and the impact of debt judgments on credit scores and mortgage accessibility. At the time of writing, we are exploring these avenues by con-

ducting heterogeneity and sensitivity analysis to better understand the characteristics of the areas experiencing changes in the provision of legal assistance. Notably, we are analyzing metropolitan areas separately from rural areas to discern any distinct patterns or effects and adding housing transaction data, property characteristics and rental data.

6.5 How can we disentangle the effect of the reform from austerity?

One might be concerned that the effects of the reform reflect only the broader impact of the austerity cuts implemented by the UK government across a wide range of sectors. This concern is based on evidence of spatial variation in the intensity of welfare cuts at the local authority level — a variation exploited by Fetzer (2019), Fetzer et al. (2023), and Berman and Hovland (2024) to measure the impacts of austerity on voting behaviour, homelessness, and mortality, respectively. To address this concern, we retrieve data on exposure to welfare austerity cuts, following Fetzer (2019), and interact our main *Access* variable with the austerity measure. Additionally, we include local authority fixed effects in our specification to account for variation within local authorities.

Pooled Difference-in-Differences estimates, using the continuous definition of the change in the distance variable, suggest that even if a higher score of austerity exposure amplifies the effect of the reform, austerity alone does not explain the observed findings. Specifically, the key DiD coefficient interacting the change in distance and the Post dummy remains significant after accounting for austerity exposure (see Table 6). Furthermore, the relationship between the change in distance and austerity exposure appears to be weakly negatively correlated (Figure 9). Finally, the results hold when local authority fixed effects are included, though confidence intervals are inflated due to the smaller sample size, as some local authorities lack variation in distance across their MSOAs (see Table 5, Figures 26, 27 and, 28). Nonetheless, to address additional concerns, we are currently constructing indices of exposure to the austerity and to the financial crisis at the MSOA level.

6.6 Marginal Value of Public Funds - Forthcoming

By examining how reduced access to legal aid affects welfare-relevant outcomes (e.g., housing stability, mortality rates, emergency uses and court outcomes), this study aims to ultimately assess whether the cost savings from the reform were offset by wider social costs adopting a Marginal Value for Public Funds perspective.

Preliminary analysis of expenditure patterns suggests the reform may have generated less fiscal relief than anticipated. As shown in Figures 74-77 in the Appendix, while total legal aid expenditure declined substantially post-reform, the composition of spending shifted markedly toward legal representation for complex cases. The elimination of Legal Help funding—which provided early intervention at relatively low cost per case—appears to have been partially offset by sustained or increased spending on more resource-intensive court proceedings. This reallocation toward higher-cost interventions raises questions about the net efficiency gains from the reform, particularly when average cost per case is considered alongside total expenditure reductions.

To fully evaluate these trade-offs, this study will calculate the reduction in public spending due to the legal aid reform, using the provider-level data on caseload volume and expenditures reductions post-reform. It will also estimate monetized welfare impacts on outcomes from the beneficiaries' perspective, using the causal estimates derived from the DiD-IV approaches. These quasi-experimental estimates enable us to calculate the excess numbers of court cases or deaths attributable to the reform.

To translate these effects into monetary values, we will: (1) estimate the additional costs borne by the public sector as externalities resulting from reduced legal aid, including courts costs from increased hearings, local authority costs for additional households in temporary accommodation, and NHS costs once assessed the impact on emergency service use, (2) use existing literature estimates to monetize welfare impacts on individuals, such as income effects from an increased likelihood of eviction (Collinson et al. (2024)), higher consumer bankruptcy risk (Dobbie and Song (2015)), and shortened life-expectancy (eg. based on mortality risk valuations), (3) incorporate changes in house prices to capture tenants' overall welfare gain, following van Dijk et al. (2024). These

findings will provide critical evidence on the full social costs of legal aid retrenchment, informing future policy decisions about the optimal structure and funding level of legal assistance programs.

7 Conclusion

Access to justice is a cornerstone of the rule of law, yet its practical implications remain insufficiently quantified. Interventions aimed at helping individuals understand, navigate, and utilize the legal system to protect or enforce their rights have rarely been evaluated outside of the courtrooms. They are often conceptualised in isolation from other social protection programs. This study underscores the importance of adopting an empirical approach to assess the functioning and broader impacts of legal assistance programs, as with any poverty alleviation policy.

By examining the 2013 legal aid reform in England and Wales, this paper provides evidence that changes in access to in-person legal assistance have far-reaching consequences, not only for the vulnerable households directly affected but also for wider socioeconomic and public health outcomes. The observed increases in eviction, homelessness, and mortality rates highlight the societal costs of unmet legal needs, suggesting that access to legal support plays a critical role in stabilizing at-risk populations and fostering social welfare.

This paper also brings new evidence on the value of combining early-stage intervention with court representation as part of an integrated scheme and within a geographical hub of services. While the distribution of work might be split between lawyers, social workers, and other specialists, the co-location and co-offering of services appear as key factors without which the effectiveness of the last-resort scheme seems to collapse. While eviction was never meant to be affected by the legal aid cuts, our study suggests that eliminating the preventive services had the unintended consequences of bringing more cases to court and producing less favourable court outcomes for tenants, with measurable impacts on the use of shelters and the number of households seeking emergency support from local governments for homelessness prevention purposes, generating substantial costs borne by local councils finances and the courts.

These insights are directly relevant to ongoing debates in several countries retrenching their legal aid funding and replacing early-stage support with digital solutions, as exemplified by policy options framed as a trade-off between funding for frontline charities providing legal advice and subsidizing lawyers only for representation in courts, as implemented in most right-to-counsel programs in the U.S. Our findings suggest that the policy debate would benefit from being reframed as a necessary bundling of services within a two-tier system, instead of a clear-cut trade-off.

More research is needed to track closely the health impact of accessing legal assistance in civil cases and uncover the mechanisms at play. Such insight would illuminate the high costs that households are willing to pay to have a lawyer by their side throughout the process, regardless of their expectations about the outcome of their cases, as evidenced recently in elicitation experiments by Caspi and Rafkin (2024), which show the high value beneficiaries give to the stress-reduction component of the scheme. It would also shed light on the potential connections and integrations of legal services with other healthcare use, since legal protections can oftentimes be only activated upon proof of medical evidence, such as in the case of domestic violence, child neglect, or housing disrepair.

While economics research has only started to open the black box of the civil justice system, expanding empirical investigations may prove increasingly important in light of the United Nations' recognition of access to justice as a global priority under its 2030 Sustainable Development Goals (Goal 16.3). Such work could contribute to a deeper understanding of justice systems and inform the design of policies that safeguard both individual rights and collective well-being.

References

Abel, L. and S. Vignola (2010). Economic and other benefits associated with the provision of civil legal aid. *Seattle Journal for Social Justice 9*(1), 139–167.

Abramson, B. (2022). The welfare effects of eviction and homelessness policies. Job Market Paper, Columbia Business School.

Balmer, N., A. Patel, and M. Smith (2012). Just a phone call away: Is telephone advice enough? *Journal of Social Welfare and Family Law 34*(1), 63–85.

Beatty, C. and S. Fothergill (2013). Hitting the poorest places hardest: The local and regional impact of welfare reform. *Centre for Regional Economic and Social Research Sheffield Hallam University*, 2–45.

Beatty, C. and S. Fothergill (2017). Welfare reform in the united kingdom 2010–16: Expectations, outcomes, and local impacts. *Social Policy and Administration 52*(5), 950–968.

Benfer, E. A., P. Hepburn, V. Nazarro, L. Robinson, J. Michener, and D. E. Keene (2025). A descriptive analysis of tenant right to counsel law and praxis 2017-2024. *Housing Policy Debate 35*, 470–95.

Bergman, P., R. Chetty, S. DeLuca, N. Hendren, L. F. Katz, and C. Palmer (2025). Creating moves to opportunity: Experimental evidence on barriers to neighborhood choice. *American Economic Review 114*, 1281–1337.

Berman, Y. and T. Hovland (2024). The impact of austerity on mortality and life expectancy. *LSE International Inequalities Institute Working Paper Series* (139), 1–54.

Borusyak, K., X. Jaravel, and J. Spiess (2021, August). Revisiting Event Study Designs: Robust and Efficient Estimation. Papers 2108.12419, arXiv.org.

Britto, D., L. Germinetti, F. Gerard, J. Naritomi, and B. Sampaio (2025). Access to justice and social protection. *AEA Papers and Proceedings 115*, 329–34.

Buenaventura, M. and P. Heaton (2025). Forced out: Civil legal access and housing stability. *Fordham Urban Law Journal 52*, 697–753.

Burton, M. (2018). Justice on the line? a comparison of telephone and face-to-face advice in social welfare legal aid. *Journal of Social Welfare and Family Law 40*(2), 195–215.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2), 200–230. Themed Issue: Treatment Effect 1.

Caspi, A. and C. Rafkin (2024). Legal assistance for evictions: Impacts, mechanisms, and demand. Unpublished manuscript.

Cassidy, M. and J. Curie (2022). The effects of legal representation on tenant outcomes in housing court: Evidence from new york city's universal access program. *NBER Working Paper 29836*.

Castell, L., M. Gurgand, C. Imbert, and T. Tochev (2025). Take-up of social benefits: Experimental evidence from france. *American Economic Journal: Economic Policy 17*, 1–29.

Collinson, R., J. E. Humphries, N. Mader, D. Reed, D. Tannenbaum, and W. van Dijk (2024). Eviction and poverty in american cities. *The Quarterly Journal of Economics 139*(1), 57–120.

Cooper, R., J. J. Doyle, and A. Hojman (2025). Effects of enhanced legal aid in child welfare: Evidence from a randomized trial of mi abogado. *American Economic Review 115*(7), 2306–2342.

Cunningham, J. and A. Goodman-Bacon (2025). Changes in family structure and welfare participation since the 1960s: The role of legal services. *American Economic Journal: Applied Economics 17*(1), 369–401.

de Chaisemartin, C. and X. D'Haultfœuille (2020, September). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

Denary, W., A. Fenelon, P. Schlesinger, J. Purtle, K. M. Blankenship, and D. E. Keene (2021). Does rental assistance improve mental health? insights from a longitudinal cohort study. *Social Science & Medicine 282*, 114100.

Desmond, M. (2016). *Evicted: Poverty and Profit in the American City*. Crown Publishers/Random House.

Dobbie, W. and J. Song (2015). Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection. *American Economic Reviews 105*(3).

Ellen, I., K. O'Regan, S. House, and R. Brenner (2021). Do lawyers matter? early evidence on eviction patterns after the rollout of nyc's universal access to counsel. *NYU Furman Center Housing Policy Debate 31*(3). Working Paper.

Fenelon, A., K. J. Lipska, W. Denary, K. M. Blankenship, P. Schlesinger, D. Esserman, and D. E. Keene (2022). Association between rental assistance programs and hemoglobin a1c levels among us adults. *JAMA Network Open 5*(7), e2222385.

Fetzer, T. (2019). Did austerity cause brexit? *American Economic Review 109*(11), 3849–86.

Fetzer, T., S. Sen, and P. C. Souza (2023). Housing insecurity and homelessness: Evidence from the uk. *Journal of the European Economic Association 21*(2), 526–559.

Finkelstein, A. and M. J. Notowidigo (2019). Take-up and targeting: Experimental evidence from snap. *Quarterly Journal of Economics 134*, 1505–1556.

Genn, H. (1999). *Paths to Justice: What People Do and Think about Going to Law*. Hart Publishing.

Genn, H. (2019). When law is good for your health: Mitigating the social determinants of health through access to justice. *Current Legal Problems 72*, 159–202. Published: 24 June 2019.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2020). Bartik instruments: What, when, why, and how. *American Economic Review 110*(8), 2528–2624.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics 225*(2), 254–277. Themed Issue: Treatment Effect 1.

Greiner, J. and C. Pattanayak (2011). Randomized evaluation in legal assistance: What difference does representation (offer and actual use) make? *Yale Law Journal 121*.

Greiner, J., C. Pattanayak, and J. Hennessy (2013). The limits of unbundled legal assistance: A randomized study in a massachusetts district court and prospects for the future. *Harvard Law Review 126*, 901.

Grimwood, G. G. (2015). Controvery in 2010-11 surrounding the government's plans for legal aid reforms. *Briefing Paper - House of Commons Library* (05840).

Hirsch, D. (2018). Priced out of justice? means testing legal aid and making ends meet. *Report commissioned by the Law Society*, 2–45.

House of Commons Justice Committee (2015). Impact of changes to civil legal aid under part 1 of the legal aid, sentencing and punishment of offenders act 2012. Technical report, United Kingdom House of Commons.

Hoynes, H. W., N. Maestas, and A. Strand (2022). Legal representation in disability claims. *NBER Working Ppaers 29871*.

Ipsos-MORI (2017). Analysis of the potential effects of early legal advice intervention. *Report for the Law Society*, 1–21.

Keene, D. E., L. Niccolai, A. Rosenberg, P. Schlesinger, and K. M. Blankenship (2020, February). Rental assistance and adult self-rated health. *Journal of Health Care for the Poor and Underserved 31*(1), 325–339.

Law Centre Network (2018). Laspo act 2012 post-implementation review submission from the law centre network. Technical report, Law Centre Network.

Leifheit, K. M., K. L. Chen, N. W. Anderson, C. Yama, A. Sriram, C. E. Pollack, A. Gemmill, and F. J. Zimmerman (2024). Tenant right-to-counsel and adverse birth outcomes in new york, new york. *JAMA Pediatrics*.

Lindo, J., C. Myers, A. Schlosser, and S. Cunningham (2020). How far is too far? new evidence on abortion clinic closures, access, and abortions. *The Journal of Human Resources 55*(4), 1137–1160.

Low Commission (2014). Tackling the advice deficit: A strategy for access to advice and legal support on social welfare law in england and wales. Report of the Low Commission on the future of advice and legal support.

Ministry of Justice of the United Kingdom (2011). Reform of legal aid in england and wales: Equality impact assessment (eia). Technical report, Ministry of Justice.

Ministry of Justice of the United Kingdom (2019). Post-implementation review of part 1 of the legal aid, sentencing and punishment of offenders act 2012 (laspo). Technical report, Ministry of Justice.

Ministry of Justice of the United Kingdom (2024). User guide to legal aid statistics in england and wales. Technical report, Ministry of Justice and Legal Aid Agency.

Moorhead, R. and M. Robinson (2006). A trouble shared legal problems clusters in solicitors' and advice agencies. Technical report, Department for Constitutional Affairs Research Series.

National Audit Office (2014). Implementing reforms to civil legal aid. Technical report, National Audit Office.

National Audit Office (2024). Government's management of legal aid. Technical report, National Audit Office.

Newton, J., P. Arboleda, M. Connors, and V. Figueroa (2020). Civil gideon and nyc's universal access: Why comprehensive public benefits advocacy is essential to preventing evictions and creating stability. *Cuny Law Review 23*(1).

OECD (2019). Legal needs surveys and access to justice. Technical report, OECD/Open Society Foundations, France.

Organ, J. and J. Sigafoos (2017). *Access to Justice and Legal Aid*, Chapter What if There Is Nowhere to Get Advice? Oxford Hart Publishing.

Organ, J. and J. Sigafoos (2018). The impacts of laspo on routes to justice. Technical report, Equality and Human Rights Commission.

Pleasence, P., N. Balmer, A. Buck, A. O'Grady, and H. Genn (2004). Multiple justiciable problems: Common clusters, problem order and social and demographic indicators. *Journal of Empirical Legal Studies 1*(2), 301–330.

Pleasence, P., N. Balmer, and R. Sandefur (2013). Paths to justice: A past, present and future roadmap. *Report for the Nuffield Foundation*, 1–165.

Pleasence, P., A. Buck, T. Goriely, J. Taylor, H. Perkins, and H. Quirk (2001). Local legal need. *Research Paper* (7).

Pyper, D., G. Sturge, S. Lipscombe, and S. Holland (2020). Spending of the ministry of justice on legal aid. Technical report, United Kingdom House of Commons Library.

Rambachan, A. and J. Roth (2023). A more credible approach to parallel trends. *Review of Economic Studies 90*, 2555–2591.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2023). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics 235*, 2218–2244.

Sandefur, R. L. and T. M. Clarke (2017). Roles beyond lawyers: Evaluation of the new york city court navigators program. Technical report, American Bar Foundation.

Sant'Anna, P. H. and J. Zhao (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics 219*(1), 101–122.

Seron, C., M. Frankel, G. Van Ryzin, and J. Kovath (2001). The impact of legal counsel on outcomes for poor tenants in new york city's housing court: Results of a randomized experiment. *Law & Society Review 35*(2), 419–434.

Smith, M., N. Balmer, J. Miles, and C. Denvir (2013). In scope but out of reach: Examining differences between publicly funded telephone and face-to-face family law advice. *Child and Family Law Quarterly 25*(3), 25–3.

Smith, M., A. Buck, J. Sidaway, and L. Scanlan (2013). Bridging the empirical gap: New insights into the experience of multiple legal problems and advice seeking. *Journal of Empirical Legal Studies 10*(1).

Stout Risius Ross, Inc. (2016, March). Report of stout risius ross, inc. Presented for: Pro Bono and Legal Services Committee of the New York City Bar Association. The Financial Cost and Benefits of Establishing a Right to Counsel in Eviction Proceedings Under Intro 214-A.

Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics 225*(2), 175–199. Themed Issue: Treatment Effect 1.

The Law Society (2017). Access denied? laspo four years on: A law society review. Technical report, The Law Society of England and Wales.

The Law Society (2021). Civil legal aid: A review of its sustainability and the challenges to its viability. Technical report, The Law Society of England and Wales.

Tobin-Tyler, E., T. Boyd-Caine, H. Genn, and N. M Ries (2023). Health justice partnerships: An international comparison of approaches to employing law to promote prevention and health equity. *The Journal of Law, Medicine & Ethics 51*(72), 332–343.

Tobin-Tyler, E., K. N. Conroy, C.-M. Fu, and M. Sandel (2011). Housing: The intersection of affordability, safety and health. In Tobin-Tyler and Lawton (Eds.), *Poverty, Health and Law. Readings and Cases for Medical-Legal Partnership*. Carolina Academic Press.

Uraz, J.-N. (2025). Exploring the impact of legal services using administrative benefits data. report for the legal education foundation. Technical report, Policy in Practice.

van Dijk, W., R. Collinson, J. E. Humphries, S. Kestelman, S. Nelson, and D. Waldinger (2024). Right-to-counsel and rental housing markets: quasi-experimental evidence from new york. Unpublished manuscript.

Wilding, J. (2021). *The Legal Aid Market*. Policy Press Bristol University Press.

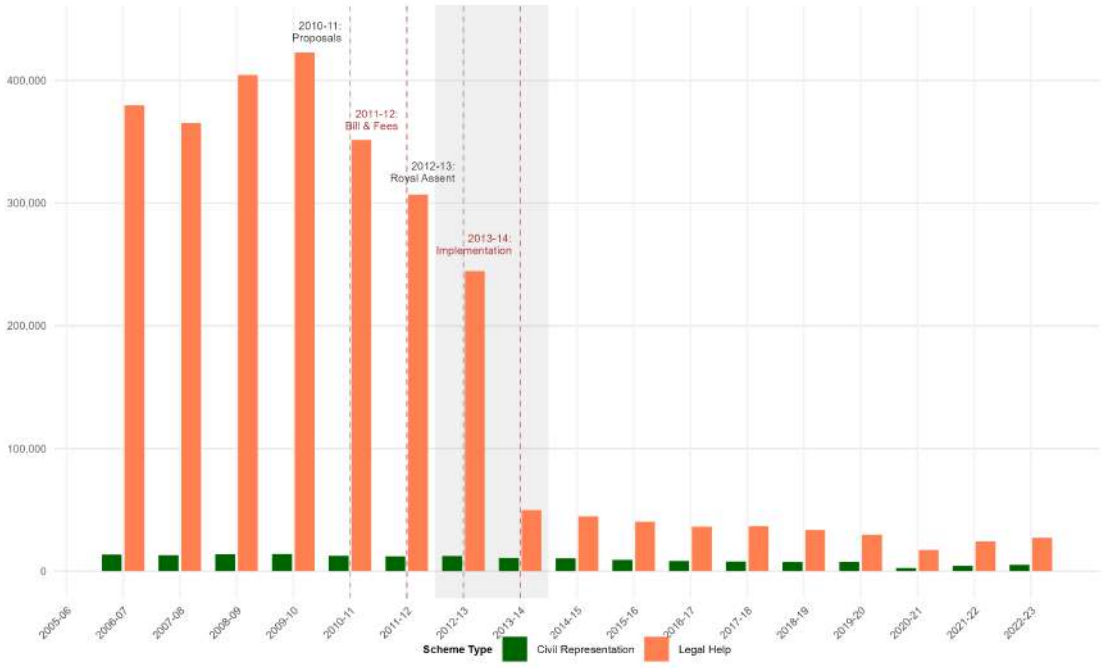
World Bank (2019). A tool for justice a cost-benefit analysis of legal aid. Technical report, World Bank/International Bar Association, United States.

World Justice Project (2019). Global insights on access to justice: Findings from the world justice project general population poll in 101 countries. Technical report, World Justice Project, United States.

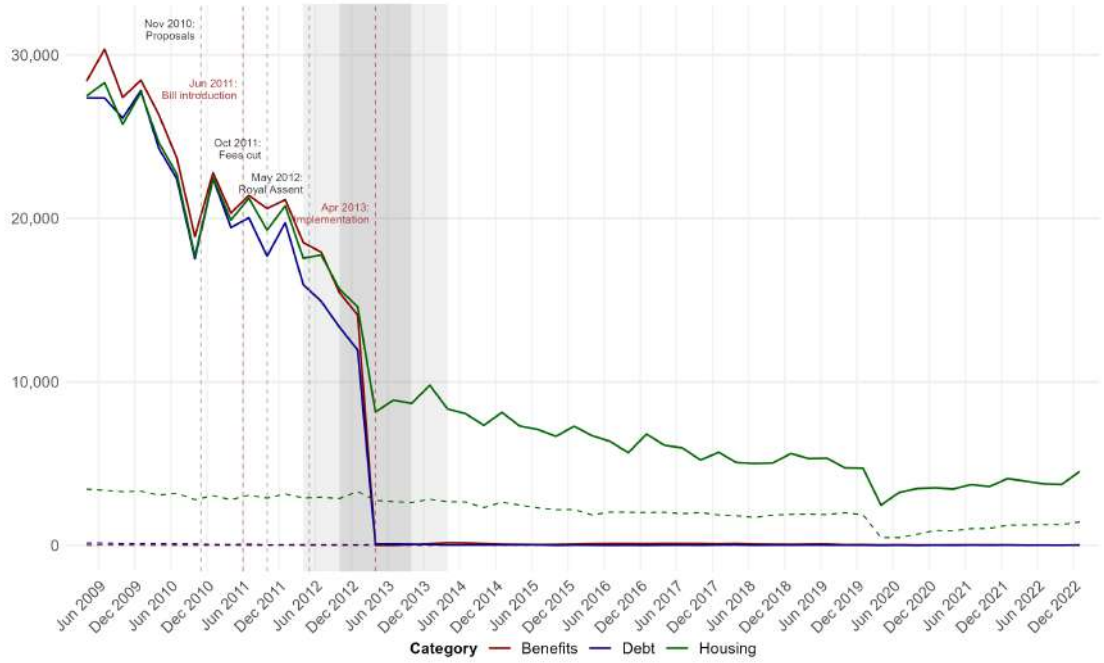
YouGov (2019). Legal needs of individuals in england and wales. Technical report, Legal Services Board and The Law Society.

YouGov (2024). Legal needs of individuals in england and wales. Technical report, Legal Services Board and The Law Society.

A Figures



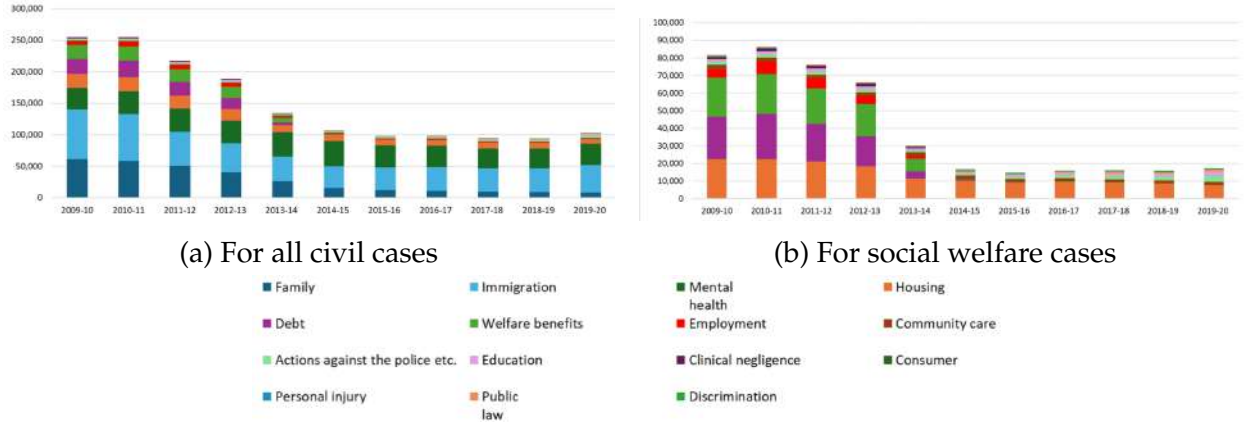
(a) By type of legal assistance



(b) By areas of law

FIGURE 1: Evolution of the number of legal aid cases opened

Source: Ministry of Justice Legal Aid Statistics
Note: These graphs depict the evolution of the number of legal aid cases opened in England and Wales. The top panel uses Ministry of Justice aggregate statistics available from financial year 2006-07 to 2022-23, providing a longer time horizon than the panel dataset used in the main analysis which begins in 2009-10. This extended period illustrates that legal aid was demand-driven, with welfare cases peaking during the Great Financial Crisis in 2009-10, before the policy reforms. The bottom panel shows quarterly data for the three welfare subcategories (debt, housing, and benefits) from 2009-10 to 2022-23. Legal aid work is categorized into two main types: legal help and civil representation. According to the Ministry of Justice definitions, legal help includes advice and assistance about a legal problem but does not include representation or advocacy in proceedings, while civil representation is representation by solicitors and barristers for civil cases that could go to court. The top panel shows aggregate welfare cases (sum of debt, housing, and benefits), with bars for each scheme type. The bottom panel shows the three welfare subcategories separately, with solid lines representing legal help cases and dashed lines representing civil representation cases. The vertical dashed lines mark key reform milestones: proposals (November 2010), bill introduction (June 2011), fees cut (October 2011), royal assent (May 2012), and implementation of LASPO (April 2013). The shaded areas highlight the transition period (light gray) and the period of highest change (dark gray). The lines representing civil representation cases for debt and benefits overlap due to low case volumes.



(a) For all civil cases

(b) For social welfare cases

FIGURE 2: Evolution of the annual value of legal aid claims (in £) by area of law
Source: Ministry of Justice Legal Aid Statistics, author's calculations.
Note: These graphs depict the evolution of legal aid funding in England and Wales between the financial years 2009 and 2019, broken down by areas of law.

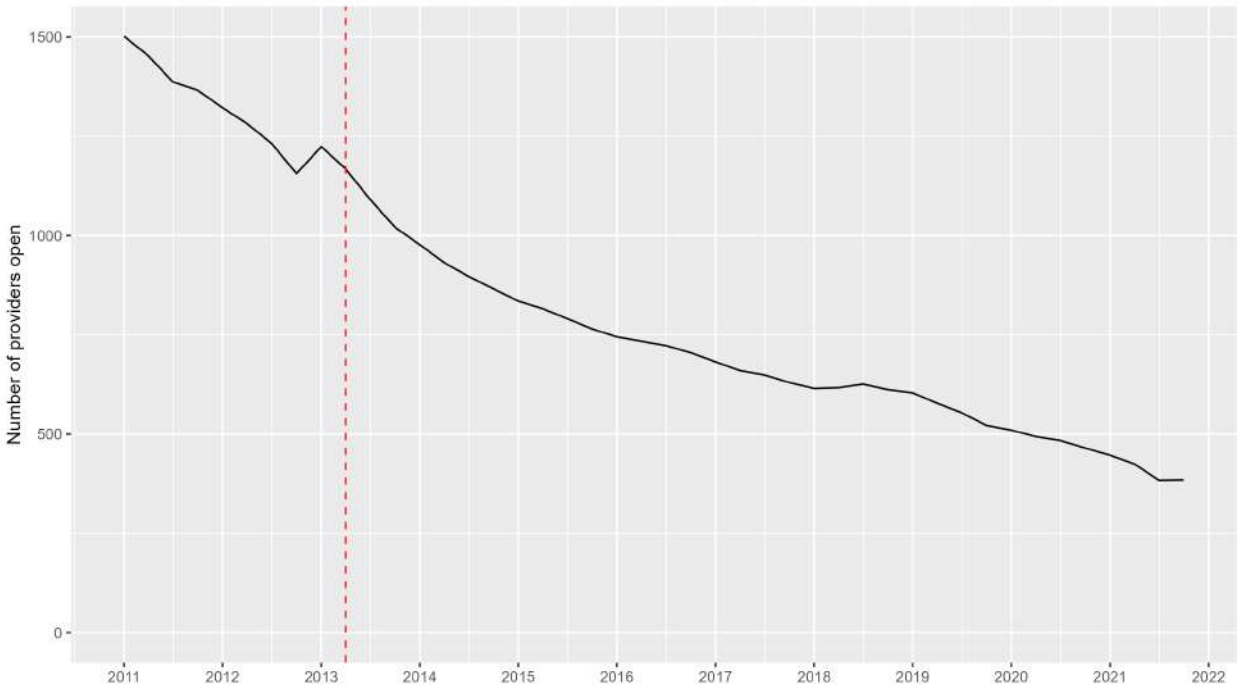


FIGURE 3: Number of active legal aid providers over time

Source: Ministry of Justice Legal Aid Statistics, author's calculations.
Note: We define an active legal aid provider as a provider that takes on new cases. In this paper, a provider is considered to cease being active at time t if they do not take on new legal aid cases for all subsequent quarters. The red dashed line indicates when the legal aid reform was implemented in April 2013. Only providers of social welfare laws are shown. Social welfare laws encompass housing, debt and benefits laws.

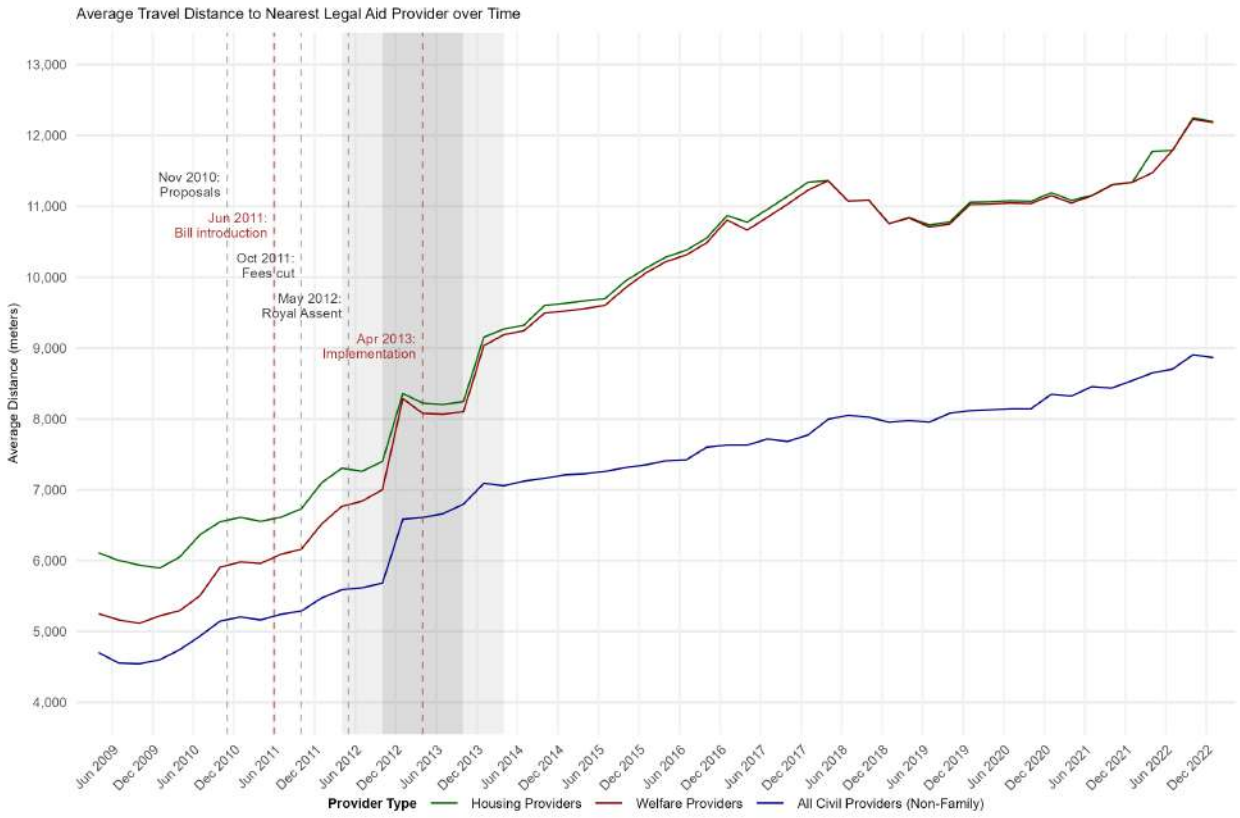


FIGURE 4: Average Distance to Nearest Legal Aid Provider over Time

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, and geographic coordinates of MSOA population centroids were obtained from the ONS using the 2011 Census boundaries.
Note: Lines show average travel distance to the nearest legal aid provider for three provider types: providers doing housing law (green), providers doing any area of welfare law - housing, debt, benefits (red), and providers doing any area of civil law excluding family-related matters (blue). The vertical dashed lines mark key reform milestones: reform proposals (November 2010), introduction of the Bill in the House of Commons (June 2011), cuts to legal aid fees (October 2011), Royal Assent (May 2012), and full implementation of the Act (April 2013). The **light gray shaded area** indicates the window chosen for the difference-in-differences analysis: from Royal Assent to one financial year post-implementation (Q2 2012 to Q1 2014). The **darker gray shaded area** highlights the period of sharpest increase in travel distance, from Q4 2012 to Q3 2013, centered around the reform implementation. Distance is measured as the straight-line distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the postcode of the nearest legal aid provider. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. Distance is measured in meters.



FIGURE 5: Cumulative Distribution of the Number of Areas of Law Covered by Legal Aid Providers, 2009–2014

Note: This graph shows the number of areas of law covered by legal aid providers in 2009–10 and 2013–14, illustrating a trend toward greater specialisation, with a lower share of providers covering multiple areas of law following the LASPO reform.

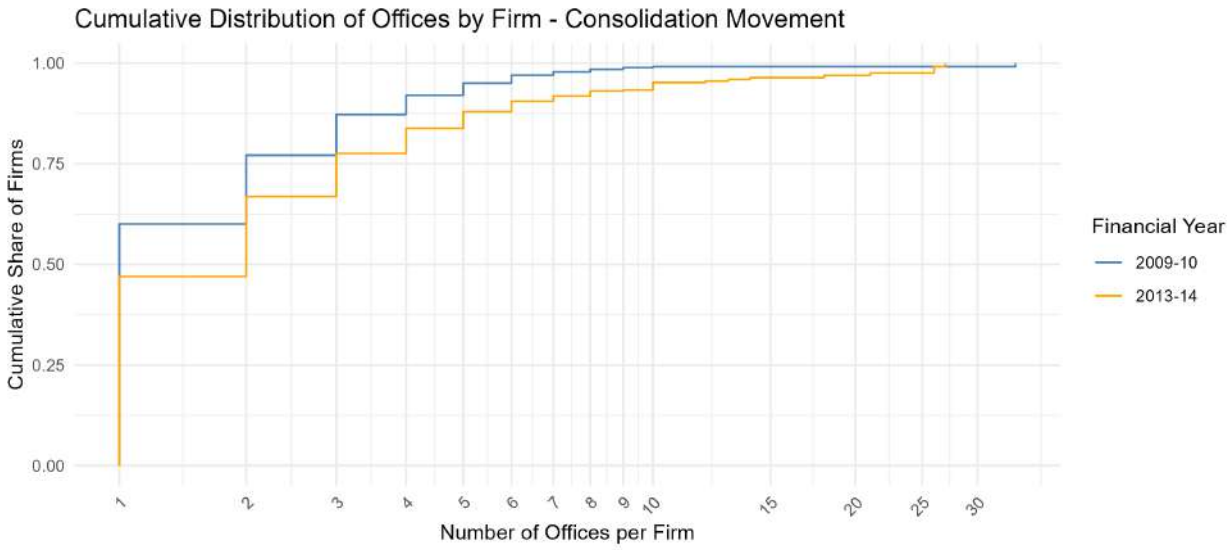


FIGURE 6: Cumulative Distribution of the Number of Provider Offices, 2009–2014

Note: This graph shows the number of offices per legal aid firm in 2009–10 and 2013–14, illustrating a trend toward greater consolidation, with a higher share of providers operating multiple offices following the LASPO reform. An office is defined as a distinct geographical location.

B Additional Figures

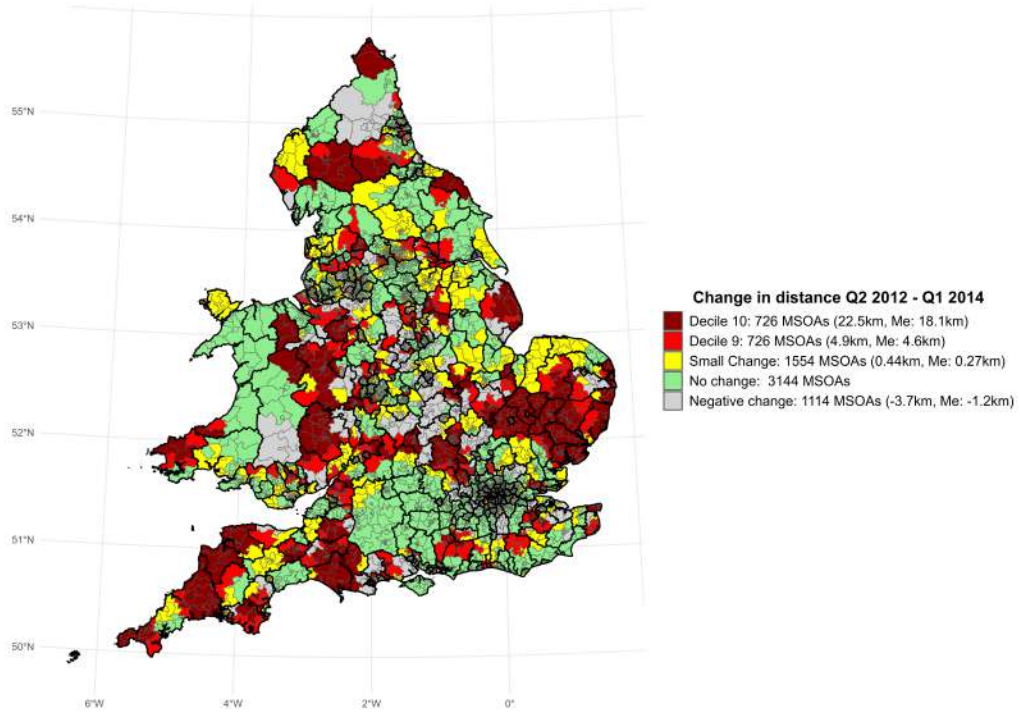


FIGURE 7: Distribution of the Treatment Group across MSOAs - Welfare Providers - Static

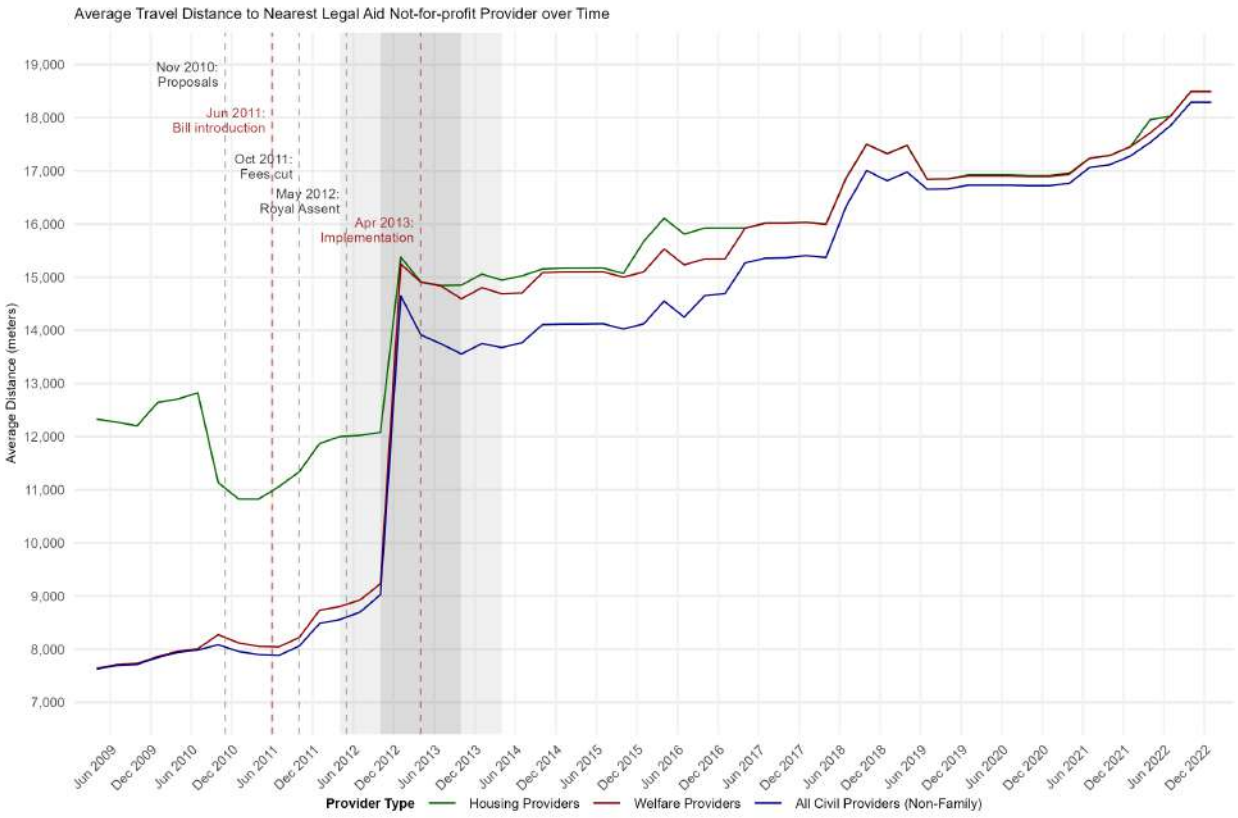


FIGURE 8: Average Distance to Nearest Legal Aid Provider over Time - Not-for-profit

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, and geographic coordinates of MSOA population centroids were obtained from the ONS using the 2011 Census boundaries.
Note: Lines show average travel distance to the nearest legal aid **not-for-profit** provider for three provider types: not-for-profit providers doing housing law (green), not-for-profit providers doing any area of welfare law - housing, debt, benefits (red), and not-for-profit providers doing any area of civil law excluding family-related matters (blue). The vertical dashed lines mark key reform milestones: reform proposals (November 2010), introduction of the Bill in the House of Commons (June 2011), cuts to legal aid fees (October 2011), Royal Assent (May 2012), and full implementation of the Act (April 2013). The light gray shaded area indicates the window chosen for the difference-in-differences analysis: from Royal Assent to one financial year post-implementation (Q2 2012 to Q1 2014). The darker gray shaded area highlights the period of sharpest increase in travel distance, from Q4 2012 to Q3 2013, centered around the reform implementation. Distance is measured as the straight-line distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the postcode of the nearest legal aid provider. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. Distance is measured in meters.

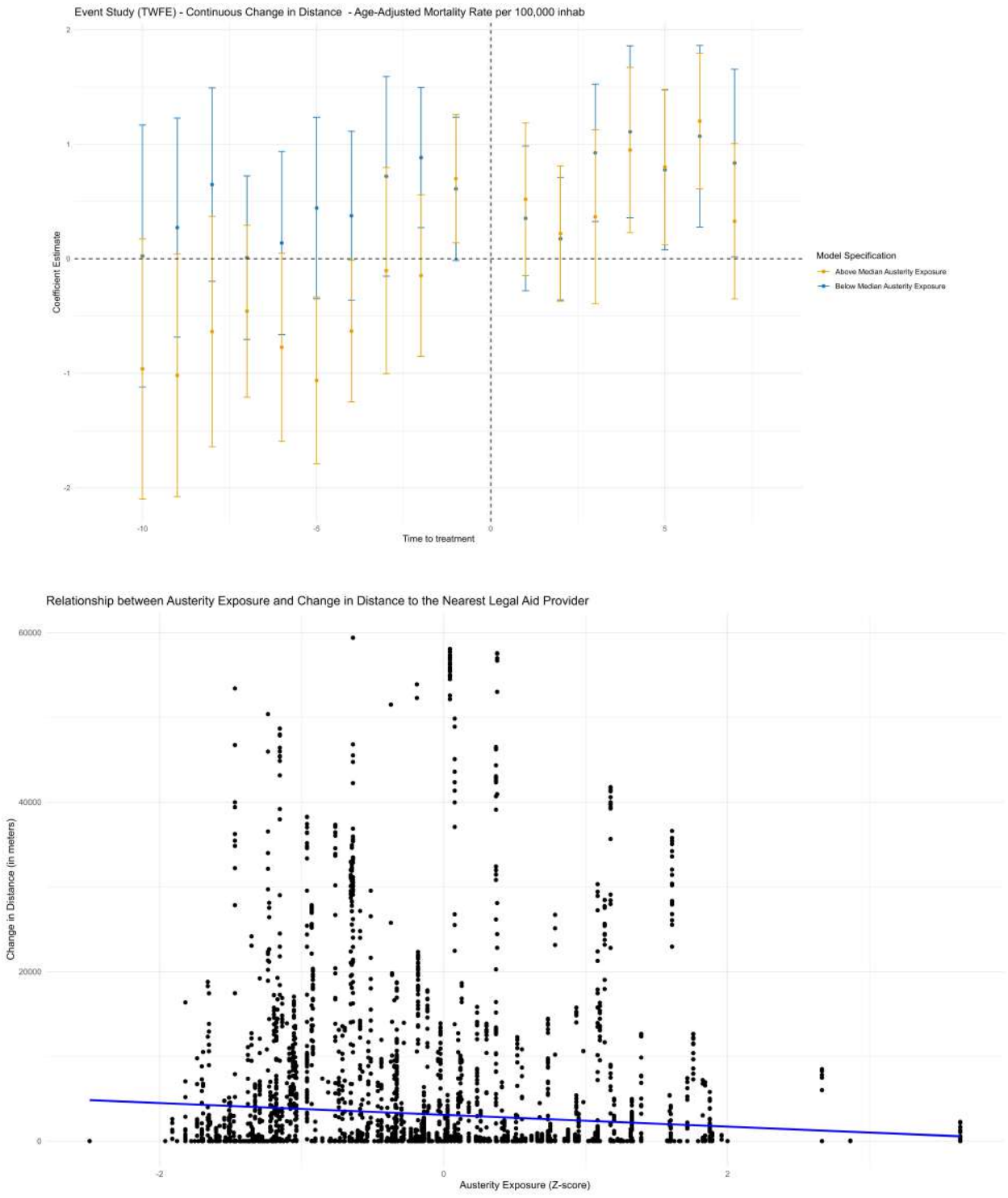


FIGURE 9: Relationship between austerity exposure and change in distance to the nearest legal aid provider

C Tables

TABLE 1: Correlations coefficients between treatment variables based on the type of providers and treatment definitions

Variables	Correlation_Coefficient
Treatment definition 1: Change in Distance (Q1 2013 - Q1 2015)	
Welfare providers vs.Housing providers	0.7544
Welfare providers vs. Civil (non family law) providers	0.4870
Treatment definition 2: Positive Change in Distance Post LASPO (Q2 2013)	
Welfare providers vs.Housing providers	0.7140
Welfare providers vs. Civil (non family law) providers	0.4934

TABLE 2: Covariates Balance Across Geographic Clusters at Baseline

	All Areas				Rural				Urban				Cities			
Variable	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff
Demographics																
Median Age	38.42	41.89	3.47***	(0.48)	44.79	45.89	1.10**	(0.20)	39.18	40.35	1.17***	(0.16)	35.63	39.35	3.72***	(0.60)
% Aged 65 or over	15.94	18.80	2.86***	(0.48)	21.02	21.72	0.69*	(0.15)	16.88	17.85	0.96***	(0.16)	13.38	16.26	2.88***	(0.60)
% Working Age (16-64)	64.26	62.51	-1.75***	(-0.23)	60.45	60.98	0.53	(0.10)	63.92	62.96	-0.96**	(-0.12)	65.83	63.99	-1.83***	(-0.26)
% Children (0-16)	18.67	18.25	-0.41***	(-0.11)	17.40	17.30	-0.10	(-0.04)	18.29	18.51	0.22	(0.06)	19.44	19.27	-0.17	(-0.04)
Ethnicity																
% White	83.50	93.84	10.34***	(0.61)	96.52	97.78	1.26*	(0.16)	90.87	93.09	2.23***	(0.17)	72.18	88.38	16.20***	(0.81)
% Asian	8.17	2.93	-5.24***	(-0.53)	1.02	0.89	-0.13*	(-0.13)	4.75	3.30	-1.45***	(-0.22)	13.78	5.83	-7.95***	(-0.60)
% Black	3.72	1.03	-2.69***	(-0.54)	0.32	0.31	-0.01	(-0.02)	1.31	1.07	-0.24*	(-0.11)	7.15	2.39	-4.76***	(-0.70)
Education																
% Degree or Higher	27.51	25.82	-1.69***	(-0.15)	29.76	29.36	-0.40	(-0.05)	25.20	24.07	-1.13*	(-0.11)	29.00	25.05	-3.96***	(-0.29)
% No Qualifications	22.26	23.14	0.88***	(0.11)	20.79	21.36	0.56	(0.10)	22.43	23.69	1.27***	(0.16)	22.58	24.77	2.20***	(0.24)
% Level 2+ Qual.	57.86	57.96	0.11	(0.01)	61.60	61.68	0.08	(0.01)	57.79	56.38	-1.41**	(-0.12)	56.71	56.20	-0.51	(-0.04)
Employment																
% Retired	13.38	16.03	2.65***	(0.52)	18.10	18.52	0.42	(0.11)	14.15	15.10	0.95***	(0.19)	11.11	14.34	3.23***	(0.72)
% Employed	60.92	63.08	2.17***	(0.24)	63.58	64.38	0.80	(0.12)	61.75	62.85	1.09**	(0.12)	59.25	61.26	2.01***	(0.22)
Housing																
Overcrowding Rate	4917	2586	-2331***	(-0.68)	1275	1330	56	(0.09)	3444	2937	-507**	(-0.19)	6319	3390	-2929***	(-0.78)
% Owner Occup.	58.85	67.54	8.69***	(0.49)	74.54	75.45	0.90	(0.10)	64.15	65.71	1.56	(0.09)	53.38	60.96	7.57***	(0.41)
% Private Rent	18.82	14.35	-4.47***	(-0.45)	12.36	12.28	-0.08	(-0.02)	17.88	15.67	-2.21***	(-0.21)	20.38	12.79	-7.59***	(-0.79)
% Social Rent	19.53	16.22	-3.31***	(-0.25)	10.28	11.01	0.73	(0.14)	15.51	16.53	1.01	(0.08)	23.24	24.05	0.81	(0.05)
LA Housing Stock	58	34	-23***	(-0.22)	18	27	9***	(0.38)	22	31	9***	(0.28)	84	61	-23	(-0.16)
Language																

Continued on next page

TABLE 2 – *Continued from previous page*

	All Areas				Rural				Urban				Cities			
Variable	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff
% Not English	8.40	3.58	-4.82***	(-0.62)	1.55	1.56	0.00	(0.00)	5.39	4.35	-1.04***	(-0.19)	13.53	4.90	-8.63***	(-0.89)
Car Ownership																
% No Car	26.75	19.65	-7.10***	(-0.53)	12.49	11.88	-0.61	(-0.10)	23.14	21.86	-1.28**	(-0.12)	34.86	27.62	-7.24***	(-0.51)
Family Structure																
Lone Parents	234	207	-27***	(-0.25)	153	147	-6	(-0.10)	225	225	-0	(-0.00)	269	264	-6	(-0.04)
Deprivation Indices																
IMD Geo Barriers	-0.09	0.30	0.38***	(0.62)	0.73	0.88	0.14***	(0.27)	0.02	0.10	0.08***	(0.16)	-0.40	-0.13	0.27***	(0.58)
IMD Living Env.	22.31	20.16	-2.15***	(-0.15)	19.06	25.37	6.31***	(0.47)	18.15	17.04	-1.11	(-0.09)	26.75	21.31	-5.44***	(-0.41)
IMD Crime	0.07	-0.24	-0.30***	(-0.47)	-0.62	-0.66	-0.04	(-0.10)	-0.05	-0.08	-0.03	(-0.04)	0.35	0.02	-0.33***	(-0.56)
IMD Overall	21.83	19.19	-2.64***	(-0.21)	13.10	14.92	1.82***	(0.27)	19.51	20.01	0.50	(0.04)	26.18	24.65	-1.53	(-0.11)
IMD Health	-0.01	-0.12	-0.11***	(-0.14)	-0.60	-0.55	0.05	(0.09)	-0.08	-0.01	0.06	(0.09)	0.20	0.35	0.15**	(0.19)
IMD Income	0.15	0.13	-0.02***	(-0.24)	0.08	0.09	0.01*	(0.15)	0.13	0.14	0.00	(0.05)	0.18	0.17	-0.01	(-0.07)
IMD Employment	0.12	0.11	-0.01***	(-0.13)	0.08	0.08	0.00	(0.11)	0.11	0.12	0.01	(0.08)	0.13	0.14	0.01	(0.12)
IMD Housing	21.90	21.81	-0.09	(-0.01)	25.09	28.27	3.18***	(0.38)	18.80	19.20	0.39	(0.07)	23.67	18.58	-5.09***	(-0.59)
IMD Education	21.13	20.96	-0.17	(-0.01)	13.53	14.49	0.96	(0.10)	22.62	23.89	1.27	(0.08)	21.93	23.02	1.09	(0.07)
Health and Disability																
% Providing Care	10.09	10.95	0.87***	(0.40)	11.51	11.69	0.18	(0.11)	10.22	10.53	0.31**	(0.14)	9.50	11.03	1.53***	(0.71)
% Disabled	17.59	18.72	1.13***	(0.23)	18.13	18.40	0.27	(0.06)	18.15	18.76	0.61**	(0.12)	16.88	19.23	2.35***	(0.51)
% Poor Health	5.58	5.52	-0.06	(-0.03)	4.92	4.92	0.01	(0.00)	5.55	5.64	0.09	(0.04)	5.82	6.32	0.49**	(0.23)
Migration																
% UK Born	84.41	92.00	7.59***	(0.54)	93.87	94.79	0.92	(0.11)	89.27	90.92	1.65**	(0.14)	76.65	90.22	13.56***	(0.85)
% Non-EU Born	9.89	4.47	-5.42***	(-0.65)	2.87	3.04	0.17	(0.08)	6.00	4.78	-1.21***	(-0.24)	15.91	6.29	-9.62***	(-0.91)
Benefits and Poverty																

Continued on next page

TABLE 2 – *Continued from previous page*

	All Areas				Rural				Urban				Cities			
Variable	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff
Housing Benefits Claimants	8216	6948	-1267***	(-0.26)	4620	4779	159	(0.07)	7717	7627	-90	(-0.02)	9868	8914	-955*	(-0.17)
% in Poverty	21.91	19.11	-2.80***	(-0.34)	15.91	17.10	1.19***	(0.26)	20.59	19.92	-0.67*	(-0.09)	25.14	20.20	-4.93***	(-0.53)
Net Inc Before HC	27.4k	26.6k	-754***	(-0.13)	28.1k	27.8k	-236	(-0.05)	26.6k	26.1k	-459*	(-0.09)	27.9k	25.9k	-1964***	(-0.27)
Net Inc After HC	24.0k	24.0k	-36	(-0.01)	25.8k	25.7k	-178	(-0.04)	23.5k	23.2k	-310	(-0.06)	23.8k	23.1k	-685	(-0.10)
Economic Conditions																
Avg House Price	218k	199k	-19k***	(-0.16)	259k	251k	-8k	(-0.09)	188k	178k	-10k**	(-0.13)	233k	173k	-60k***	(-0.39)
Affordability	7.78	7.54	-0.24*	(-0.07)	9.11	9.29	0.18	(0.06)	6.98	6.89	-0.09	(-0.04)	8.10	6.49	-1.61***	(-0.44)
Fuel Poverty Rate	7161	8250	1089***	(0.33)	8513	10056	1543***	(0.43)	6633	7215	582***	(0.19)	7244	8481	1237***	(0.45)
Mortality																
Death Rate	8743	9765	1023***	(0.32)	9760	9871	111	(0.04)	9391	9819	428**	(0.12)	7788	9333	1545***	(0.52)
Age-Standardized Mortality	9112	8855	-257***	(-0.12)	8197	8087	-110	(-0.07)	9088	9103	15	(0.01)	9433	9531	98	(0.04)
Housing Stress (Local Authority Level)																
Social Housing Wait List	3611	2628	-982***	(-0.45)	2756	2491	-266*	(-0.17)	3111	2528	-584***	(-0.33)	4225	3164	-1061***	(-0.43)
Rough Sleepers	35	12	-23***	(-0.27)	8	6	-2	(-0.09)	16	10	-6***	(-0.19)	58	29	-29***	(-0.25)
Homeless Reviews	7631	5105	-2526***	(-0.22)	2409	2811	402	(0.10)	3398	2856	-542**	(-0.15)	12347	15825	3478*	(0.18)
Homeless Decisions	3626	2479	-1146***	(-0.17)	1061	1275	214*	(0.16)	1558	1430	-128	(-0.08)	5932	7693	1761	(0.14)
Financial Distress (Local Authority Level)																
Bankruptcy Rate	3931	4612	682***	(0.21)	3383	4588	1205***	(0.35)	3516	4200	684***	(0.25)	4515	6266	1750***	(0.46)
Insolvency Rate	7197	8189	992***	(0.17)	6134	7991	1857***	(0.31)	6385	7394	1008***	(0.21)	8338	11696	3358***	(0.46)
Eviction Proceedings																
Possession Claims	259	178	-80***	(-0.37)	102	99	-3	(-0.03)	203	203	0	(0.00)	363	250	-113***	(-0.41)
Outright Orders	117	80	-38***	(-0.37)	45	42	-3	(-0.07)	98	96	-3	(-0.03)	159	97	-62***	(-0.53)
Suspended Orders	89	81	-8*	(-0.08)	48	49	1	(0.02)	88	94	6	(0.05)	104	101	-3	(-0.03)

Continued on next page

TABLE 2 – *Continued from previous page*

	All Areas				Rural				Urban				Cities			
Variable	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff	Control	Treated	Diff	Std. Diff
Legal Aid Provision																
% Generalist	53.7	50.1	-3.6***	(-0.56)	51.2	50.2	-0.9***	(-0.27)	51.7	49.7	-2.0***	(-0.34)	56.4	51.1	-5.2***	(-0.69)
% Poverty Law	83.5	82.6	-0.8***	(-0.30)	82.8	82.9	0.0	(0.01)	82.9	82.6	-0.3*	(-0.11)	84.2	82.2	-2.0***	(-0.69)
% NFP	19.2	19.5	0.3**	(0.08)	18.8	19.3	0.5***	(0.31)	19.3	19.6	0.3	(0.08)	19.1	19.2	0.1	(0.03)
% Interconnected	67.4	67.3	-0.1	(-0.03)	67.5	67.7	0.3	(0.11)	68.0	67.5	-0.5*	(-0.11)	66.8	65.5	-1.4***	(-0.31)
Total Volume	1048	663	-385***	(-0.68)	595	536	-59***	(-0.29)	701	646	-55***	(-0.25)	1530	993	-536***	(-0.77)
Poverty Law Vol	886	548	-338***	(-0.67)	494	445	-49***	(-0.29)	583	533	-49***	(-0.26)	1306	820	-486***	(-0.78)
Interconnect Vol	712	445	-267***	(-0.66)	402	363	-39***	(-0.28)	479	436	-44***	(-0.27)	1037	653	-384***	(-0.76)
Generalist Vol	597	337	-260***	(-0.66)	309	272	-37***	(-0.31)	366	324	-42***	(-0.31)	914	523	-391***	(-0.77)
NFP Volume	200	130	-70***	(-0.60)	112	104	-8**	(-0.21)	136	128	-9***	(-0.15)	289	194	-96***	(-0.65)
NFP Poverty Vol	197	129	-69***	(-0.60)	111	103	-8**	(-0.21)	135	126	-9***	(-0.15)	285	192	-93***	(-0.64)
N (Observations)	3,146	1,452			446	441			1,322	802			1,373	208		

Sources: Geographic classifications are based on the Office for National Statistics (ONS) Rural-Urban Classification from the 2011 Census. Demographics, ethnicity, education, employment, housing tenure, language, car ownership, health, disability, and migration variables are from the 2011 UK Census. Index of Multiple Deprivation measures are from the 2015 release based on 2011-12 administrative data. The number of housing benefits claimants is from StatExplore and measured in 2011. The share of households in poverty is derived from the Small area model-based households in poverty estimates for the financial year ending in 2012. Net income measures are equivalised household means from Small Area Income Estimates for the financial year 2011-12. The Median price paid for residential property is derived from the House price statistics for small areas in England and Wales measured in 2011. Fuel poverty rates come from Fuel Poverty Statistics for 2009. Mortality data are from 2009. Housing Stress and Financial Stress variables are defined at the local authority level and derived from the Local Authority Housing Statistics, the Statutory Homelessness Statistics and the UK's Insolvency Service. Eviction proceedings data are from 2009 calendar year court records obtained through Freedom of Information requests. Legal aid provision measures are from the Legal Aid Statistics for the 2009-10 financial year.

Notes: This table presents covariate balance between treated and control areas across different geographic settings based on 2021 MSOA boundaries

at baseline. Treatment is defined as being in the top quintile of the distribution of changes in distance to the nearest legal aid provider between Q2 2012 and Q1 2014. Control areas experienced no change in distance during this period. Areas not in either group are excluded from the analysis. *Rural* areas include: Rural town and fringe, and Rural village and dispersed (both including sparse settings). *Urban* areas include: Urban city and town (including sparse settings). *Cities* include: Urban major conurbation and Urban minor conurbation. The following variables are expressed as rates per 100,000 inhabitants: Overcrowding Rate, Housing Benefits Claimants, Fuel Poverty Rate, Death Rate, Age-Standardized Mortality, Housing Wait List, Homelessness Rate, Rough Sleepers, Homeless Decisions, Total Homelessness, Bankruptcy Rate, Insolvency Rate, Possession Claims, Outright Orders, Suspended Orders, and all Legal Aid Volume measures. LA Housing Stock represents the number of social housing units in the Local Authority containing each MSOA. The housing affordability ratio follows ONS methodology as the ratio of house price to net income before housing costs. Legal aid provision measures represent distance-weighted volumes at the MSOA level, calculated as the sum across all providers of the product of inverse distance from the MSOA population-weighted centroid to each provider multiplied by that provider's volume in the 2009-10 financial year. Provider shares are relative to total civil legal aid cases. Poverty Law providers handle housing, welfare benefits, and debt cases; Interconnected providers handle at least two of these three areas; Generalist providers handle at least three distinct areas of civil law; and NFP denotes not-for-profit providers. The Index of Multiple Deprivation (IMD) measures comprise: *IMD Geographic Barriers* (standardized) measures access to services (GP surgeries, primary schools, post offices, supermarkets), with positive values indicating worse access. *IMD Living Environment* (0-100) captures housing quality, air quality, and road traffic accidents. *IMD Crime* (standardized) measures crime rates, with negative values indicating lower crime. *IMD Health* (standardized) captures mortality, disability, emergency service use, and mental health, with negative values indicating better health outcomes. *IMD Income* (proportion) shows the share receiving income-related benefits. *IMD Employment* (proportion) shows the share of working-age population excluded from employment. *IMD Housing Barriers* (0-100) measures overcrowding, homelessness, and housing affordability. *IMD Education* (0-100) captures educational attainment and English proficiency. *IMD Overall Score* (0-100) is the composite deprivation measure across all seven domains, with lower scores indicating less deprivation.

For each variable, we report the control mean, treated mean, their difference, and the standardized difference (Cohen's d) in parentheses. Standardized differences above 0.25 in absolute value indicate meaningful imbalance. Statistical significance based on t-tests: *** p<0.001, ** p<0.01, * p<0.05.

TABLE 3: Summary statistics - Deciles of the Treatment Variable

Decile	Min	Q1	Mean	SD	Q3	Max
1	-62762.41	-6974.65	-5566.35	6954.59	-1276.71	-425.23
2	-419.59	-158.95	-88.23	118.16	0.00	0.00
3	0.00	0.00	0.00	0.00	0.00	0.00
4	0.00	0.00	0.00	0.00	0.00	0.00
5	0.00	0.00	0.00	0.00	0.00	0.00
6	0.00	0.00	1.93	5.51	0.00	27.03
7	27.11	76.15	145.13	78.68	212.26	299.38
8	300.08	461.56	791.44	427.30	1075.98	1835.29
9	1838.14	2831.60	4939.24	2279.93	6552.81	9654.72
10	9660.84	13157.31	22481.88	11959.34	29452.43	58433.10

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, geographic coordinates of MSOA population centroids were obtained from the ONS using the 2021 Census boundaries.
Note: The treatment variable represents the change in distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the nearest legal aid provider. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. The change in distance is measured between Q2 2012 and Q1 2014. Distance is measured in meters.

TABLE 4: Summary statistics - Treatment Intensity Groups - Welfare Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	9660.84	13157.31	22481.88	11959.34	29452.43	58433.10	726
Decile 9	1838.14	2831.60	4939.24	2279.93	6552.81	9654.72	726
Small Change	0.08	105.37	438.45	445.48	587.21	1835.29	1554
Negative Change	-62762.41	-4175.39	-3690.20	6178.34	-237.95	-0.66	1114
Null Change	0.00	0.00	0.00	0.00	0.00	0.00	3144

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, geographic coordinates of MSOA population centroids were obtained from the ONS using the 2021 Census boundaries.
Note: The treatment variable represents the change in distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the nearest legal aid provider. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. The change in distance is measured between Q2 2012 and Q1 2014. Distance is measured in meters. A small change means that the change in distance is strictly positive but does not belong to the 5th quintile of the change-in-distance variable distribution.

TABLE 5: Pooled Difference-in-Differences Estimates using a Continuous Definition of the Change in Distance Variable

	Eviction claims		Eviction orders		Age-adjusted Mortality	
Change in Distance in km*Post_2012	**0.35*** (0.090)**	**0.386*** (0.098)**	**0.179** (0.054)**	**0.200*** (0.056)**	**0.646*** (0.182)**	**0.686*** (0.196)**
Mean at Baseline (rate per 100,000 inhab.)	76.34	83.67	53.86	58.62	961.23	974.85
Average Effect (if linear) Mean Change in Distance: 3.2km / 4km	1.2	1.54	0.57	0.8	2.07	2.74
MSOA + Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Local Authority FE	No	Yes	No	Yes	No	Yes
Nb. Observations	586,109	494,468	586,109	494,468	109,818	92,664

TABLE 6: Heterogeneity analysis - Pooled DiD estimates using the Continuous Change in Distance definition by austerity exposure

	Eviction claims	Eviction orders	Age-adjusted mortality
Mean at Baseline (rate per 100,000 inh.) [Pre-reform average outcome level]	67.12	48.40	944.2
Change in Distance * Austerity [Pre-reform differences in levels]	-0.593*** (0.169)	-0.320** (0.112)	-0.704 (0.411)
Austerity exposure [Effect of austerity alone]	22.18*** (2.897)	15.02*** (1.659)	78.55*** (7.197)
Change in Distance *Post [Main treatment effect]	**0.246**** (0.082)	**0.124*** (0.052)	**0.655***** (0.171)
Austerity *Post [Effet of austerity alone after reform]	-8.679*** (1.469)	-5.023*** (0.837)	1.519 (2.808)
Change in Distance * Post * Austerity [Heterogeneous effect by austerity]	0.362*** (0.079)	0.174*** (0.051)	0.581** (0.221)
Nb. Observations	562,516	562,516	105,390
MSOA + Time Fixed Effects	Yes	Yes	Yes

D Additional Results

D.1 Treatment Intensity and Concentration

D.1.1 Treatment Distribution

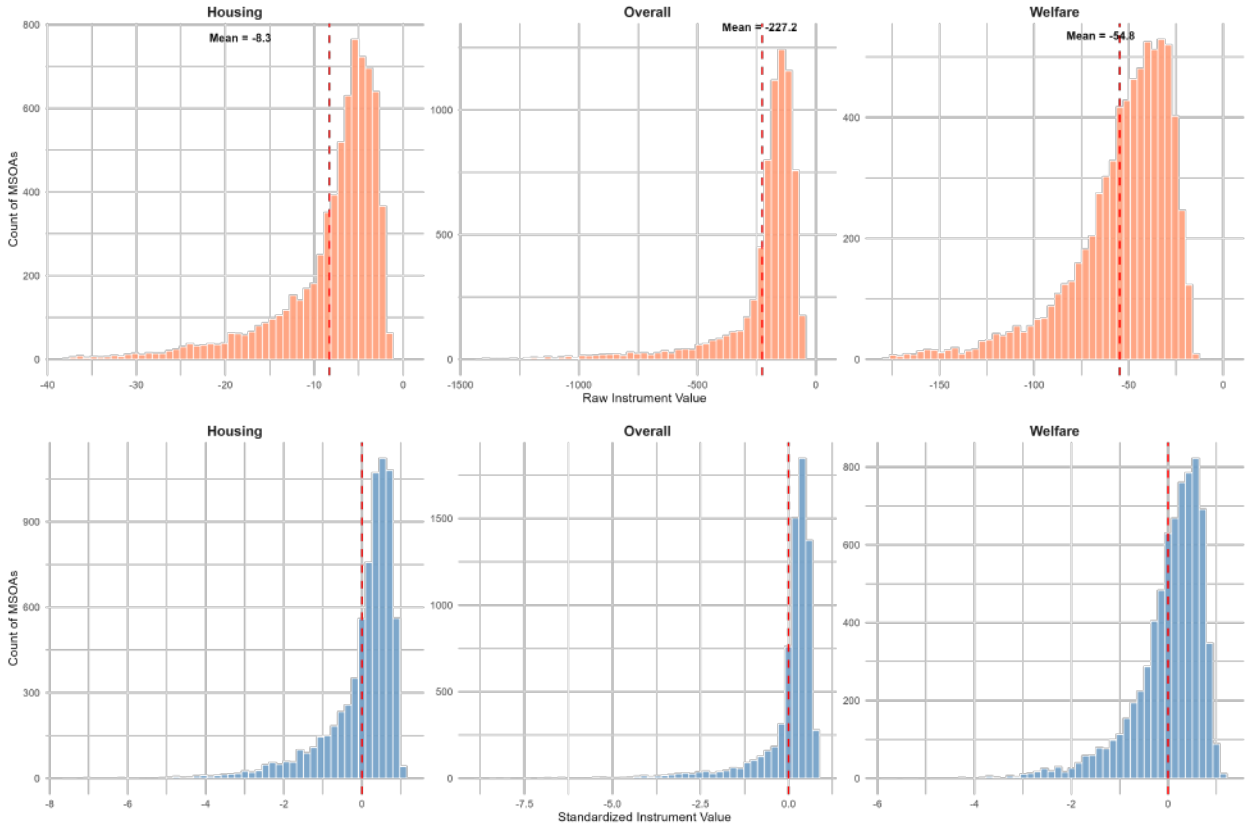


FIGURE 10: Distribution of the Shift-Share Instruments across MSOAs (Raw vs. Standardized Values)

Source: Ministry of Justice Legal Aid Statistics, author's calculations.
Note: This figure shows the distribution of our shift-share instruments across MSOAs. The top panel plots raw values; the bottom panel shows the same instruments standardized to mean zero and unit variance. We display three variants: one specific to housing law; one aggregated across welfare-related areas (housing, welfare benefits, and debt); and one aggregated across all fifteen areas of civil law. Instrument values are consistently negative, reflecting predicted reductions in legal aid availability due to the reform. More negative values indicate larger predicted cuts, based on pre-reform caseload composition, provider specialization patterns, and Ministry of Justice–predicted shocks to legal aid provision. The standardized versions exhibit right-skewed distributions with long left tails, indicating that while most areas saw moderate cuts relative to the mean, some experienced severe predicted reductions exceeding two standard deviations.

D.1.2 Treatment Spatial Concentration

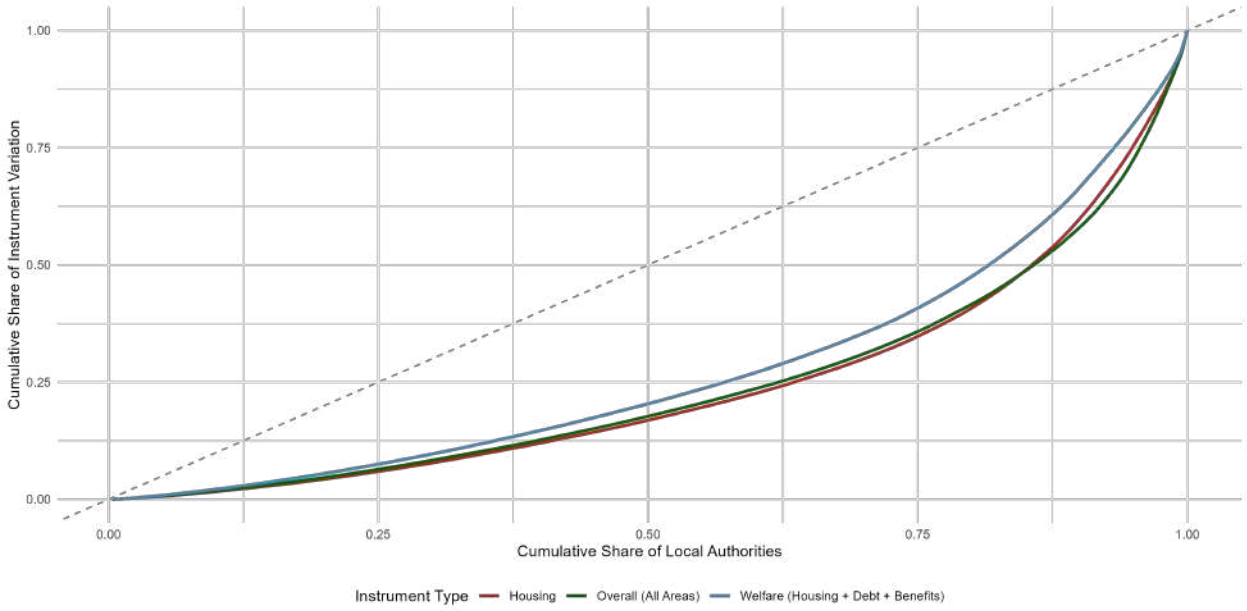


FIGURE 11: Geographic Concentration of Predicted Legal Aid Cuts

Source: Ministry of Justice Legal Aid Statistics, author's calculations.
Note: This figure displays Lorenz curves showing the geographic concentration of three instrument variants across 331 local authorities. The x-axis represents the cumulative share of local authorities by ascending contribution, while the y-axis shows the cumulative share of total instrument variation. The 45-degree dashed line represents perfect equality; greater deviation from this line indicates higher concentration. We display three instrument variants: one specific to housing law; one aggregated across welfare-related areas (housing, welfare benefits, and debt); and one aggregated across all fifteen areas of civil law. All instruments exhibit high levels of geographical concentration, with the top 10% of local authorities capturing respectively 42% (Gini=0.52), 35% (Gini=0.45) and 43% (Gini=0.51) of the instrument variation.

D.2 Static DiD

D.2.1 Event-studies

D.2.2 Binary Definition of Change in Distance - top Quintile vs Areas with No Change in Distance

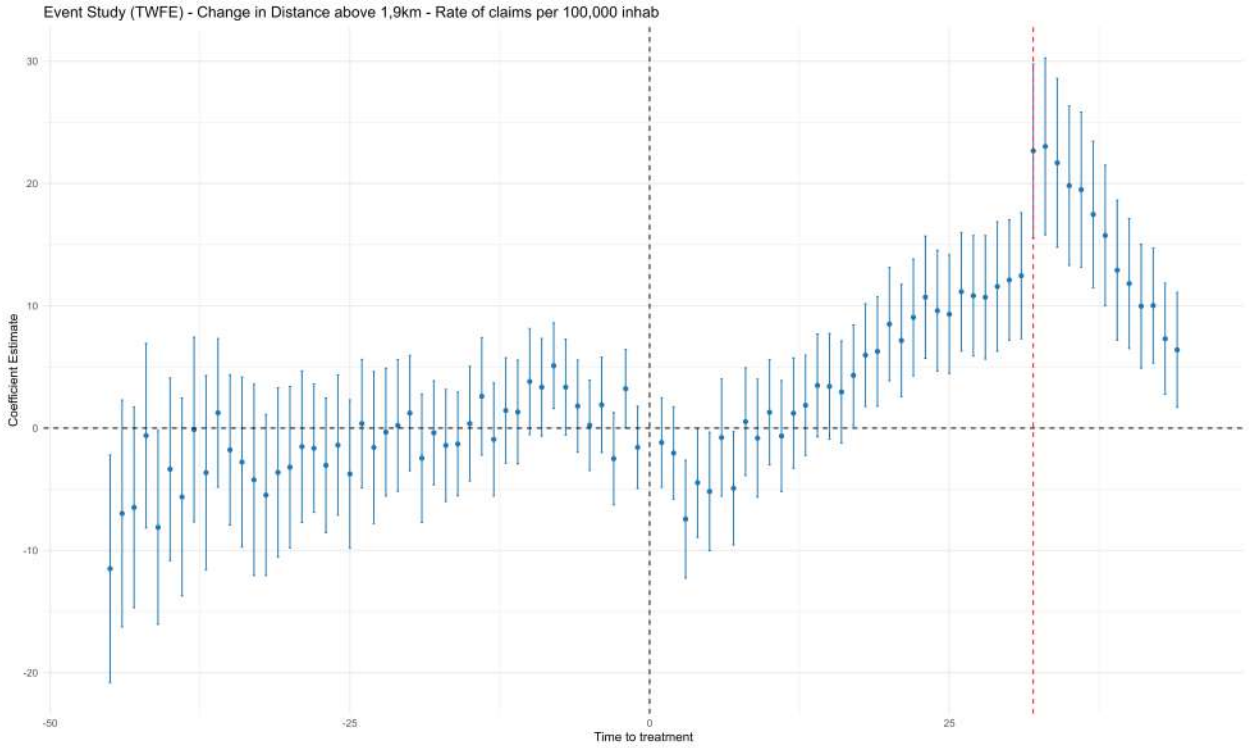


FIGURE 12: Eviction claim rate per 100,000 inhabitants - Static - Change in Distance >1.9km

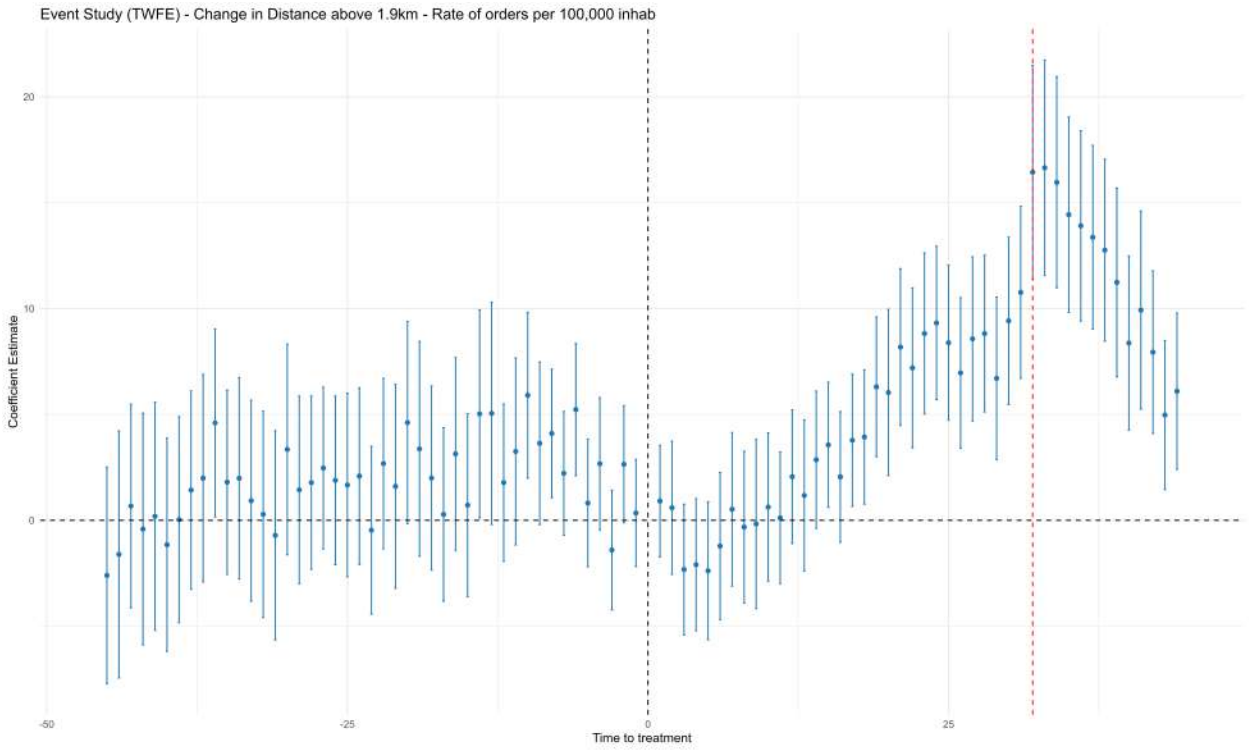


FIGURE 13: Eviction orders per 100,000 inhabitants - Static - Change in Distance >1.9km

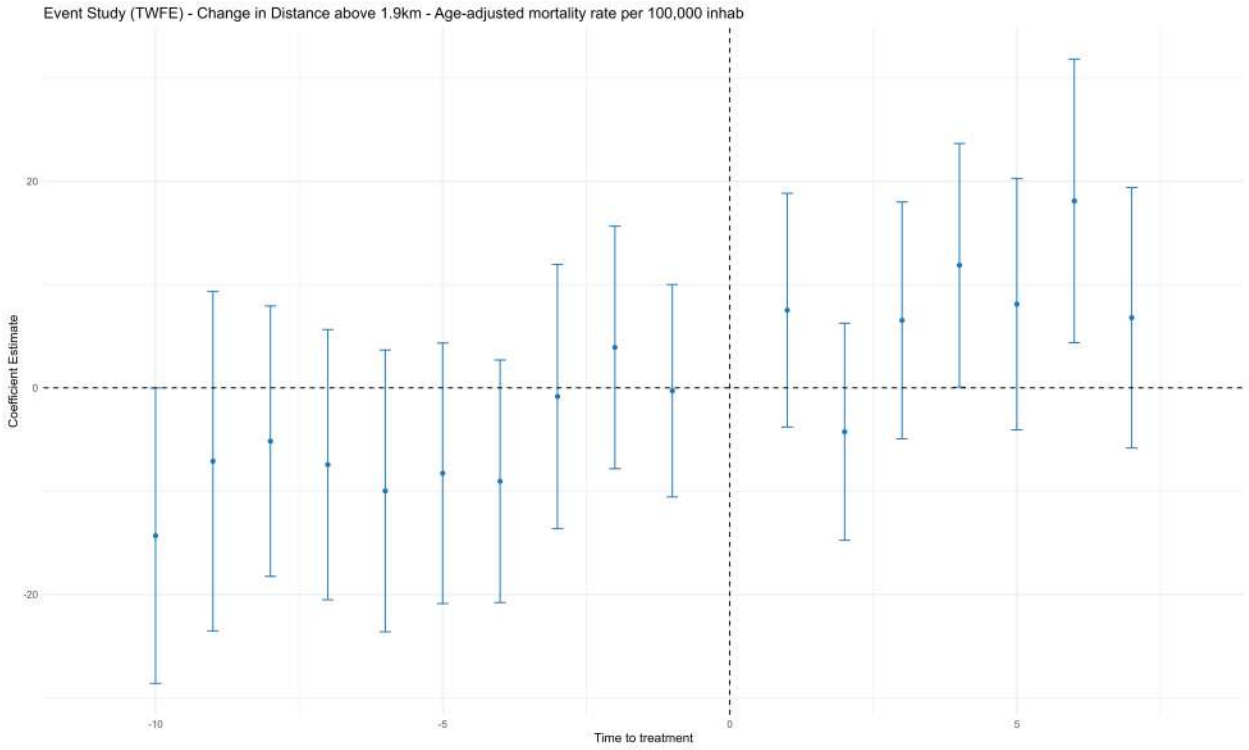


FIGURE 14: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Change in Distance >1.9km

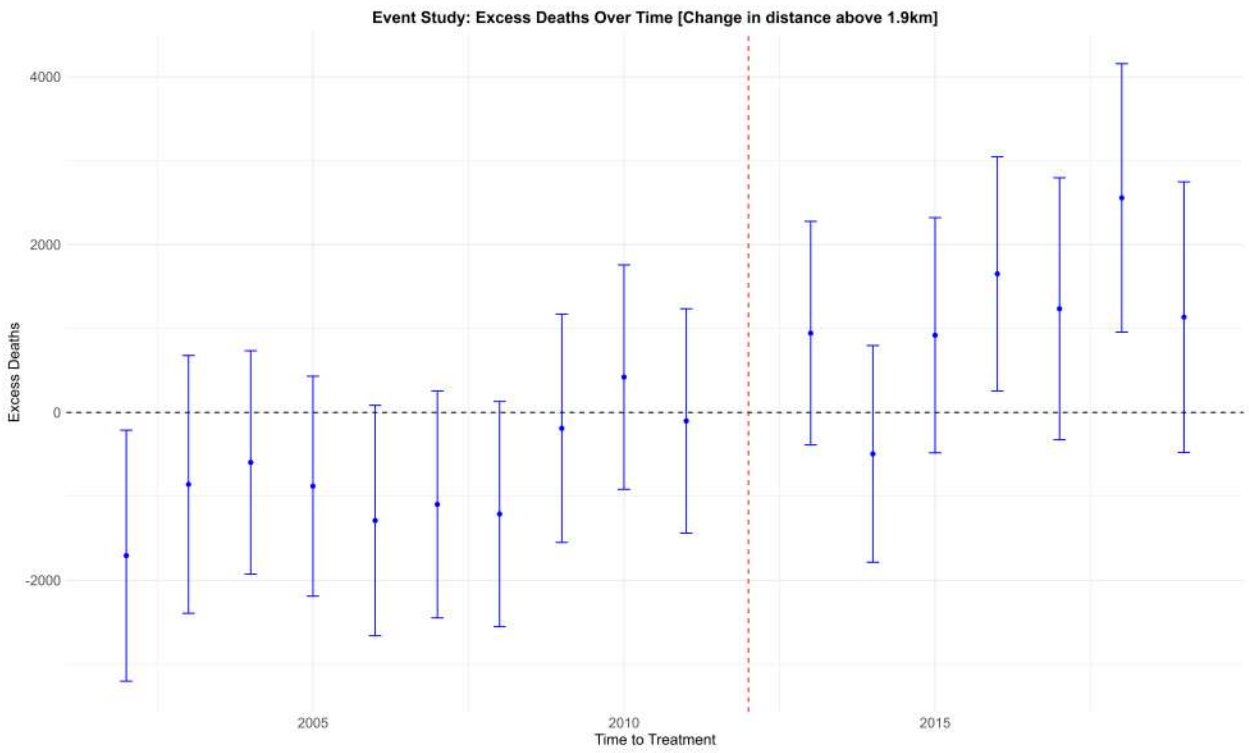


FIGURE 15: Excess deaths - Static - Change in Distance >1.9km

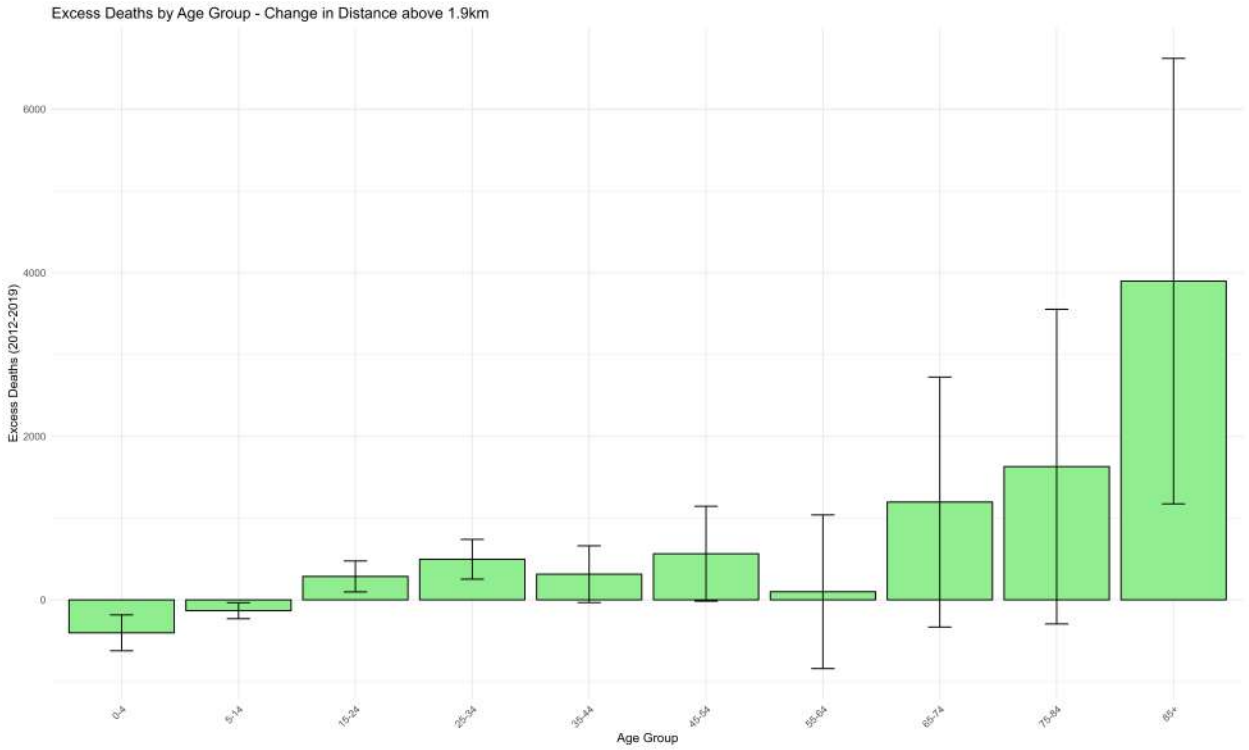


FIGURE 16: Pooled ATT Age-Adjusted mortality by Age Group - Change in Distance >1.9km

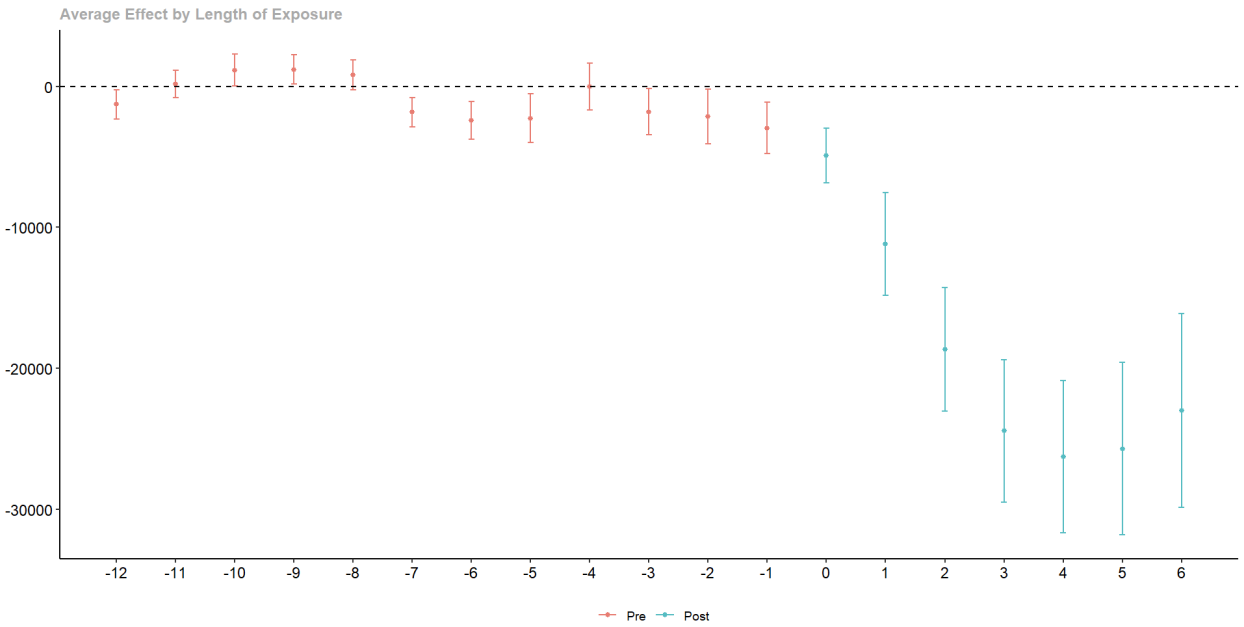


FIGURE 17: Mean House prices - Static

D.2.3 Binary Definition of Change in Distance - by Intensity of Treatment vs Areas with No Change in Distance

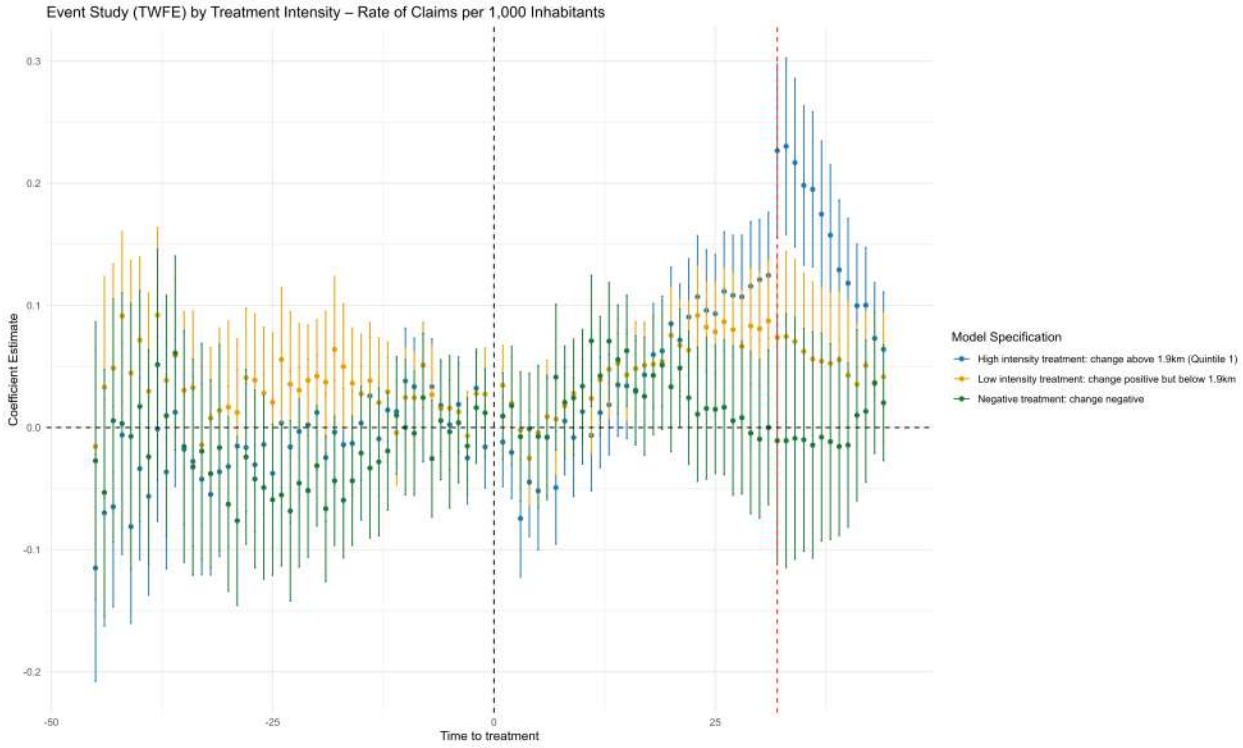


FIGURE 18: Eviction claim rate per 100,000 inhabitants - Static - Intensity of Change in Distance

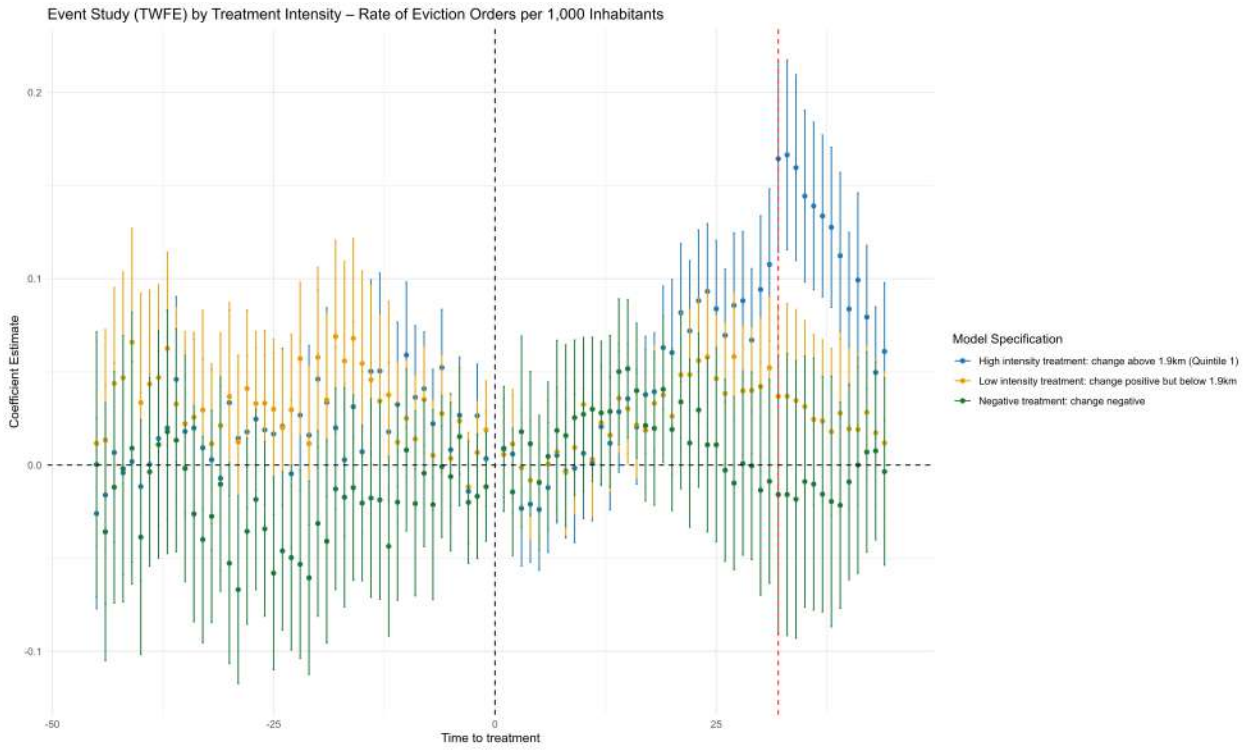


FIGURE 19: Eviction orders per 100,000 inhabitants - Static - Intensity of Change in Distance

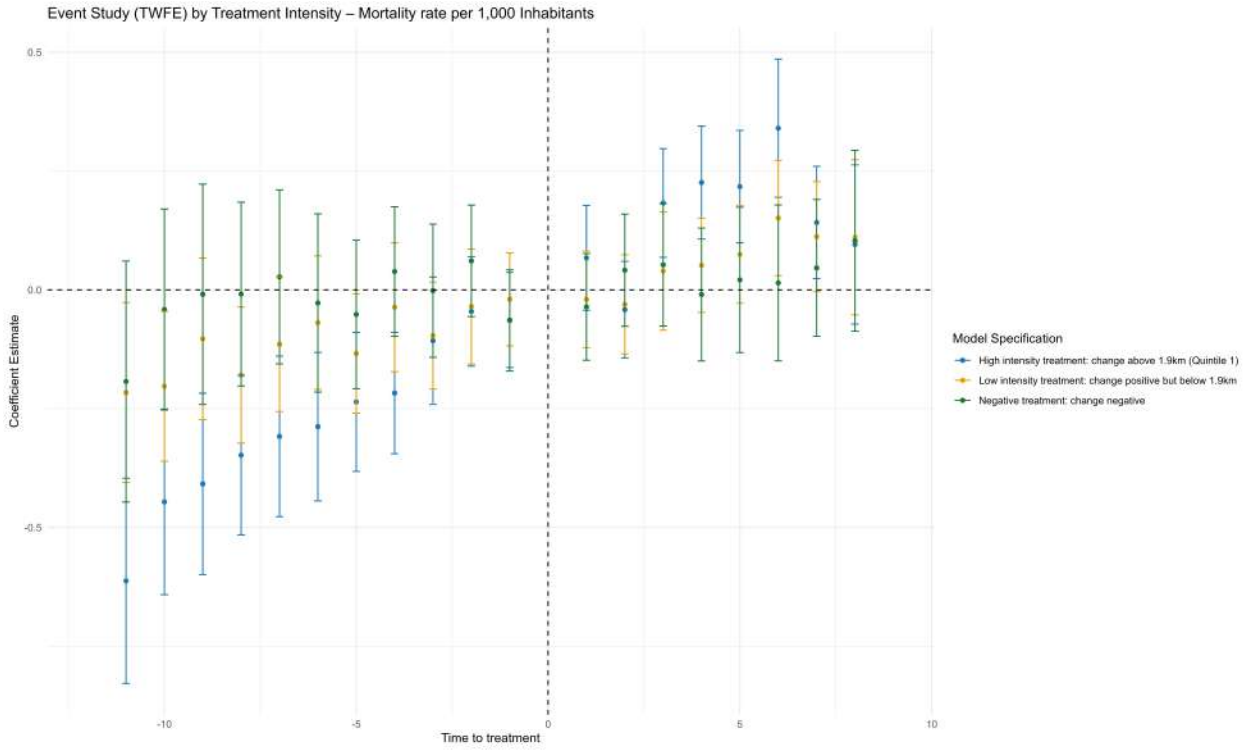


FIGURE 20: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Intensity of Change in Distance

D.2.4 Continuous Definition of Change in Distance

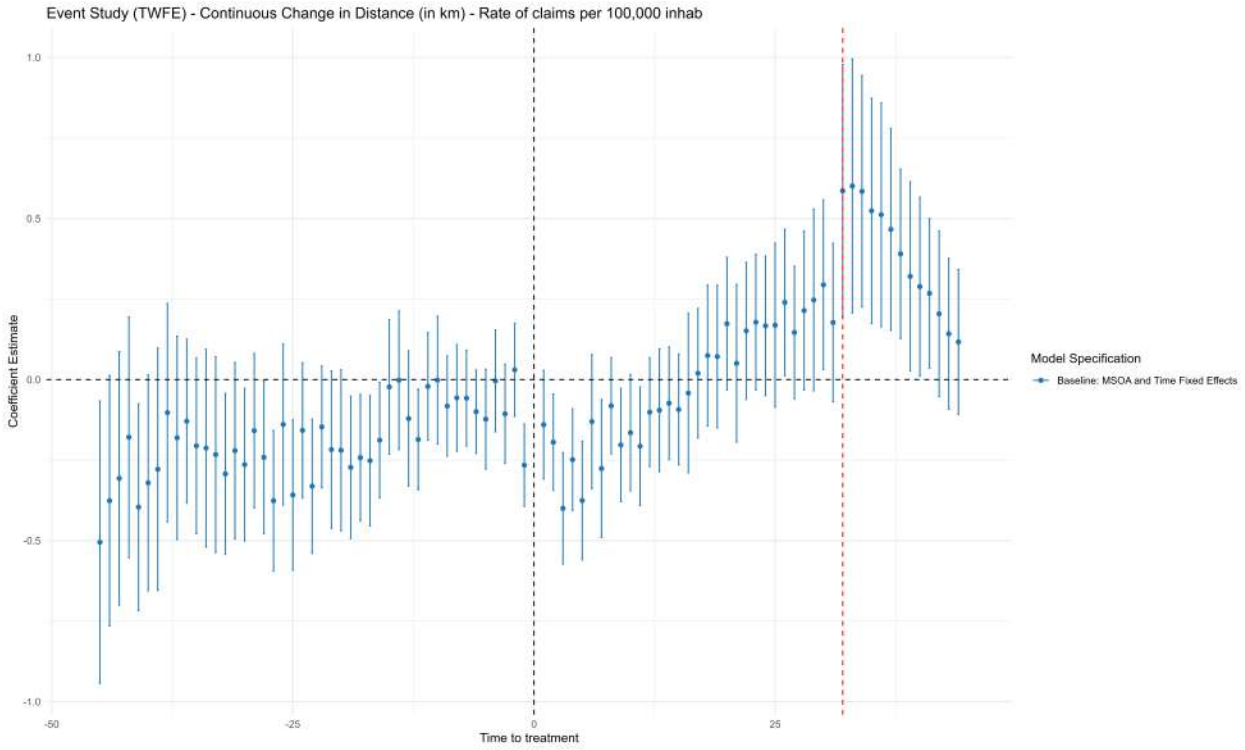


FIGURE 21: Eviction claim rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

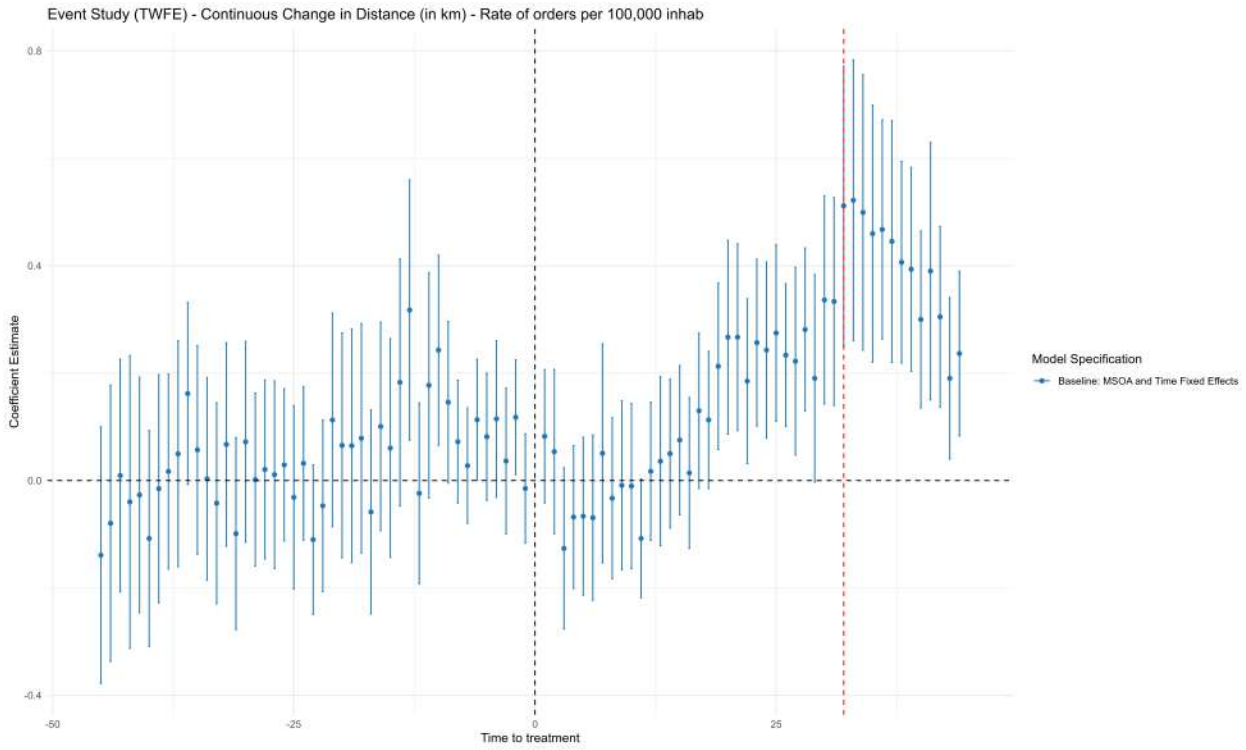


FIGURE 22: Eviction orders per 100,000 inhabitants - Static - Continuous Change in Distance in km

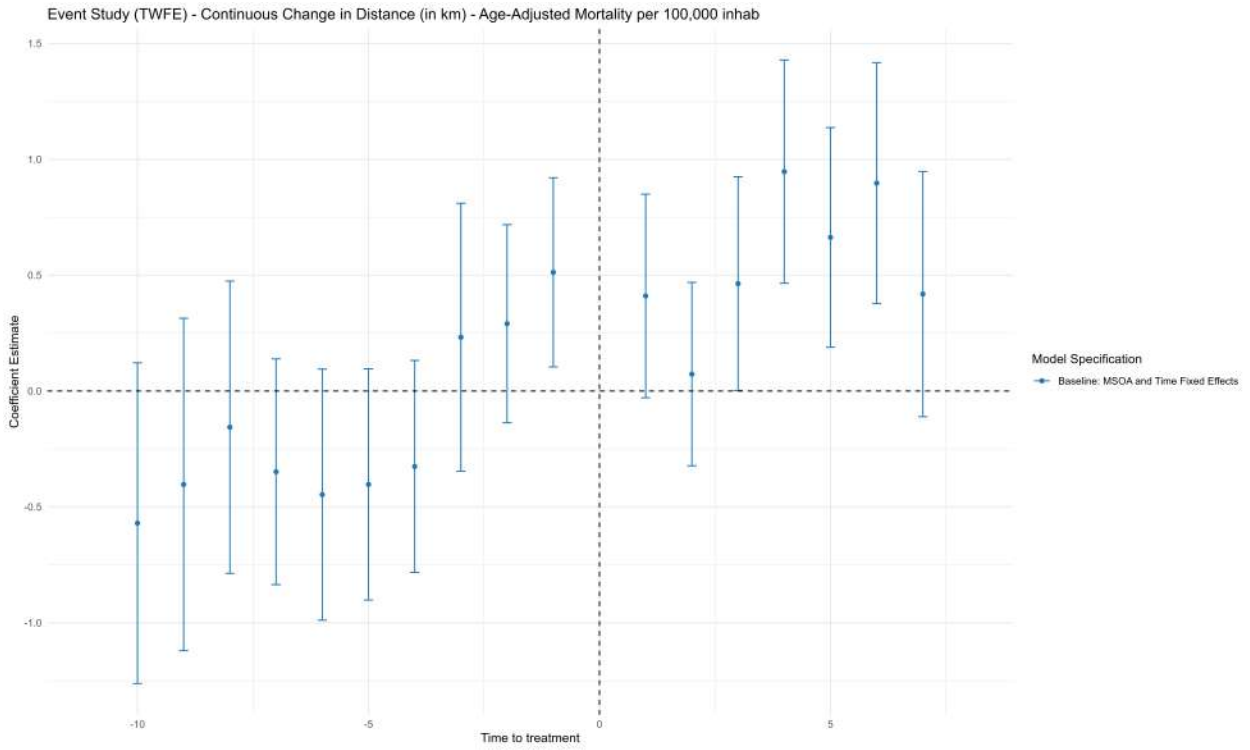


FIGURE 23: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

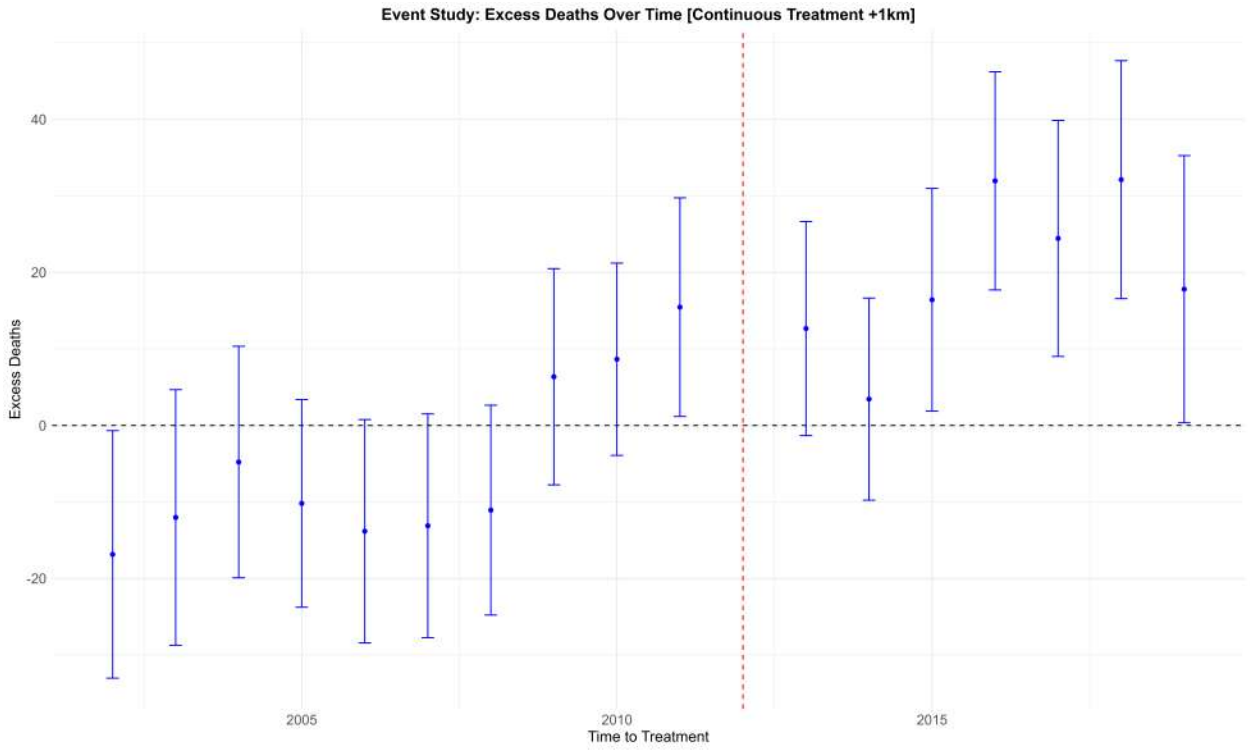


FIGURE 24: Excess deaths - Static - Continuous Change in Distance

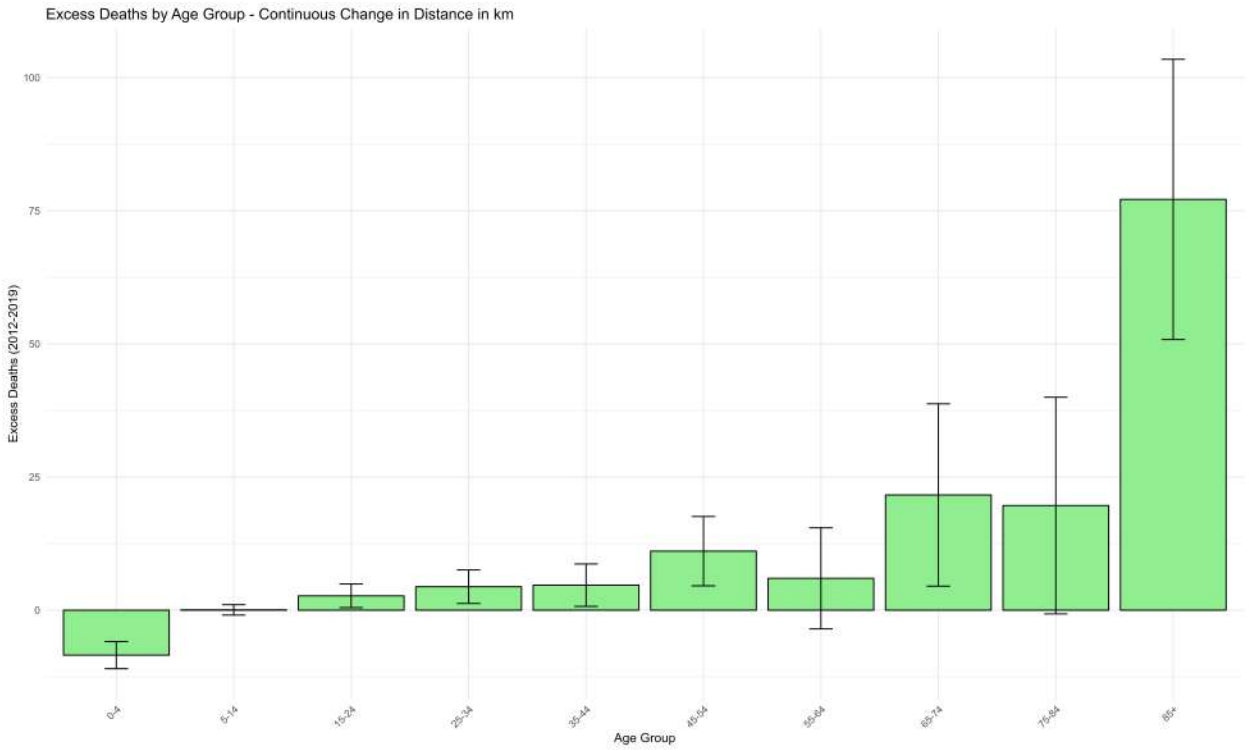


FIGURE 25: Pooled ATT Age-Adjusted mortality by Age Group - Continuous Change in Distance

D.2.5 With Local Authority Fixed Effects

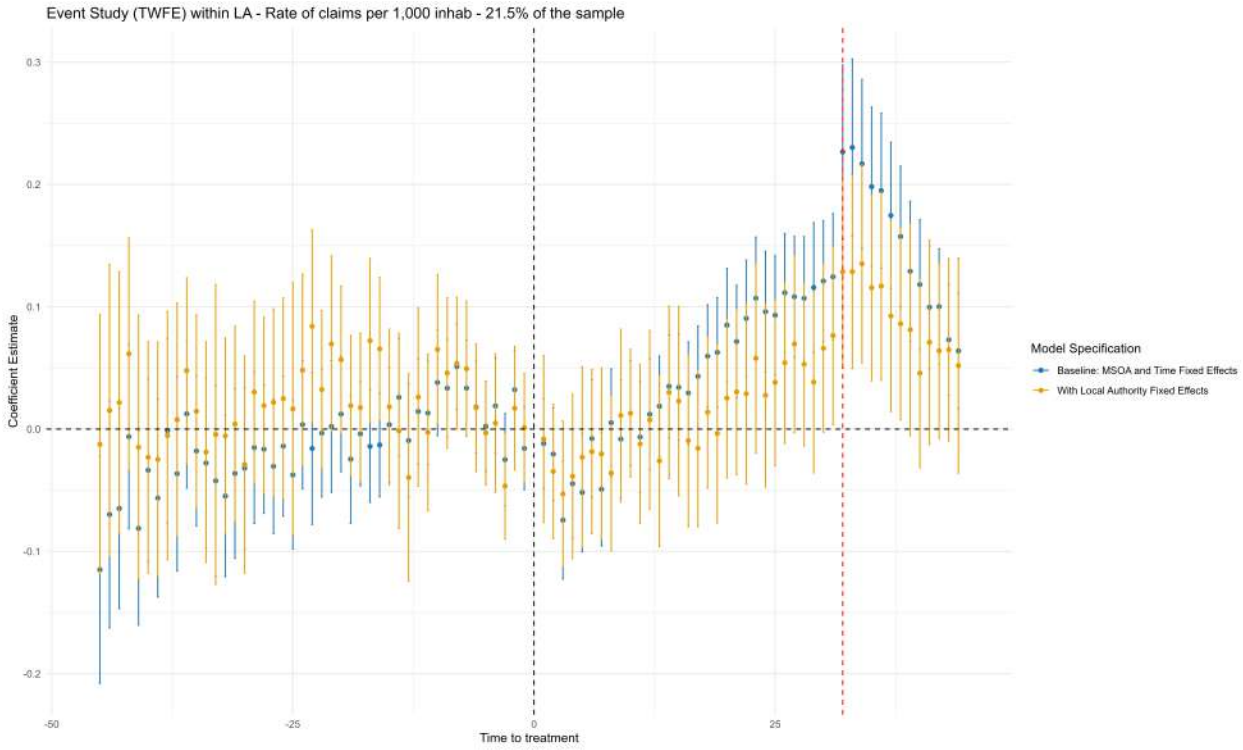


FIGURE 26: Event-study - Eviction Claims - With/without Local Authority Fixed Effects

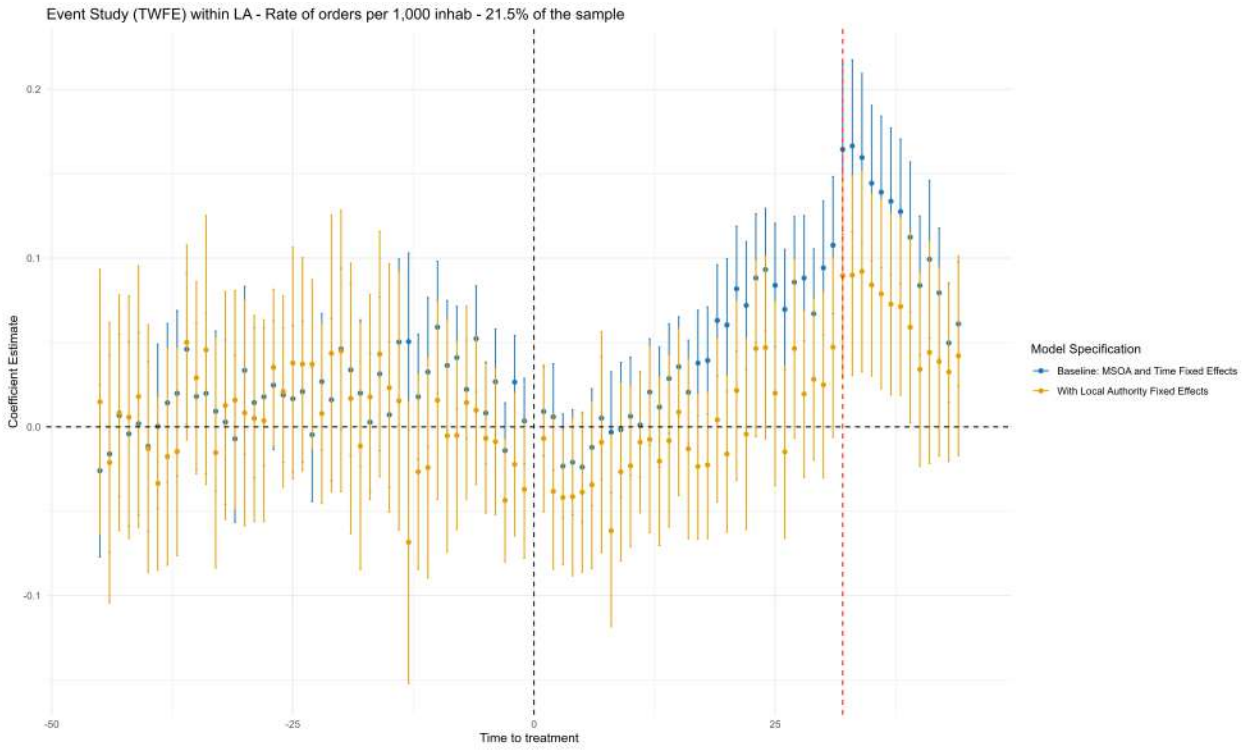


FIGURE 27: Event-study - Eviction Orders - With/without Local Authority Fixed Effects

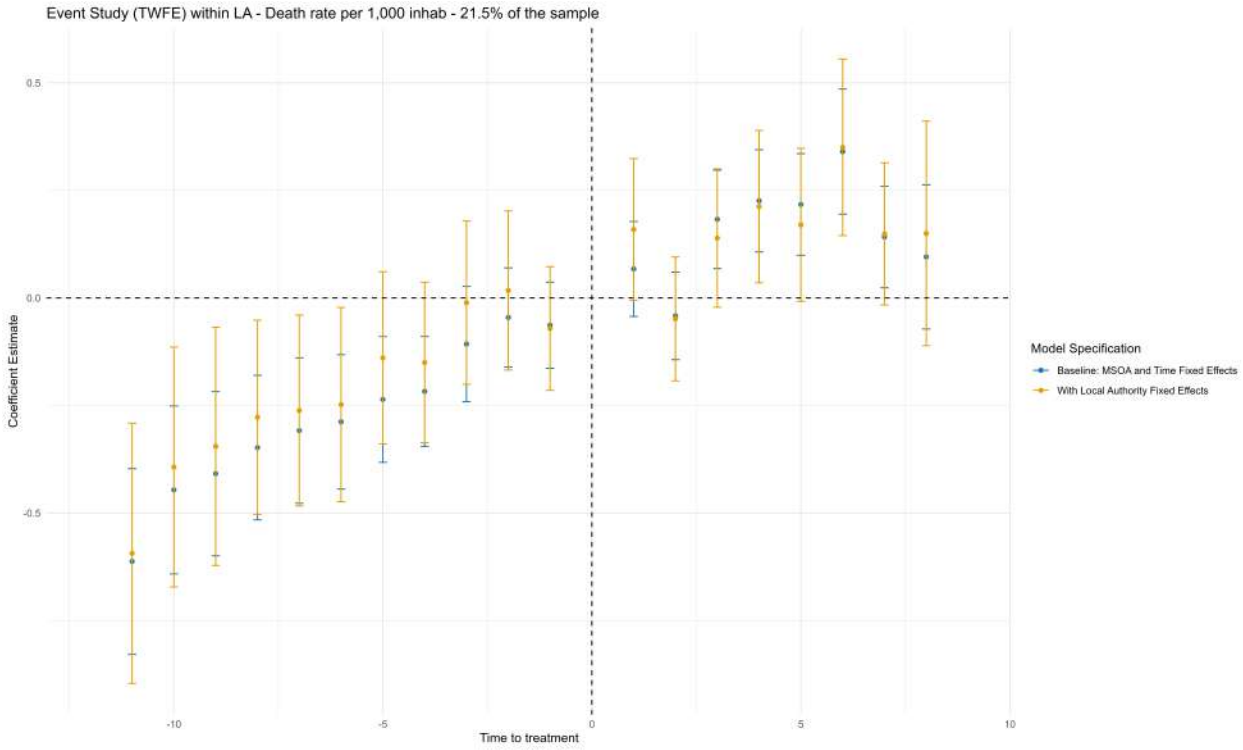


FIGURE 28: Event-study - Mortality - With/without Local Authority Fixed Effects

D.2.6 Pooled Average Treatment on the Treated - Binary Definition of Change in Distance

TABLE 7: Eviction claims

ATT	Std. Error	[95% Conf. Int.]
8.6869*	2.0812	(4.6077, 12.7661)

Note: Estimation using *did* package from Callaway and Sant'Anna (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 8: Eviction orders

ATT	Std. Error	[95% Conf. Int.]
5.6146*	1.3462	(2.9761, 8.253)

Note: Estimation using *did* package from Callaway and Sant'Anna (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 9: Age-adjusted mortality

ATT	Std. Error	[95% Conf. Int.]
7.1145	5.914	(-4.4766, 18.7056)

Note: Estimation using *did* package from Callaway and Sant'Anna (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 10: Mean House Prices

ATT	Std. Error	[95% Conf. Int.]
-19161.08 *	1569.634	(-22237.51, -16084.65)

Note: Estimation using *did* package from Callaway and Sant'Anna (2021). $*$ denotes that the confidence band does not cover 0.

D.3 Staggered DiD

D.3.1 Event-studies

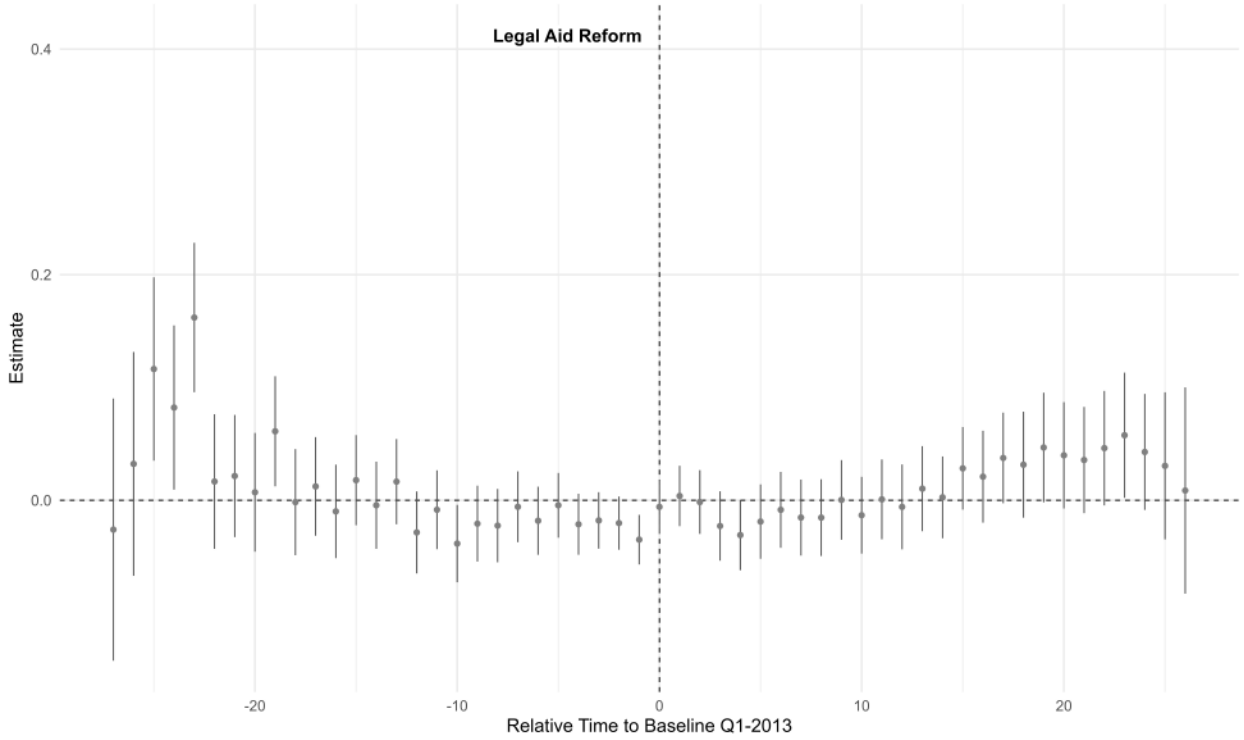


FIGURE 29: Eviction claim rate per 1,000 inhabitants - Staggered

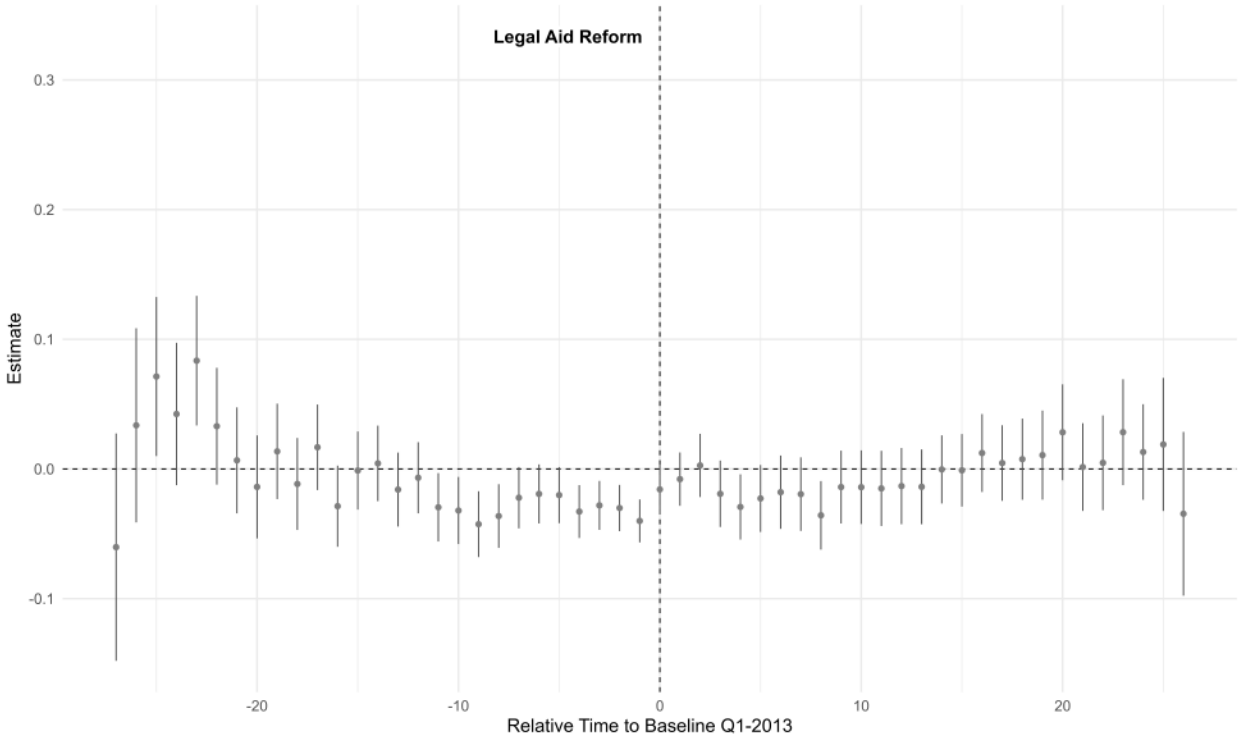


FIGURE 30: Eviction orders per 1,000 inhabitants - Staggered

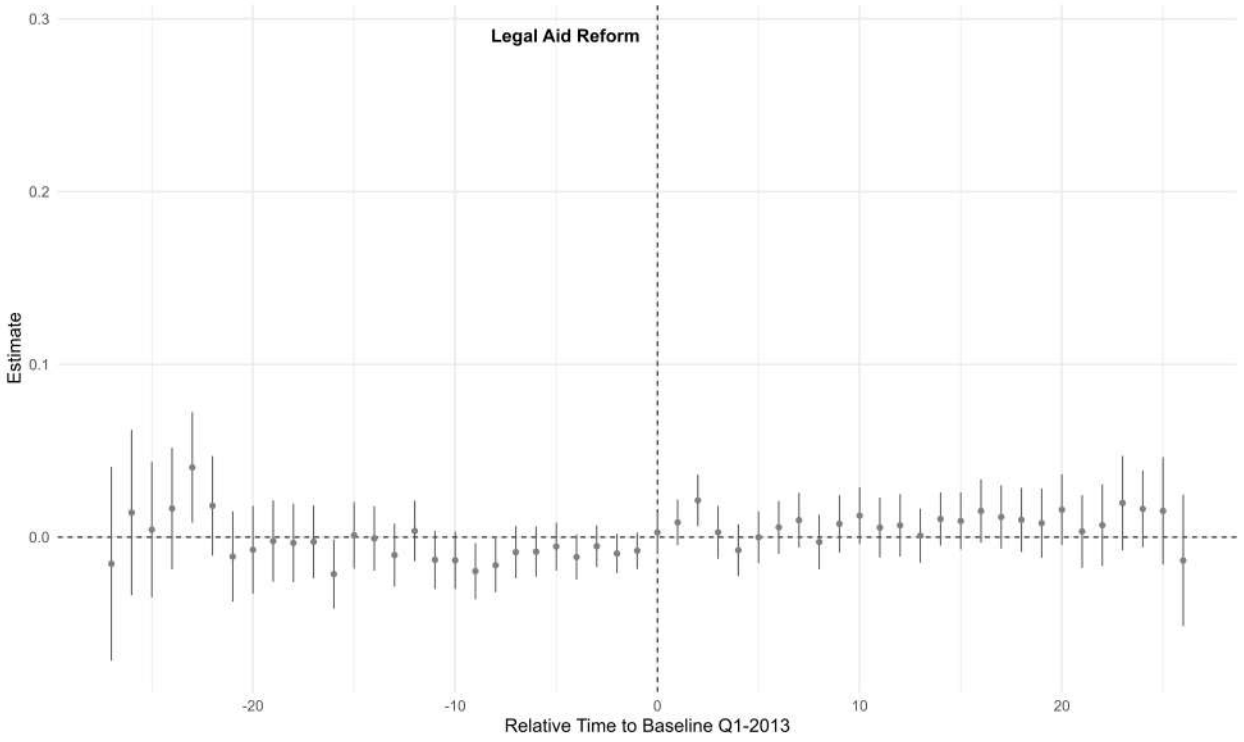


FIGURE 31: Eviction Suspended Possession orders per 1,000 inhabitants - Staggered

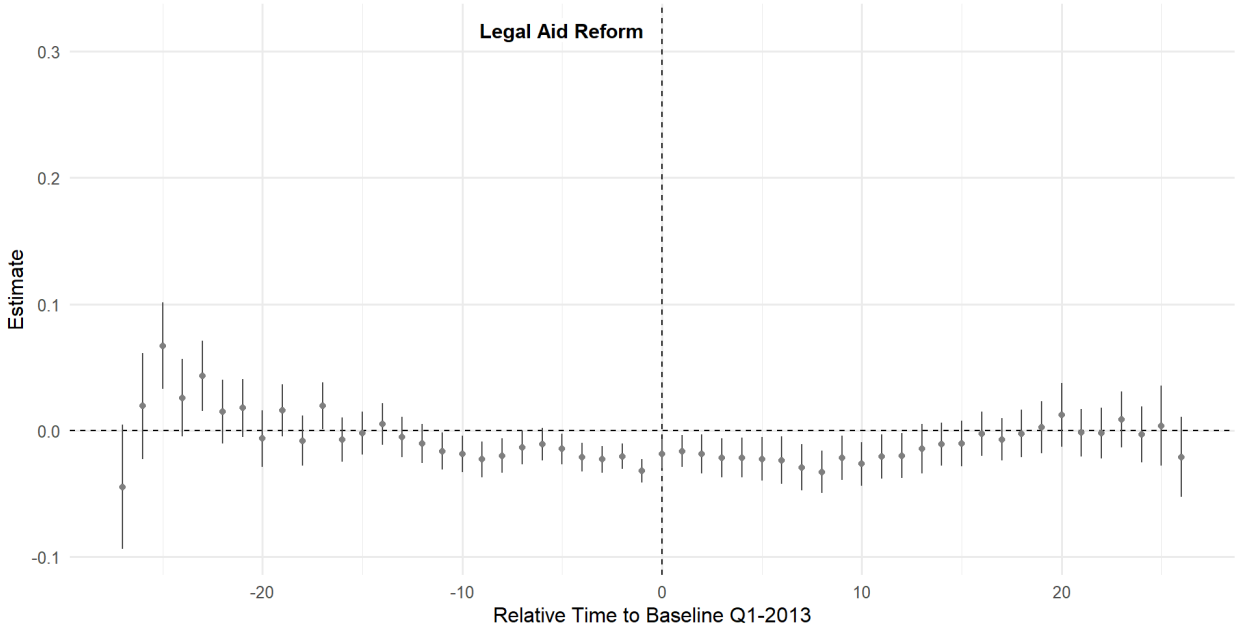


FIGURE 32: Eviction Outright orders per 1,000 inhabitants - Staggered

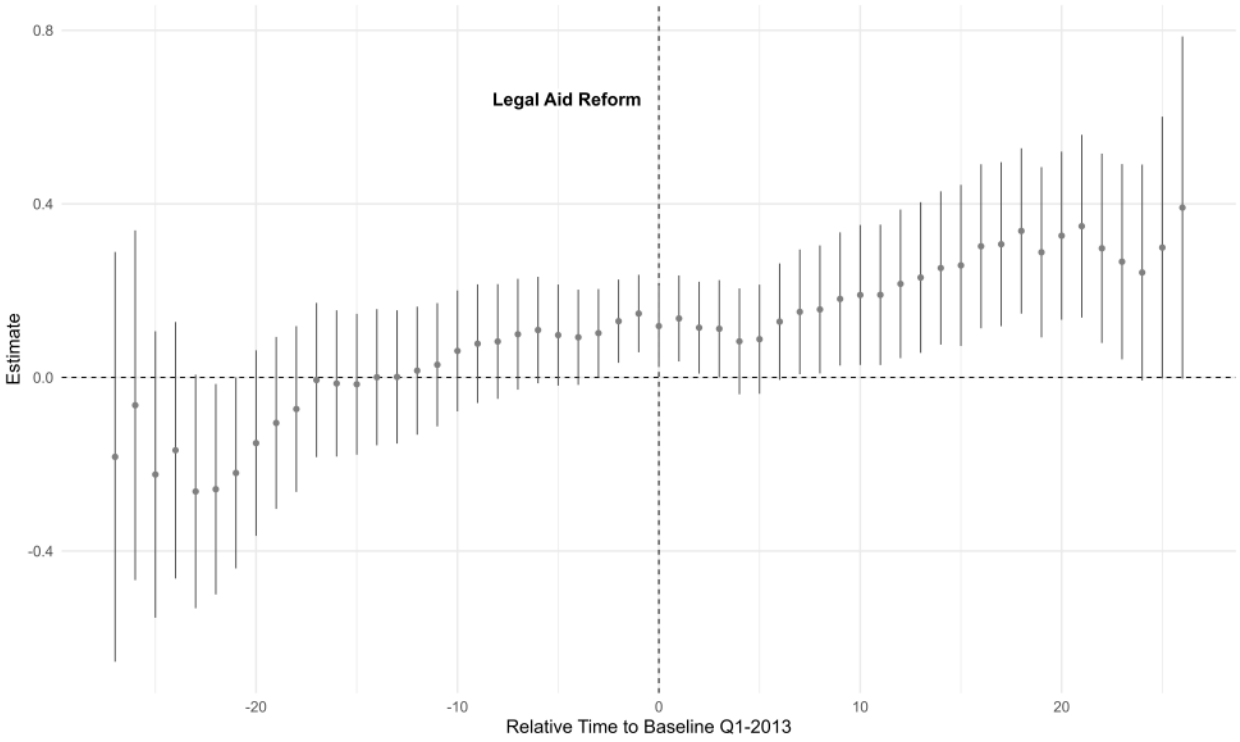


FIGURE 33: Mortality rate per 1,000 inhabitants - Staggered

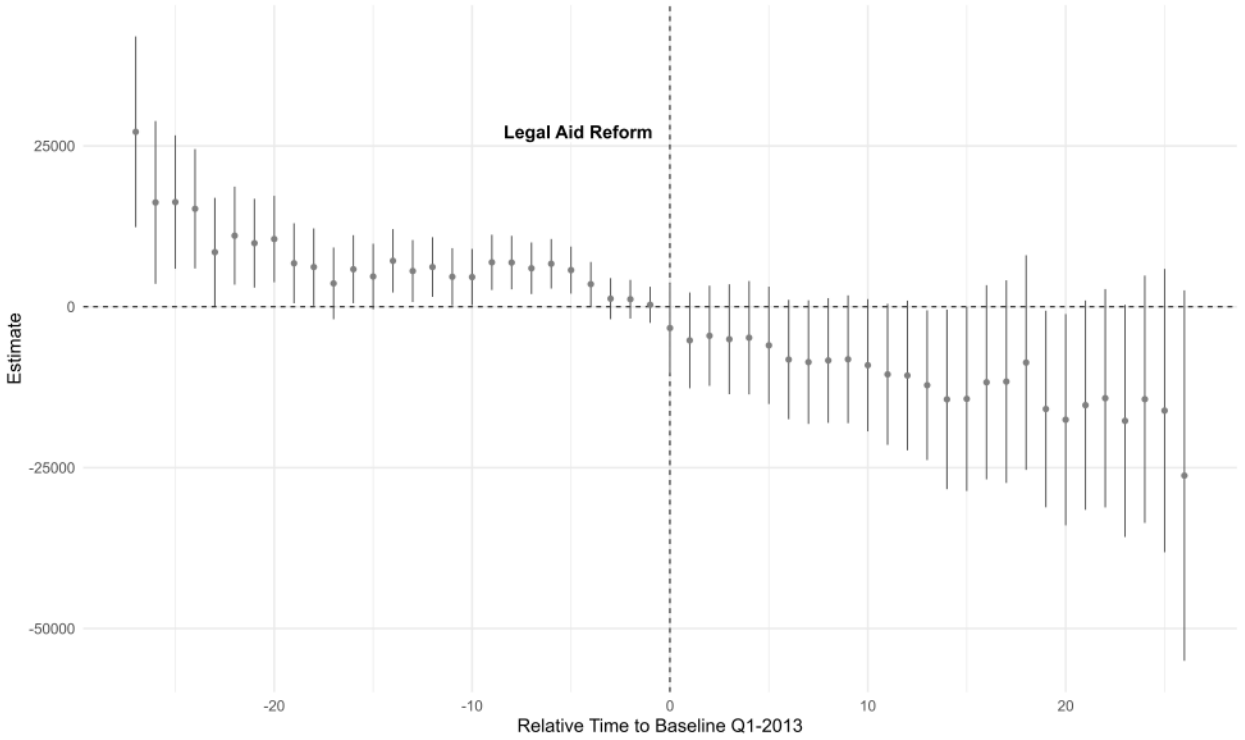


FIGURE 34: Mean House prices - Staggered

D.3.2 Cohort-Time Average Treatment on the Treated

TABLE 11: GT-ATE - Eviction Claims

Event time	Estimate	Std. Error	[95% Simult. Conf. Band]
0	-0.0058212896	0.01221910	(-0.029770735, 0.0181281558)
1	0.0038380878	0.01366749	(-0.022950201, 0.0306263767)
2	-0.0015459717	0.01441278	(-0.029795028, 0.0267030848)
3	-0.0227825502	0.01569268	(-0.053540210, 0.0079751097)
4	-0.0308101560	0.01589500	(-0.061964356, 0.0003440443)
5	-0.0188855258	0.01681682	(-0.051846492, 0.0140754407)
6	-0.0084444386	0.01715801	(-0.042074148, 0.0251852703)
7	-0.0152883725	0.01718069	(-0.048962517, 0.0183857722)
8	-0.0153646919	0.01735690	(-0.049384218, 0.0186548344)
9	0.0003128173	0.01800067	(-0.034968492, 0.0355941263)
10	-0.0132355039	0.01735611	(-0.047253488, 0.0207824797)
11	0.0008103778	0.01804111	(-0.034550192, 0.0361709474)
12	-0.0058369531	0.01920648	(-0.043481651, 0.0318077447)
13	0.0103065405	0.01924266	(-0.027409074, 0.0480221547)
14	0.0026189228	0.01849220	(-0.033625785, 0.0388636311)
15	0.0283242507	0.01868263	(-0.008293707, 0.0649422079)
16	0.0209290644	0.02078222	(-0.019804084, 0.0616622126)
17	0.0375377427	0.02049524	(-0.002632932, 0.0777084176)
18	0.0315266543	0.02395786	(-0.015430747, 0.0784840560)
19	0.0467860726	0.02478792	(-0.001798254, 0.0953703989)
20	0.0398440599	0.02404416	(-0.007282503, 0.0869706232)
21	0.0357184572	0.02402215	(-0.011364965, 0.0828018796)
22	0.0462365442	0.02588332	(-0.004494765, 0.0969678538)
23	0.0576706254	0.02831252	(0.002178083, 0.1131631678) *
24	0.0428455306	0.02623411	(-0.008573322, 0.0942643828)
25	0.0305187221	0.03325669	(-0.034664389, 0.0957018327)
26	0.0086694469	0.04667872	(-0.082820844, 0.1001597382)

Note: Estimation using *did_imputation* package from Borusyak et al. (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 12: GT-ATE - Eviction Claims

Event time	Estimate	Std. Error	[95% Simult. Conf. Band]
0	-0.0157631941	0.009880479	(-0.035128934, 0.003602545)
1	-0.0078343930	0.010470642	(-0.028356852, 0.012688066)
2	0.0027305405	0.012426013	(-0.021624445, 0.027085526)
3	-0.0191630643	0.013061038	(-0.044762698, 0.006436570)
4	-0.0292725461	0.012824230	(-0.054408038, -0.004137055) *
5	-0.0227435811	0.013188375	(-0.048592796, 0.003105634)
6	-0.0179130299	0.014425768	(-0.046187535, 0.010361475)
7	-0.0194070013	0.014474190	(-0.047776413, 0.008962410)
8	-0.0357402193	0.013475314	(-0.062151835, -0.009328604) *
9	-0.0139517180	0.014328709	(-0.042035987, 0.014132551)
10	-0.0140489918	0.014470104	(-0.042410396, 0.014312412)
11	-0.0150066809	0.014797553	(-0.044009885, 0.013996523)
12	-0.0132005289	0.014992453	(-0.042585737, 0.016184679)
13	-0.0137710952	0.014714515	(-0.042611545, 0.015069355)
14	-0.0003576999	0.013404287	(-0.026630103, 0.025914703)
15	-0.0010529619	0.014268005	(-0.029018251, 0.026912328)
16	0.0122874721	0.015342058	(-0.017782962, 0.042357906)
17	0.0045523939	0.014862965	(-0.024579017, 0.033683804)
18	0.0075349021	0.015962587	(-0.023751768, 0.038821572)
19	0.0106637719	0.017513230	(-0.023662158, 0.044989702)
20	0.0282794280	0.018858648	(-0.008683522, 0.065242378)
21	0.0014642653	0.017263171	(-0.032371550, 0.035300081)
22	0.0047351419	0.018645012	(-0.031809082, 0.041279366)
23	0.0283481591	0.020834104	(-0.012486685, 0.069183003)
24	0.0130771045	0.018809071	(-0.023788675, 0.049942884)
25	0.0189037183	0.026171221	(-0.032391874, 0.070199311)
26	-0.0345617796	0.032217270	(-0.097707629, 0.028584070)

Note: Estimation using $did_imputation$ package from Borusyak et al. (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 13: GT-ATE - Mortality

Event time	Estimate	Std. Error	[95% Simult. Conf. Band]
0	0.1183945134	0.04780122	(0.0247041210, 0.2120849058) *
1	0.1360067974	0.05056329	(0.0369027395, 0.2351108553) *
2	0.1147744899	0.05398527	(0.0089633537, 0.2205856260) *
3	0.1122332581	0.05718429	(0.0001520563, 0.2243144599) *
4	0.0832685917	0.06226851	(-0.0387776904, 0.2053148739)
5	0.0881794500	0.06416050	(-0.0375751398, 0.2139340398)
6	0.1286745825	0.06841485	(-0.0054185149, 0.2627676799)
7	0.1512392720	0.07329238	(0.0075862051, 0.2948923389) *
8	0.1567378337	0.07518975	(0.0093659241, 0.3041097432) *
9	0.1809608878	0.07829576	(0.0275012070, 0.3344205685) *
10	0.1901537472	0.08227245	(0.0288997467, 0.3514077476) *
11	0.1904456817	0.08251574	(0.0287148385, 0.3521765250) *
12	0.2157141509	0.08735649	(0.0444954382, 0.3869328635) *
13	0.2302717574	0.08855636	(0.0567013003, 0.4038422145) *
14	0.2522739902	0.09014187	(0.0755959319, 0.4289520485) *
15	0.2582332631	0.09472893	(0.0725645632, 0.4439019629) *
16	0.3023262513	0.09646528	(0.1132542943, 0.4913982084) *
17	0.3070651684	0.09641038	(0.1181008276, 0.4960295091) *
18	0.3376706569	0.09720292	(0.1471529433, 0.5281883705) *
19	0.2885098553	0.10009919	(0.0923154432, 0.4847042675) *
20	0.3267333218	0.09887008	(0.1329479731, 0.5205186705) *
21	0.3486107405	0.10747449	(0.1379607376, 0.5592607434) *
22	0.2976342439	0.11127959	(0.0795262554, 0.5157422325) *
23	0.2667923094	0.11481314	(0.0417585624, 0.4918260565) *
24	0.2418770769	0.12694093	(-0.0069271488, 0.4906813026)
25	0.2993726959	0.15391244	(-0.0022956812, 0.6010410729)
26	0.3914376322	0.20115390	(-0.0028240111, 0.7856992756)

Note: Estimation using *did_imputation* package from Borusyak et al. (2021). $*$ denotes that the confidence band does not cover 0.

TABLE 14: GT-ATE - Mean House Prices

Event time	Estimate	Std. Error	[95% Simult. Conf. Band]
0	-3302.3948	3561.543	(-10283.01881, 3678.229299)
1	-5222.8619	3795.767	(-12662.56490, 2216.841169)
2	-4505.3332	3972.053	(-12290.55703, 3279.890721)
3	-5043.5651	4357.149	(-13583.57786, 3496.447677)
4	-4802.8931	4494.883	(-13612.86315, 4007.077006)
5	-5993.3997	4650.602	(-15108.58002, 3121.780567)
6	-8198.3762	4733.803	(-17476.63000, 1079.877530)
7	-8600.6077	4898.121	(-18200.92485, 999.709529)
8	-8342.2133	4945.127	(-18034.66278, 1350.236087)
9	-8159.6467	5062.779	(-18082.69351, 1763.400152)
10	-9079.6416	5241.246	(-19352.48372, 1193.200607)
11	-10499.3834	5592.302	(-21460.29470, 461.527808)
12	-10679.8705	5932.170	(-22306.92332, 947.182400)
13	-12188.8439	5935.832	(-23823.07521, -554.612559) *
14	-14388.7774	7116.551	(-28337.21791, -440.336829) *
15	-14312.6812	7300.656	(-28621.96771, -3.394761) *
16	-11733.0017	7697.793	(-26820.67588, 3354.672457)
17	-11624.6981	8035.152	(-27373.59577, 4124.199470)
18	-8670.7767	8513.009	(-25356.27450, 8014.721090)
19	-15889.1039	7791.670	(-31160.77758, -617.430189) *
20	-17546.8893	8377.372	(-33966.53893, -1127.239658) *
21	-15290.8574	8297.094	(-31553.16080, 971.446071)
22	-14211.0698	8656.290	(-31177.39737, 2755.257678)
23	-17728.9016	9206.335	(-35773.31904, 315.515884)
24	-14365.5805	9802.393	(-33578.27069, 4847.109788)
25	-16136.3217	11231.903	(-38150.85118, 5878.207830)
26	-26232.6984	14686.709	(-55018.64711, 2553.250283)

Note: Estimation using $did_imputation$ package from Borusyak et al. (2021). $*$ denotes that the confidence band does not cover 0.

D.4 Bartik IV

D.4.1 First Stages

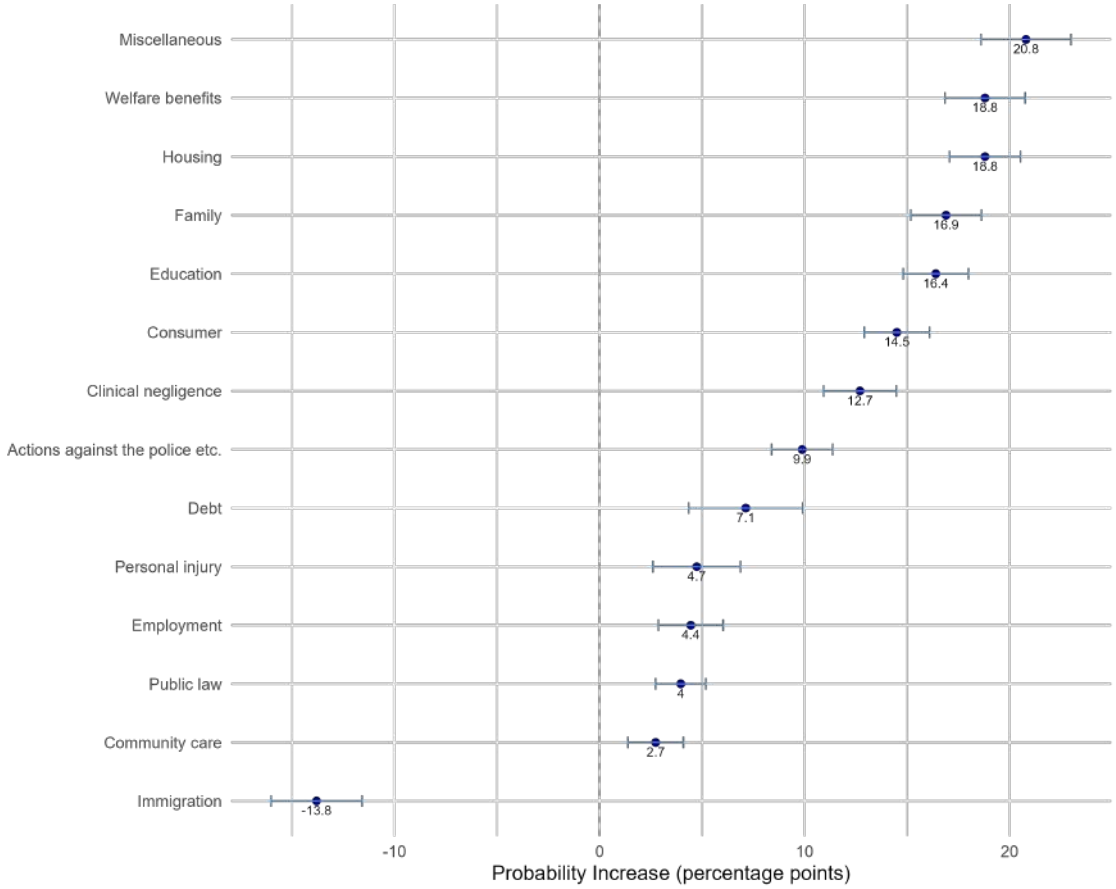


FIGURE 35: First Stage: Instrument Predicts Treatment Assignment in Difference-in-Differences Specification

Note: Points show the estimated effect of a one standard deviation increase in the Bartik instrument on the probability of being in the top quintile of the distance change distribution (treatment group in the DiD specification). Horizontal bars represent 95% confidence intervals. Values indicate percentage point increases in treatment probability. The instrument is constructed as the interaction of baseline provider exposure to each area of law with Ministry of Justice predicted funding cuts. Positive values indicate that areas with higher predicted cuts were more likely to experience large increases in distance to providers. Immigration shows a negative coefficient, suggesting different market dynamics in this area of law. Mental health is excluded due to insufficient variation. Estimates are from separate OLS regressions for each area of law with MSOA-level observations (N=7,264).

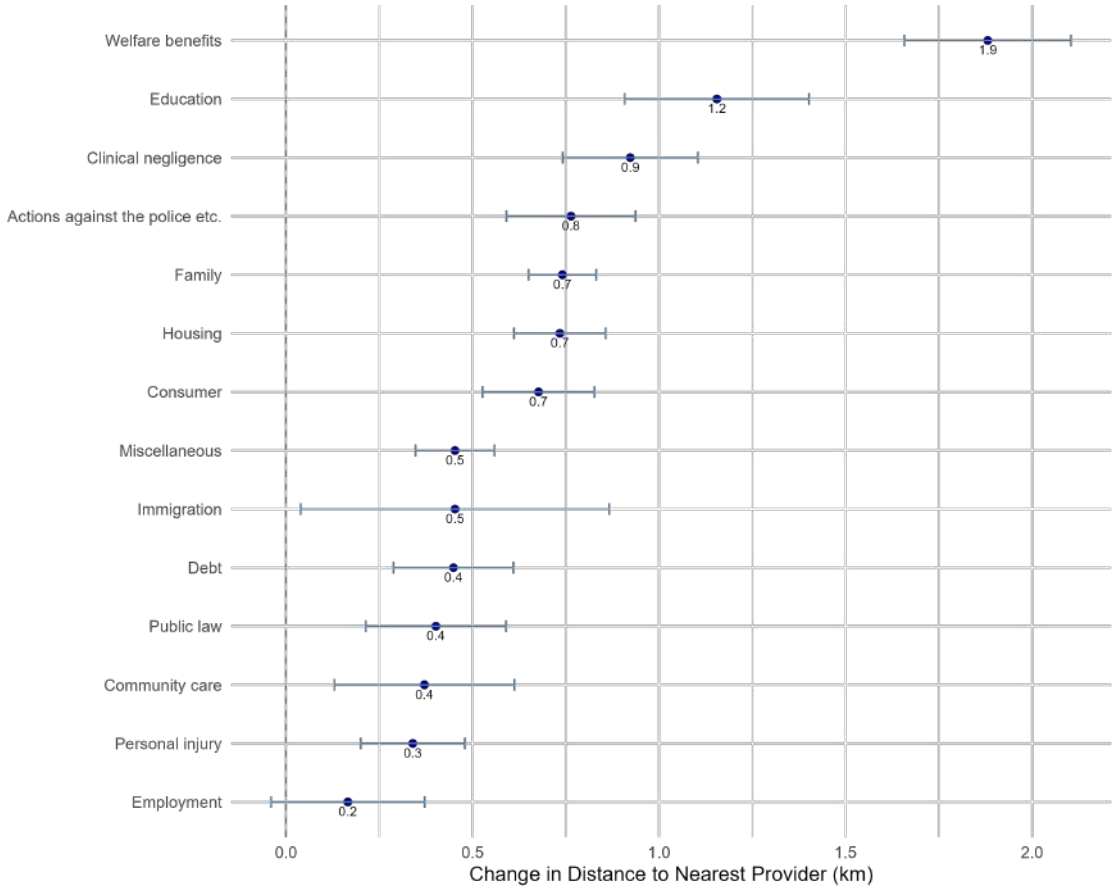


FIGURE 36: First Stage: Effect of Instrument on Distance to Nearest Provider

Note: Points show the estimated effect of a one standard deviation increase in the Bartik instrument on the change in distance (in kilometers) to the nearest legal aid provider. Horizontal bars represent 95% confidence intervals. The instrument is constructed as the interaction of baseline provider exposure to each area of law with Ministry of Justice predicted funding cuts. Positive values indicate that areas with higher predicted cuts experienced larger increases in distance to providers. Welfare benefits shows the largest effect (1.9 km), followed by Education (1.2 km) and Clinical negligence (0.9 km). Employment shows a small, statistically insignificant effect (0.17 km, p=0.115), suggesting minimal impact on provider geography in this area. Mental health is excluded due to insufficient variation. Estimates are from separate OLS regressions for each area of law with MSOA-level observations (N=7,264).

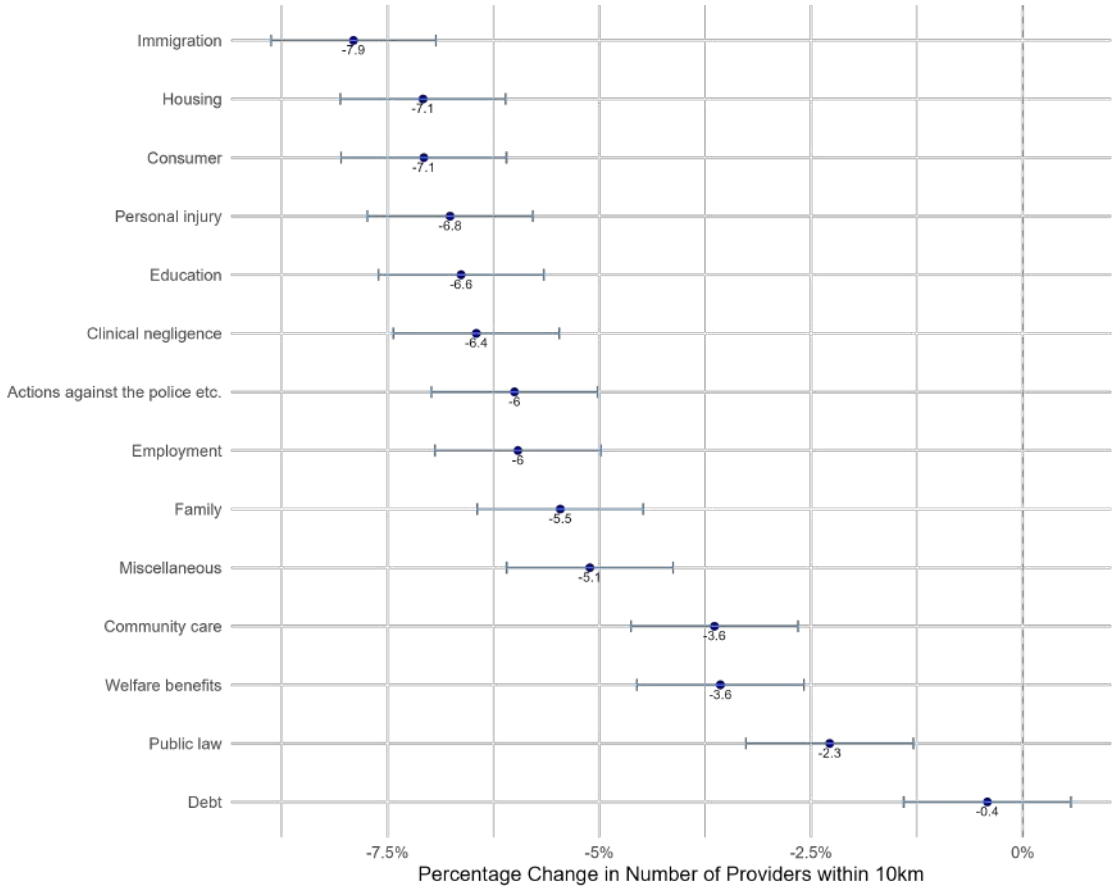


FIGURE 37: First Stage: Percentage Change in Number of Providers within 10km

Note: Points show the estimated effect of a one standard deviation increase in the Bartik instrument on the percentage change in the number of legal aid providers within a 10km radius between Q2 2012 and Q1 2014. Horizontal bars represent 95% confidence intervals. The instrument is constructed as the interaction of baseline provider exposure to each area of law with Ministry of Justice predicted funding cuts. Immigration experienced the largest percentage reduction (-7.9%), followed by Housing (-7.1%) and Consumer (-7.1%). Debt shows a negligible and statistically insignificant reduction (-0.4%, p=0.406), suggesting providers in this area had already exited before the measurement period or were less affected by the cuts. Public law (-2.3%) and Welfare benefits (-3.6%) show moderate reductions despite facing severe funding cuts, possibly reflecting providers' attempts to maintain services in these critical areas. Mental health is excluded due to insufficient variation. Estimates are from separate OLS regressions for each area of law with MSOA-level observations (N=7,264).

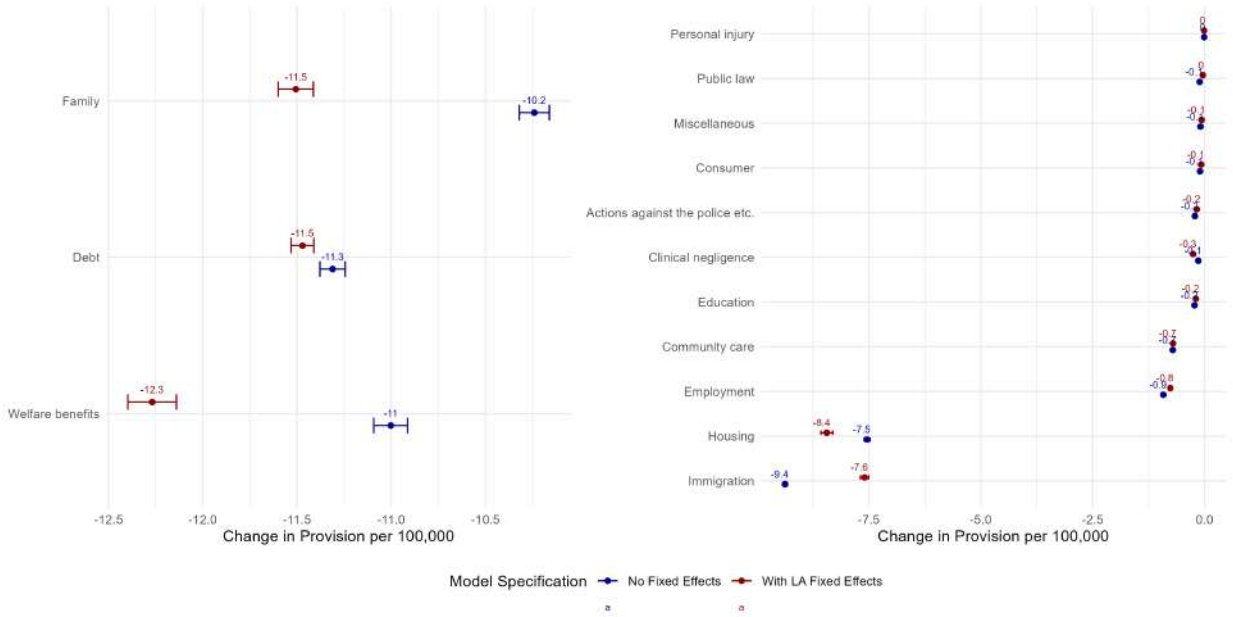


FIGURE 38: First Stage: Effect of Instrument on Change in Distance-Weighted Provision

Note: Points show the estimated effect of a one standard deviation increase in the Bartik instrument on the change in distance-weighted provision per 100,000 population between 2009-10 and 2013-14. Horizontal bars represent 95% confidence intervals. Red coefficients indicate additionally controlling for Local Authorities fixed effects. Negative values indicate reductions in provision. The instrument is constructed as the interaction of baseline provider exposure to each area of law with Ministry of Justice predicted funding cuts. Distance-weighted provision measures the inverse-distance-weighted sum of legal aid case volumes from all active providers, capturing both provider exit (extensive margin) and volume reductions among surviving providers (intensive margin). Left panel shows areas with larger reductions in provision (>10 per 100,000); right panel shows areas with smaller effects. Welfare benefits experienced the largest reduction (-12.3 per 100,000), followed by Debt (-11.5). Mental health is excluded due to insufficient variation. Estimates are from separate OLS regressions for each area of law with MSOA-level observations (N=7,264).

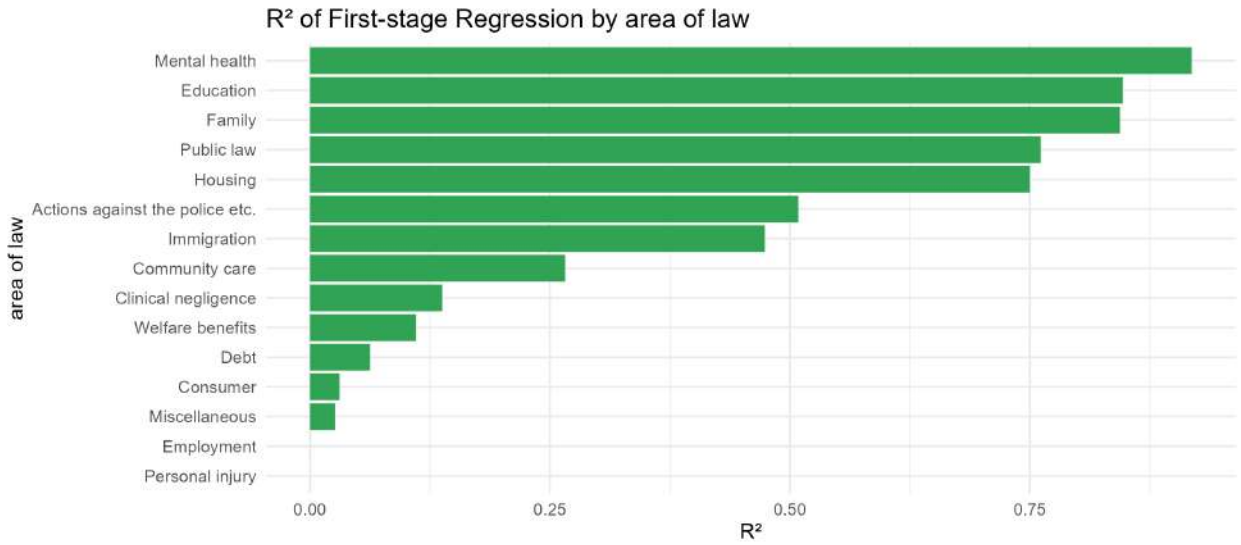


FIGURE 39: Instrument Relevance: R^2 from First-Stage Regressions by Area of Law

Note: This graph displays the adjusted R^2 values from first-stage regressions of post-reform MSOA-level access weights on the instrument, separately for each area of law. The access weights are constructed using the same spatial weighting method as the instrument but are based on observed provider activity in 2013–14, following the reform. Higher R^2 values indicate stronger explanatory power of the instrument.

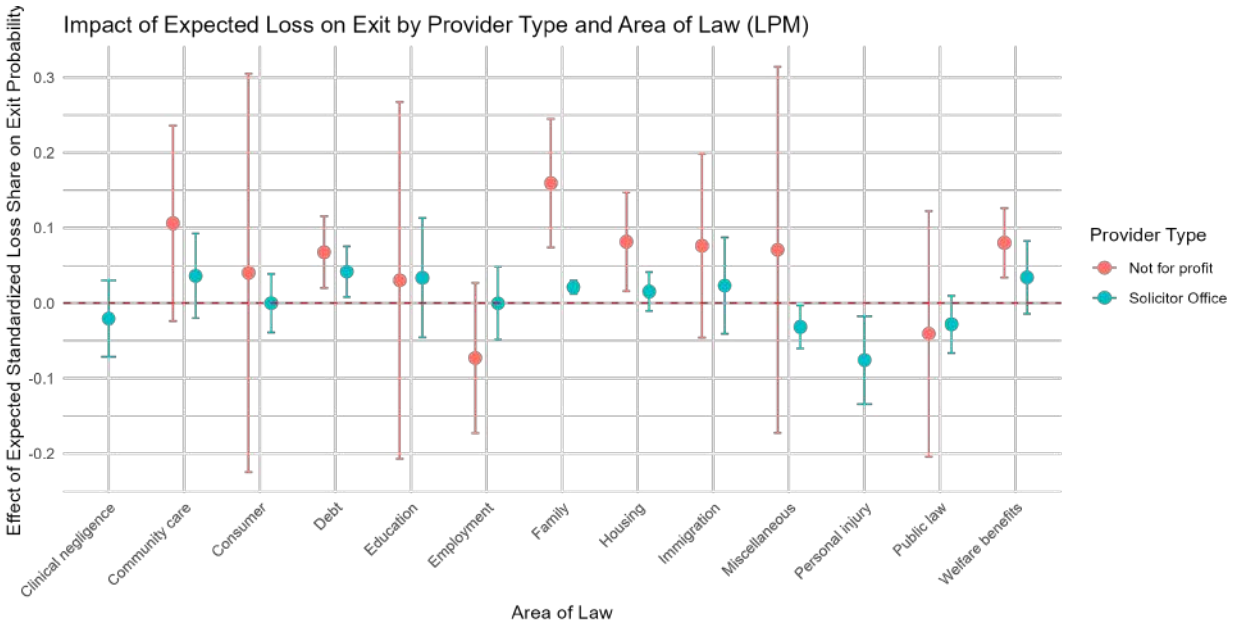


FIGURE 40: Instrument Predictiveness for Provider Exit by Area of Law and Provider Type

Note: This graph displays coefficient estimates from linear probability models regressing legal aid provider exit (defined as no longer active after the 2013–14 financial year) on the instrument, separately by area of law and provider type (not-for-profit organisations vs. private solicitor firms). The instrument is computed at the area-of-law level and captures pre-reform exposure to legal aid retrenchment. Statistically significant estimates indicate that greater predicted exposure is associated with a higher likelihood of exit.

D.4.2 Homelessness

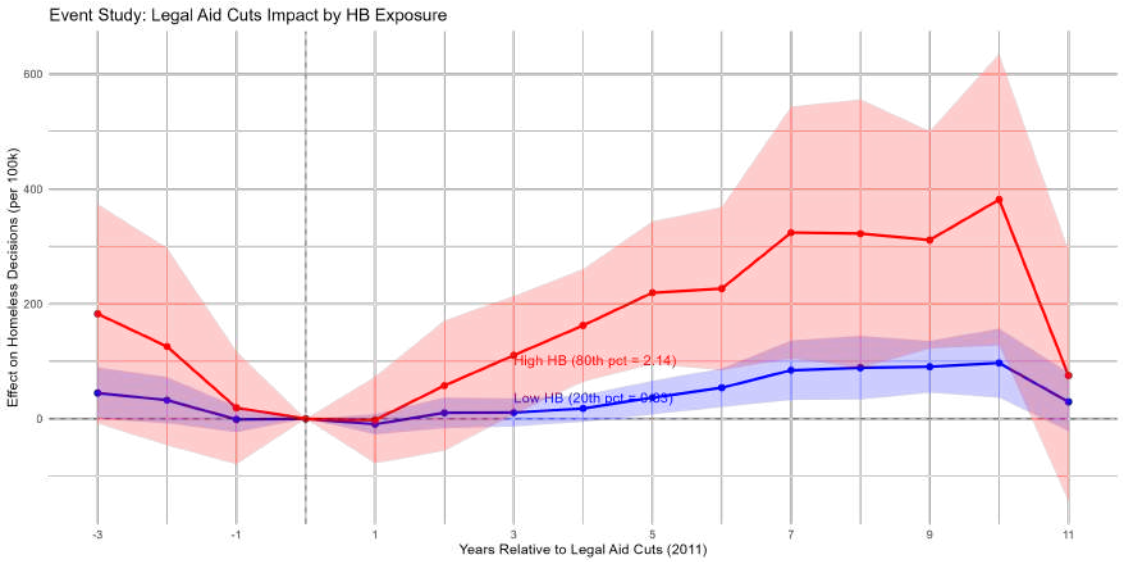


FIGURE 41: Event Study: Heterogeneous Effects of Legal Aid Cuts by Housing Benefit Exposure

Note: Lines show the effect of a one standard deviation increase in legal aid cuts on homelessness decisions per 100,000 population for areas at different levels of housing benefit reduction exposure. Blue line represents areas at the 20th percentile of housing benefit cuts (0.67 standard deviations); red line represents areas at the 80th percentile (2.05 standard deviations). Shaded areas indicate 95% confidence intervals based on clustered standard errors at the local authority level. Year 0 (2011) is the reference period when legal aid fees were cut by 10%. The divergence in trajectories post-treatment indicates that legal aid cuts had substantially larger effects in areas also experiencing deep housing benefit reductions. Sample includes 314 local authorities observed quarterly from 2008-2022.

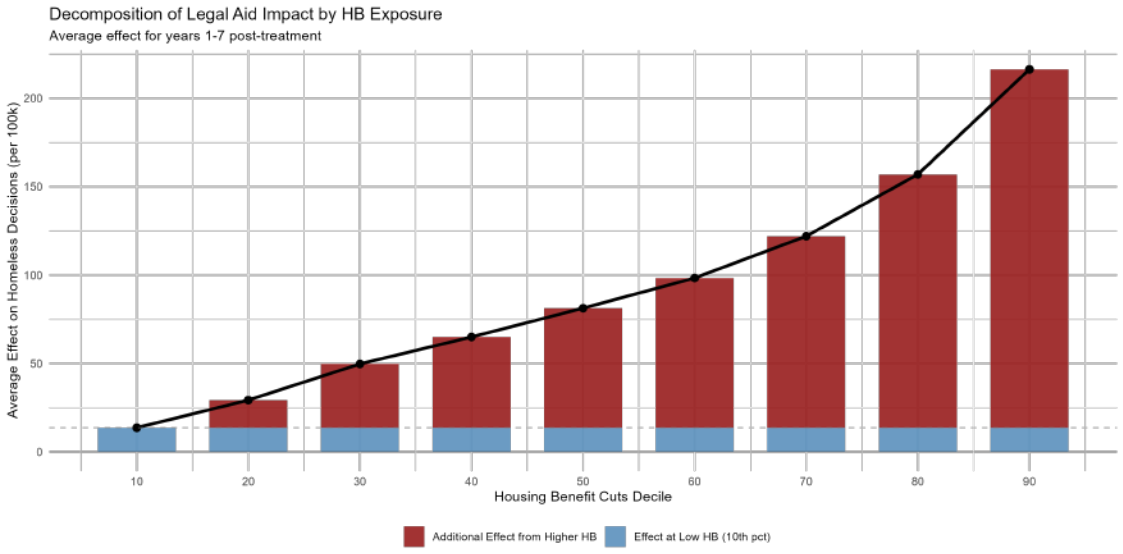


FIGURE 42: Decomposition of Legal Aid Impact on Homelessness by Housing Benefit Cut Intensity

Note: Bars show the average effect of a one standard deviation increase in legal aid cuts on homelessness decisions per 100,000 population for years 1-7 post-treatment, decomposed by housing benefit cut exposure deciles. Blue component represents the baseline effect experienced by areas with minimal housing benefit cuts (10th percentile); red component shows the additional impact attributable to higher housing benefit exposure. Black line and points indicate the total effect at each decile. The horizontal dashed line marks the baseline effect for reference. Housing benefit cuts are measured as the standardized combined loss in annual household income from all benefit reforms. The 16-fold difference between the bottom and top deciles demonstrates strong complementarity between legal aid access and welfare support in preventing homelessness.

D.5 Heterogeneity

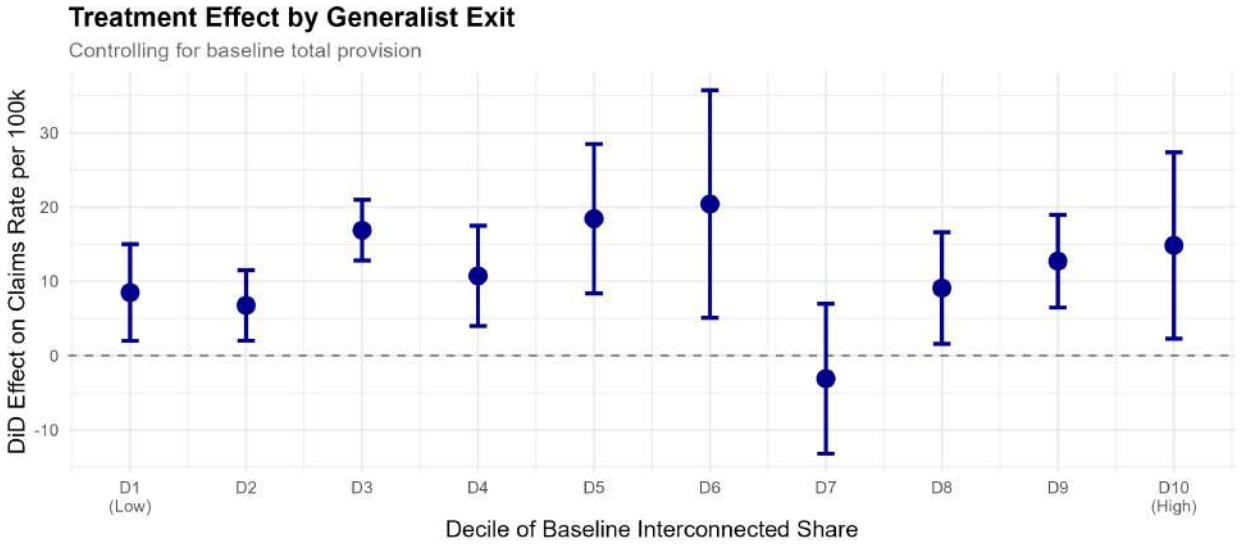


FIGURE 43: Heterogeneous Treatment Effects by Generalist Provider Exit Rates

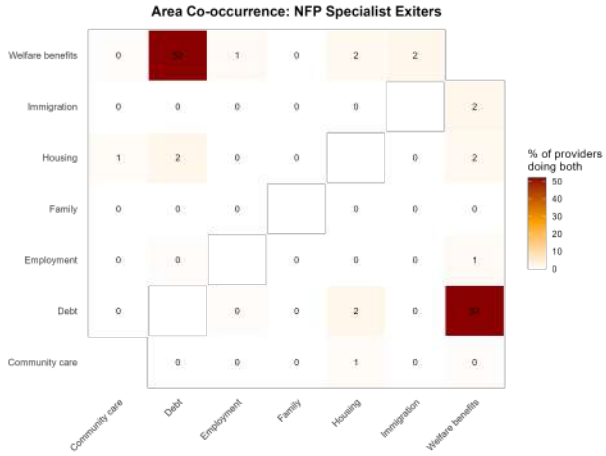
Note: Points show difference-in-differences estimates of the effect of legal aid cuts on eviction claims per 100,000 population, stratified by deciles of generalist provider exit rates. Vertical bars represent 95% confidence intervals. Generalist providers are defined as those serving three or more areas of law with at least 5% specialization at baseline. Exit rates measure the proportion of baseline generalist providers that ceased operations by 2014. The specification controls for baseline total provision and includes MSOA and quarter fixed effects with standard errors clustered at the local authority level. The pattern reveals that areas experiencing higher generalist exit rates (deciles 1-6 and 8-10) saw significantly larger increases in eviction claims, ranging from 7 to 20 additional claims per 100,000. The anomalous negative effect at decile 7 may reflect threshold effects or compositional differences in local market structure. Results demonstrate that the loss of generalist providers—who integrated services across housing, debt, and welfare benefits—had particularly severe consequences for housing stability outcomes.

D.6 Exiters

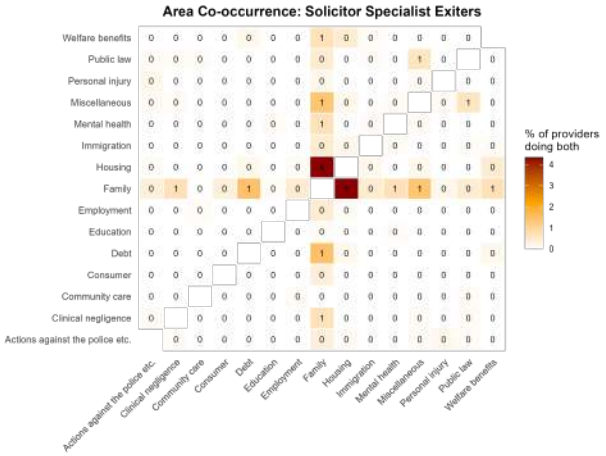
TABLE 15: Exit Patterns: Most Common Provider Portfolios

Panel A: NFP Specialists (N=224)	
Debt + Welfare benefits	117
Debt only	32
Welfare benefits only	29
Housing only	20
Debt + Housing	5
Panel B: NFP Generalists (N=88)	
Debt + Housing + Welfare benefits	37
Debt + Employment + Welfare benefits	10
Debt + Employment + Family + Housing + Welfare benefits	7
Debt + Employment + Housing + Welfare benefits	6
Community care + Debt + Housing + Welfare benefits	5
Panel C: Solicitor Specialists (N=1,176)	
Family only	802
Family + Housing	51
Miscellaneous only	44
Clinical negligence only	33
Immigration only	33
Panel D: Solicitor Generalists (N=104)	
Debt + Family + Housing	12
Debt + Employment + Family + Housing	5
Family + Housing + Miscellaneous	5
Debt + Family + Housing + Miscellaneous	4
Debt + Family + Miscellaneous	4

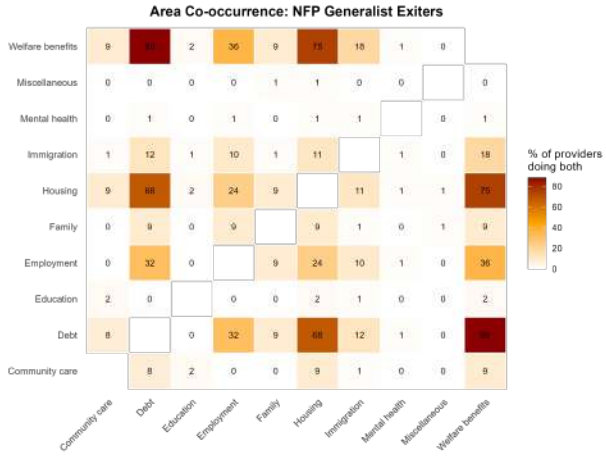
Note: This table shows the five most common portfolio combinations for each provider type among those who exited the legal aid market by 2013-14, in terms of the legal domains in which they undertake legal aid work. Only legal domain where providers had >5% specialization are included.



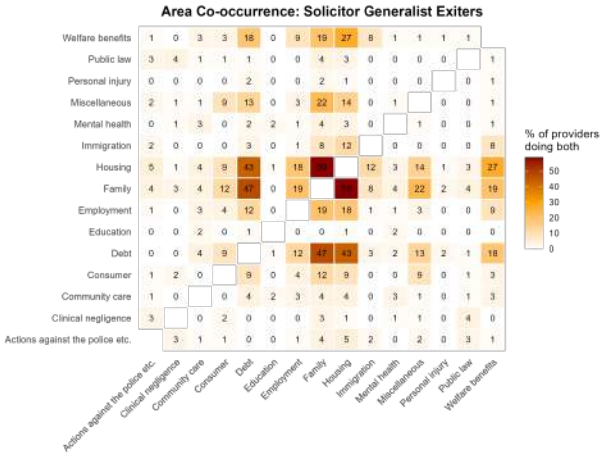
(a) NFP Specialists (N=224)



(b) Solicitor Specialists (N=1,176)



(c) NFP Generalists (N=88)



(d) Solicitor Generalists (N=104)

FIGURE 44: Area Co-occurrence Patterns Among Exiting Providers by Type and Specialization

Note: Heatmaps show the percentage of exiting providers who offered each combination of legal aid areas in 2009-10. Darker colors indicate more common combinations. Only providers with >5% specialization in an area are counted as offering that area. The diagonal is excluded as it would show 100% for each area with itself. NFP specialists (Panel a) show strong clustering around poverty law combinations (debt-welfare-housing), with over 50% offering debt and welfare benefits together. Solicitor specialists (Panel b) show minimal interconnection except for family-housing combinations. Generalist providers (Panels c and d) by definition serve multiple areas but show distinct patterns: NFP generalists concentrate on poverty law combinations while solicitor generalists mix family law with poverty law areas. These patterns support our instrument construction that assigns higher vulnerability weights to providers with interconnected poverty law portfolios.

E Appendix

E.1 Data

E.1.1 Legal Aid Providers

Data Sources and Quality

This project builds on the Ministry of Justice's Legal Aid Statistics, which releases the number of cases opened and completed each financial quarter by providers contracted to deliver legal aid. The data is released in two open-source datasets which do not share common identifiers. However, the data includes postcodes and names of providers at the office level, with branches sharing the same name and varying only by postcode. To merge the datasets, we undertook significant standardization work on providers' names, which used various abbreviations and suffixes (e.g., LLP, LTD, LIMITED), employing fuzzy matching with manual review.

We identified errors in the Ministry of Justice data. We found that postcode information had been misclassified for over 100 providers, which were registered as having their offices at the postcode of the ministry headquarters. For these providers, we manually looked them up in the Companies House Registry and extracted relevant addresses for their periods of operation. We were unable to assign 15 providers to accurate locations due to insufficient information about which offices they covered - 13 of these were offices of the charity "Immigration Advisory Service" and 2 were unidentified firms. We dropped these providers from the spatial analysis to avoid artificially inflating the measure of provision in London where the ministry headquarters are. We also retrieved latitudes and longitudes where possible using the API postcode.io after noticing inconsistencies in the dataset, defaulting to the original information when unavailable.

Variable Definitions and Construction

To construct our shift-share instrument, we combine information from both datasets to capture the vulnerability and exposure of providers to cuts in legal aid. We measure both their income stream (how much they rely on legal aid funding) and their effective caseloads (how many cases they opened compared to the entirety of work they undertake

in legal aid). Cases opened refer to cases for which work is undertaken by the provider in that quarter, while cases completed mean that the provider bills and receives payment in that quarter for earlier work. The casework data is further broken down into schemes: civil representation, legal help, and mediation (the latter only applies to family-related areas of law). Our instrument measures are defined relative to one of the fifteen areas of law considered in this work. Note that the Legal Aid Statistics contain additional disaggregation for family areas of law, which are consolidated into a single category for this project.

For billing data, we have information on different streams of legal aid funding. To measure funding from the provider's perspective, we construct a net profit measure by subtracting disbursements (out-of-pocket expenses incurred by a provider for case management) from total case costs. In some cases, the case costs may include a portion covered by the opponent and another by the Legal Aid Agency, the entity administering the scheme for the Ministry of Justice. Since we aim to measure the dependence and resilience of providers, we focus on total costs regardless of payer. When taking the MVPF approach, we focus on the net payment from the Legal Aid Agency after deducting the portion of the claimant's costs paid by the opponent when applicable.

E.1.2 Eviction data

TABLE 16: Summary statistics: Distribution of Possession Claims and Orders Rate Across MSOAs by Year

Year	Claims rate (per 1,000 individuals)						Orders rate (per 1,000 individuals)					
	Min	Q1	Mean	Q3	Max	Sd	Min	Q1	Mean	Q3	Max	Sd
2001	0.00	1.65	4.36	5.67	50.76	4.09	0.00	1.11	3.01	4.05	32.37	2.79
2002	0.00	1.65	4.38	5.73	35.20	4.04	0.00	1.04	2.94	3.95	31.24	2.77
2003	0.00	1.71	4.33	5.81	32.60	3.76	0.00	1.07	2.87	3.89	19.88	2.56
2004	0.00	1.87	4.52	6.05	26.70	3.75	0.00	1.13	2.95	4.01	21.36	2.57
2005	0.00	2.35	5.07	6.78	38.32	3.72	0.00	1.45	3.31	4.53	21.01	2.51
2006	0.00	2.49	5.25	7.07	33.05	3.71	0.00	1.62	3.59	4.89	25.99	2.66
2007	0.14	2.55	5.19	6.98	33.32	3.57	0.00	1.86	4.12	5.60	32.68	3.10
2008	0.00	2.66	5.25	6.98	35.97	3.53	0.00	2.22	4.66	6.25	32.00	3.37
2009	0.00	1.99	4.11	5.50	25.28	2.92	0.00	1.63	3.49	4.73	28.40	2.57
2010	0.00	1.81	3.72	4.95	24.55	2.64	0.00	1.36	2.88	3.92	16.82	2.03
2011	0.00	1.86	3.76	5.00	28.09	2.67	0.00	1.39	2.88	3.89	22.92	2.05
2012	0.00	1.69	3.64	4.75	23.84	2.77	0.00	1.25	2.72	3.65	18.02	2.03
2013	0.00	1.72	3.83	5.02	23.93	3.03	0.00	1.20	2.79	3.74	21.26	2.25
2014	0.00	1.50	3.47	4.61	22.26	2.80	0.00	1.09	2.65	3.58	18.51	2.18
2015	0.00	1.11	2.88	3.81	18.88	2.55	0.00	0.80	2.20	2.95	17.70	1.99
2016	0.00	1.01	2.56	3.44	15.54	2.21	0.00	0.73	1.97	2.67	14.10	1.76
2017	0.00	1.03	2.51	3.41	17.44	2.04	0.00	0.73	1.88	2.58	15.54	1.59
2018	0.00	0.98	2.31	3.10	23.75	1.84	0.00	0.71	1.75	2.38	23.40	1.45
2019	0.00	1.00	2.23	3.04	15.98	1.68	0.00	0.72	1.71	2.33	10.39	1.37
2020	0.00	0.29	0.73	0.99	12.35	0.62	0.00	0.14	0.42	0.60	8.15	0.40

Source: Ministry of Justice data obtained through Freedom of Information, Mid-year MSOA-level population estimates from ONS

Note: Population-weighted summary statistics calculated for Middle Layer Super Output Area (MSOA,n= 7201) for each year between 2000 and 2022. The number of claims and orders are aggregated over four possession types for each MSOA: Accelerated landlord, Mortgage, Private landlord, and Social landlord.

E.1.3 Mortality Outcomes - Measurement and Definitions

Excess Death Definitions

$$\text{Excess Deaths}_t = \sum_{\text{Age}} (\hat{\delta}_t^{\text{Age}} \sum_i \Delta\text{Distance}_i \times \text{Pop}_{i,t}^{\text{Age}})$$

$$\text{Excess Deaths}^{\text{Age}} = \sum_{t>2011} (\hat{\delta}_t^{\text{Age}} \sum_i \Delta\text{Distance}_i \times \text{Pop}_{i,t}^{\text{Age}})$$

$$\text{Excess Deaths}^{\text{Total}} = \sum_{\text{Age}} (\sum_{t>2011} (\hat{\delta}_t^{\text{Age}} \sum_i \Delta\text{Distance}_i \times \text{Pop}_{i,t}^{\text{Age}}))$$

E.2 Background Information

E.2.1 Timeline

TABLE 18: Timeline of Legal Aid Reform Events

Quarter (Financial Year)	Date	Event
Q1 – 2010	May 2010	Publication of the Coalition programme which mentions the intention to review legal aid.
Q1 – 2010	June 2010	Lord Chancellor and Secretary of State for Justice speech to the Centre for Crime and Justice Studies announcing a consultation on legal aid in autumn 2010.
Q3 – 2010	November 2010	Publication of the Ministry of Justice Green Paper and Proposals for the Reform of Legal Aid presented to Parliament.
Q1 – 2011	June 2011	Bill introduced in the House of Commons.
Q1 – 2012	May 2012	Bill received Royal Assent.
Q1 – 2013	April 2013	Legal Aid, Sentencing and Punishment of Offenders Act (LASPO) 2012 came fully into effect on April 1st, 2013.

Source: Grimwood (2015)

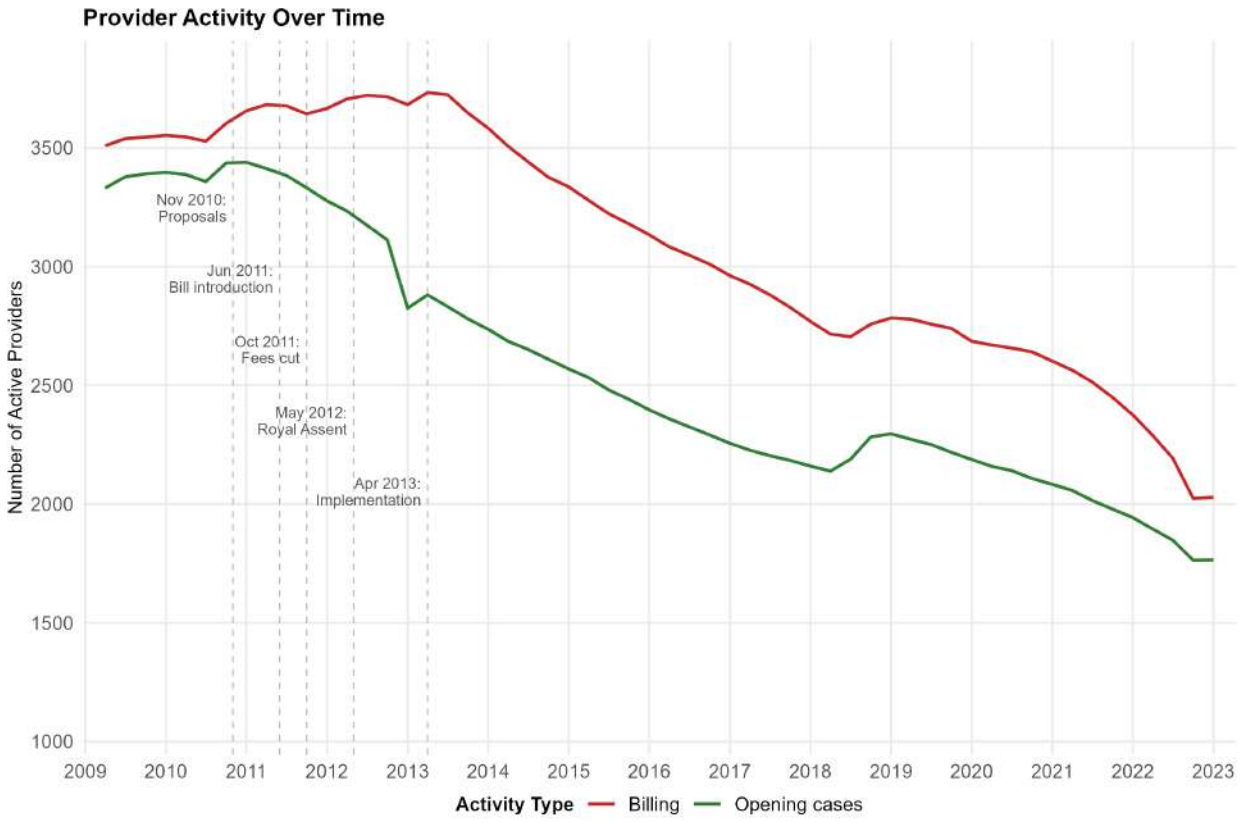


FIGURE 45: Provider Activity and Reform Anticipation: Case Opening versus Billing Patterns

Note: Lines show the quarterly number of active legal aid providers, distinguished by activity type. Green line represents providers opening new cases; red line represents providers billing for completed work. The vertical dashed lines mark key reform milestones: reform proposals (November 2010), introduction of the Bill in the House of Commons (June 2011), cuts to legal aid fees (October 2011), Royal Assent (May 2012), and full implementation of the Act (April 2013). The divergence between case opening and billing patterns reveals strong anticipation effects. Providers began reducing new case intake immediately after the reform proposal, with sharp acceleration following the 10% fee cuts in October 2011. However, billing activity remained elevated through implementation as providers completed existing cases, creating a bunching effect visible as the peak just before April 2013. The persistent gap between the two measures post-reform suggests many providers maintained contracts but operated at reduced capacity. By 2023, active providers had fallen from approximately 3,500 to 1,700, a 50% reduction from baseline levels.

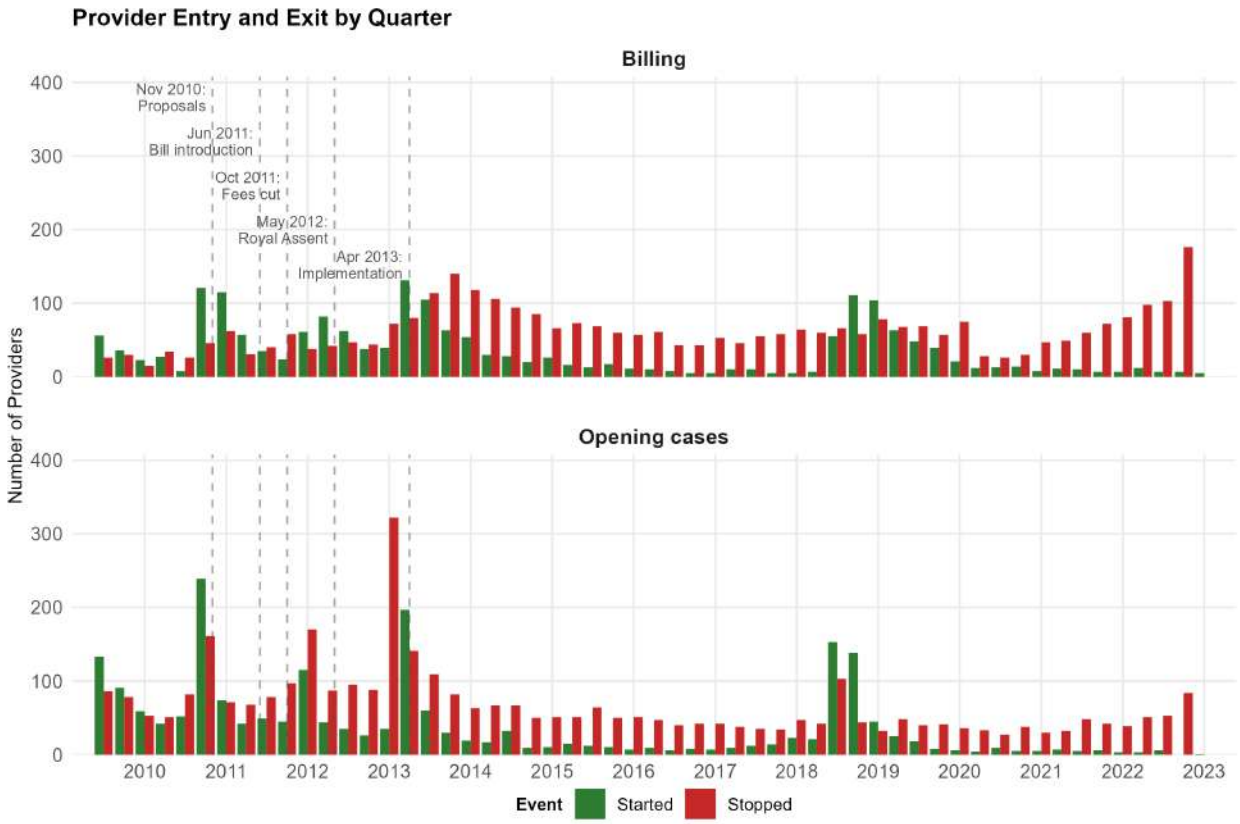


FIGURE 46: Provider Activity and Reform Anticipation: Flows of Entries and Exits from the Legal Aid Market

Note: Bars show quarterly flows of legal aid providers entering (green) and exiting (red) the market, separated by activity type. Upper panel displays providers billing for cases; lower panel shows providers opening new cases. The vertical dashed lines mark key reform milestones: reform proposals (November 2010), introduction of the Bill in the House of Commons (June 2011), cuts to legal aid fees (October 2011), Royal Assent (May 2012), and full implementation of the Act (April 2013). The flow dynamics reveal distinct anticipation patterns across activities. For case opening, exit rates began increasing immediately following the reform proposals in late 2010, with particularly sharp spikes after the October 2011 fee cuts and before the April 2013 implementation deadline. New entries virtually ceased from 2014 through 2018, until a modest recovery following the introduction of new legal aid contracts in 2019. Billing activity shows a different pattern: exits remained moderate until the implementation date, consistent with providers completing existing cases before exiting. The asymmetry between entry and exit flows—with exits exceeding entries from 2014 onwards—drove the 50% reduction in active providers documented in Figure 45. The post-2019 recovery in entries remains insufficient to offset the cumulative exits, indicating persistent structural changes to the market following LASPO.

E.2.2 Impact Assessment of the Reform

TABLE 19: Expected and Actual Volume Shocks by Area of Law and Scheme, in Percentage

Area of Law	Legal Help		Civil Representation	
	Expected	Actual	Expected	Actual
Actions against the police etc.	-48	31	-70	-21
Clinical negligence	-75	-100	-65	-94
Community care	-1	-36	0	75
Consumer	-100	-100	-99	-100
Debt	-74	-100	-13	-80
Discrimination	–	–	–	–
Domestic violence	–	–	0	-15
Education	-58	19	-29	-78
Employment	-78	-100	-95	-100
Family	-80	-94	–	–
Financial provision	–	–	-64	-92
Housing	-40	-48	-11	-36
Immigration	-92	-80	-9.46	-62
Mental health	0	-13	0	108
Miscellaneous	-85	-80	-60	130
Other family proceedings	–	–	-65	-95
Personal injury	-91	-100	-81	1000
Private law Children Act	–	–	-53	-79
Public law	-14	26	-1	-39
Welfare benefits	-98	-99	0	-40
Combined family proceedings	–	–	-2	-100

Source: Ministry of Justice of the United Kingdom (2019), please see Figure 3 page 31 for non family areas of law and Figure 89 page 145 for family areas.

TABLE 20: Expected and Actual Spending Shocks by Area of Law and Scheme, in Percentage

Area of Law	Legal Help		Civil Representation	
	Expected	Actual	Expected	Actual
Actions against the police etc.	-44	31	-59	-26
Clinical negligence	-75	-100	-64	-93
Community care	-1	-36	0	138
Consumer	-100	-100	-99	-100
Debt	-75	-100	-13	-85
Discrimination	–	–	–	–
Domestic violence	–	–	0	-22
Education	-32	19	-50	-85
Employment	-76	-100	-95	-100
Family	-80	-95	–	–
Financial provision	–	–	-72	-94
Housing	-38	-48	-12	-41
Immigration	-89	-80	-9.14	-64
Mental health	0	-13	0	66
Miscellaneous	-82	-80	-63	121
Other family proceedings	–	–	-83	-95
Personal injury	-91	-100	-80	1000
Private law Children Act	–	–	-70	-79
Public law	-16	26	-1	-38
Welfare benefits	-97	-99	0	-50
Combined family proceedings	–	–	0	-100

Source: Ministry of Justice of the United Kingdom (2019), please see Figure 3 page 31 for non family areas of law and Figure 89 page 145 for family areas.

E.2.3 Eviction process in the county courts of England and Wales

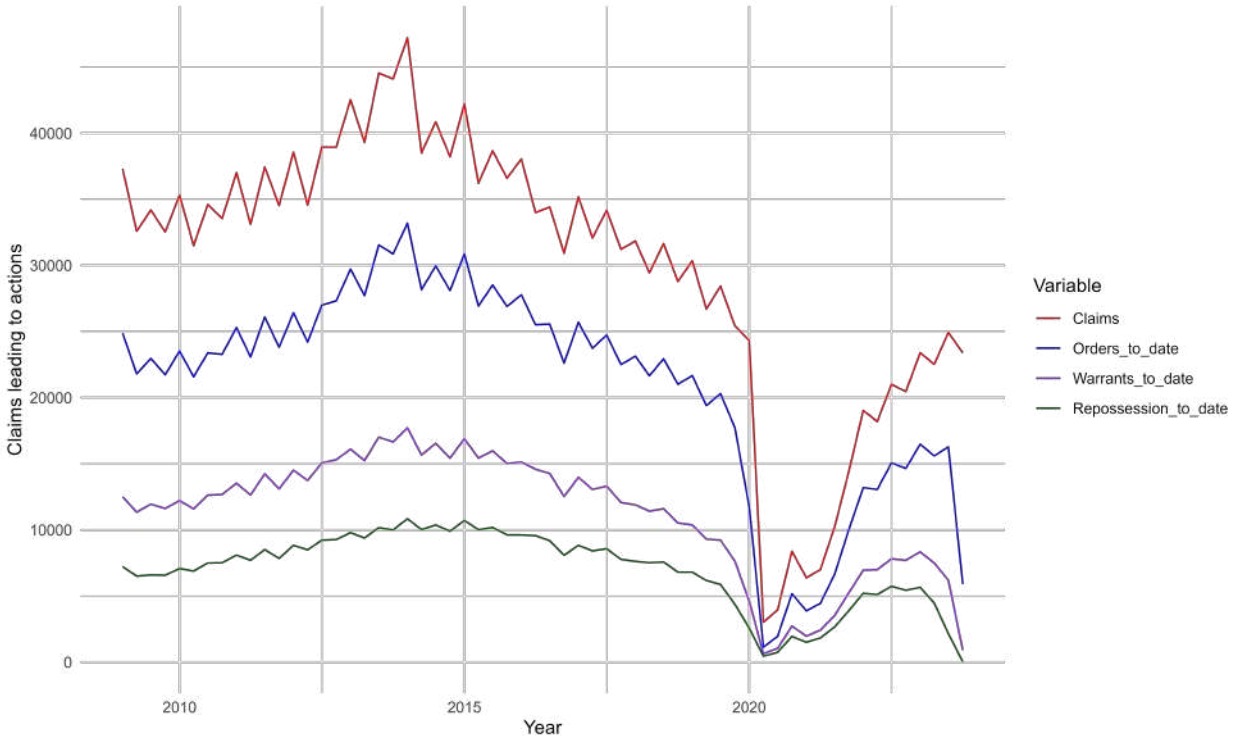


FIGURE 47: Number of possession claims that lead to orders, warrants and repossessions over time

Source: Ministry of Justice Mortgage and Landlord Possession Statistics.
Note: Quarterly National Statistics on possession claims that lead to orders, warrants and repossessions. This graph only covers actions initiated by landlords in the county courts of England and Wales and excludes mortgage cases. Numbers are measured "to date", meaning by the end of the considered quarter.

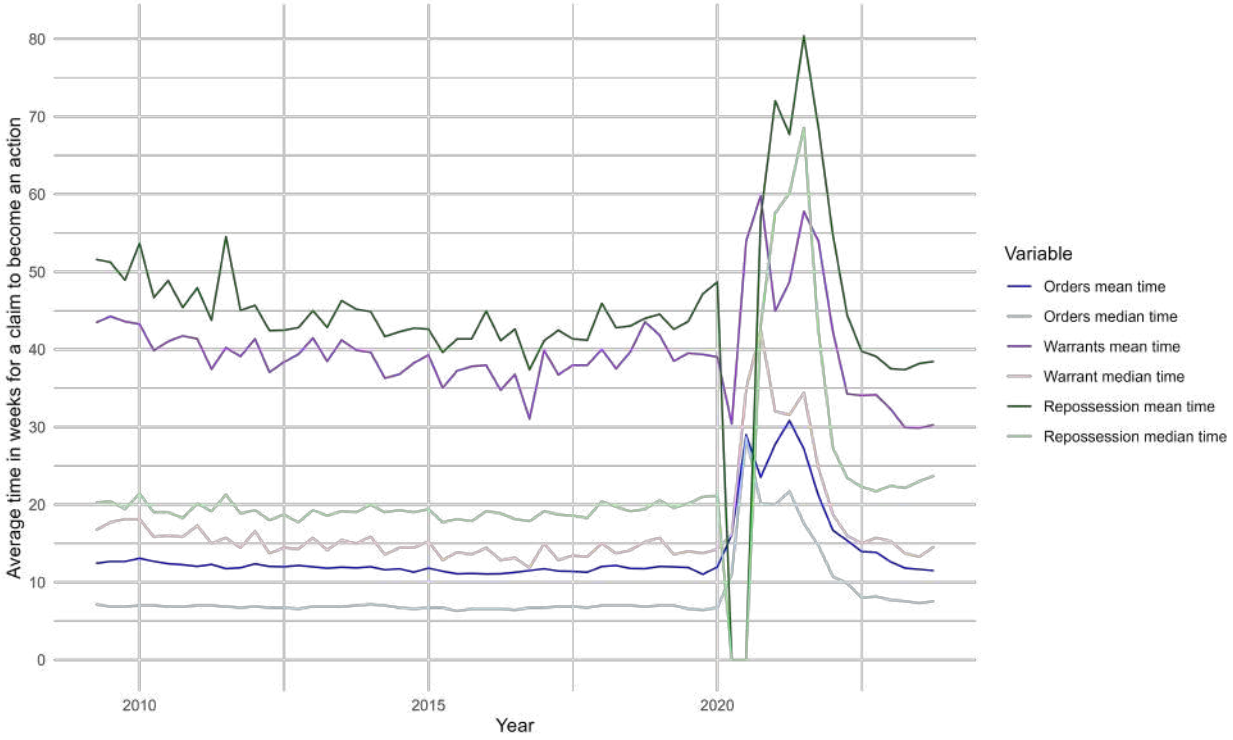


FIGURE 48: Average time for possession claims to become an order, warrant and repossession over time

Source: Ministry of Justice Mortgage and Landlord Possession Statistics.

Note: Quarterly National Statistics on the average length of time (median/mean) for possession claims to become an order, warrant and repossession. This graph only covers actions initiated by landlords in the county courts of England and Wales and excludes mortgage cases. Time is measured in weeks.

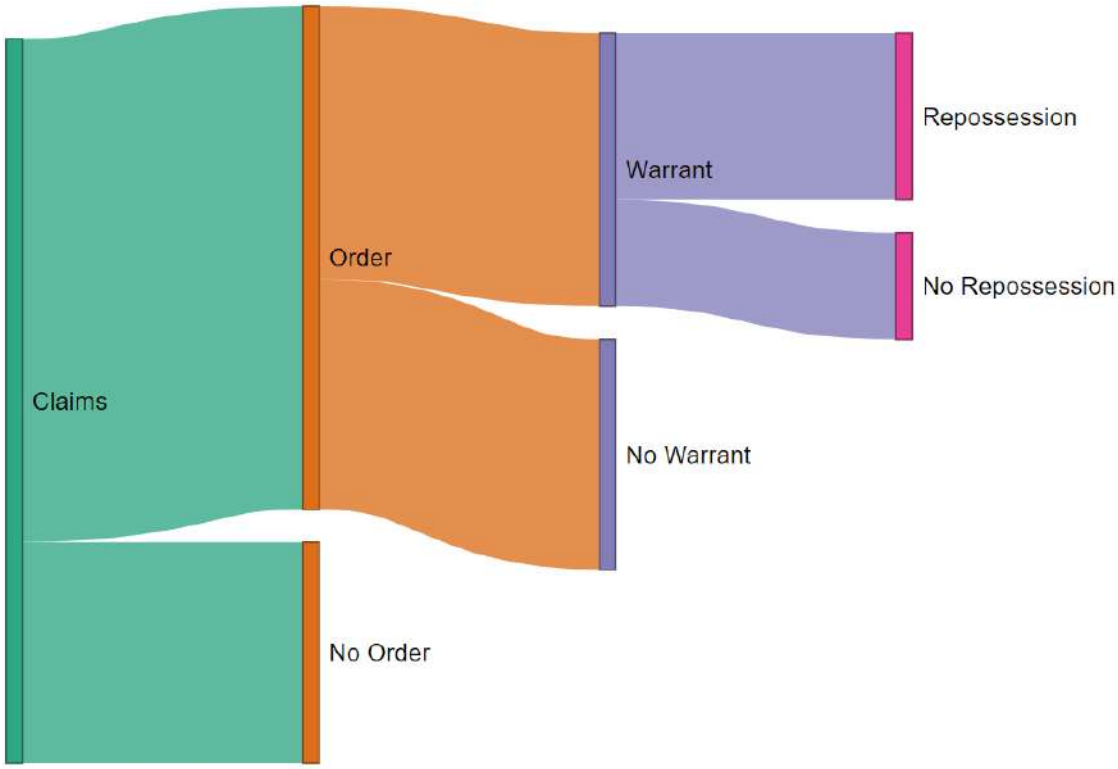


FIGURE 49: Flowchart eviction proceedings outcomes

Source: Ministry of Justice Mortgage and Landlord Possession Statistics Data Visualisation Tool

E.3 Baseline patterns in legal aid provision: Spatial inequality and specialisation

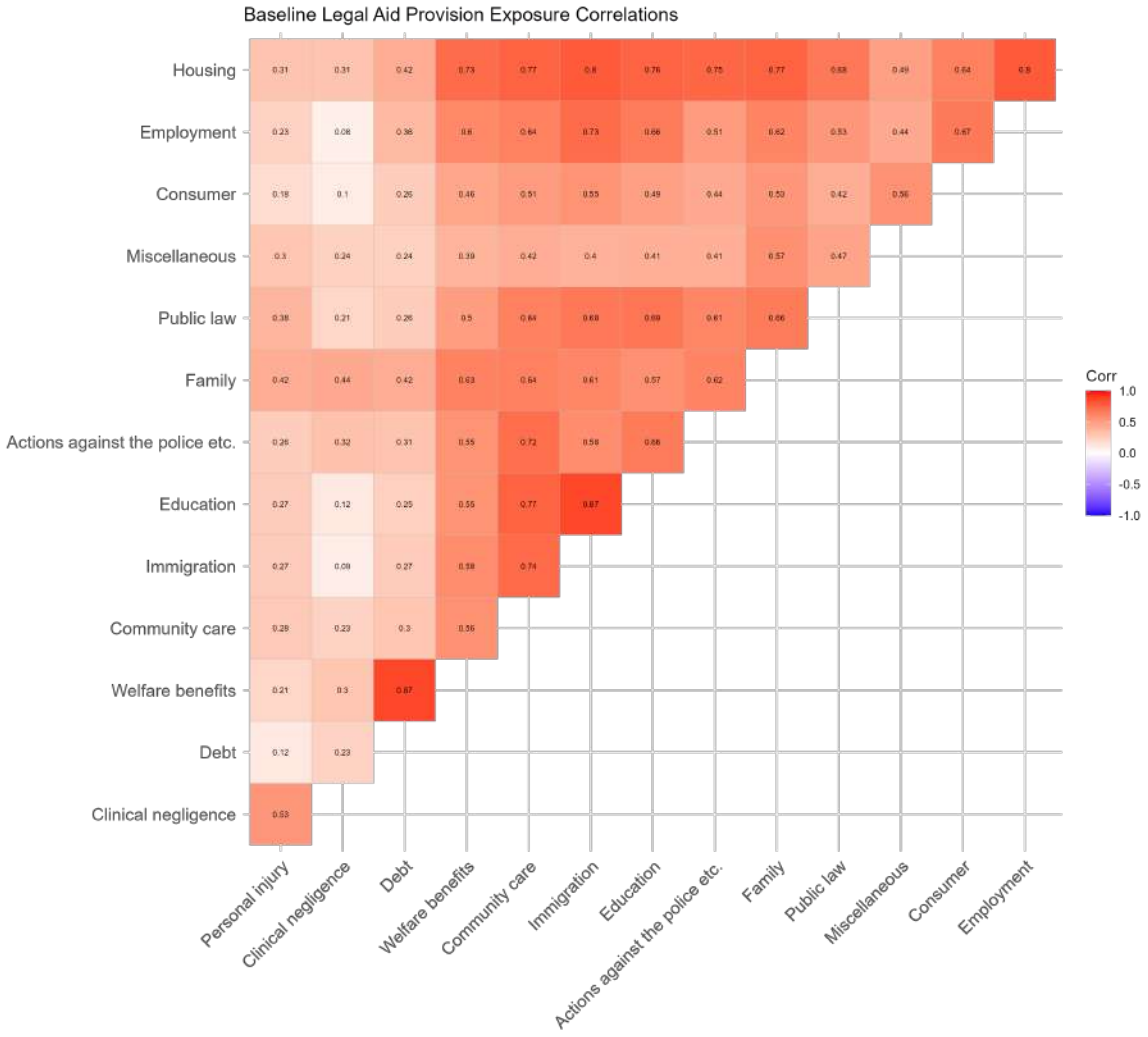


FIGURE 50: Baseline Legal Aid Provision Correlations Across Areas of Law

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This correlation heatmap displays pairwise correlations between baseline legal aid provision levels across areas of law at the MSOA level. Each square is color-coded by correlation strength and annotated with correlation coefficients. High correlations suggest that areas with strong provision in one legal area tend to have strong provision in others, reflecting either geographic clustering of legal aid services and/or generalist providers offering services across multiple legal domains. The mean pairwise correlation across areas of law is 0.48, with 45 out of 91 area pairs exhibiting correlations above 0.5. While these values indicate substantial co-provision of legal aid services, a high correlation does not imply an abundance of provision but reflects similarity in relative provision levels across areas—that is, both high–high and low–low combinations contribute to positive correlations. Co-provision may therefore arise in contexts of both strong and weak overall access to legal aid. Baseline provision is measured using a distance-weighted capacity metric: for each area of law, we sum across all providers the inverse distance from the MSOA centroid to each provider in the 2009–10 financial year, weighted by the provider's case volume in that area in the same year. This measure corresponds to the share-side of our shift–share instrument, excluding the specialization adjustment and predicted shock components.

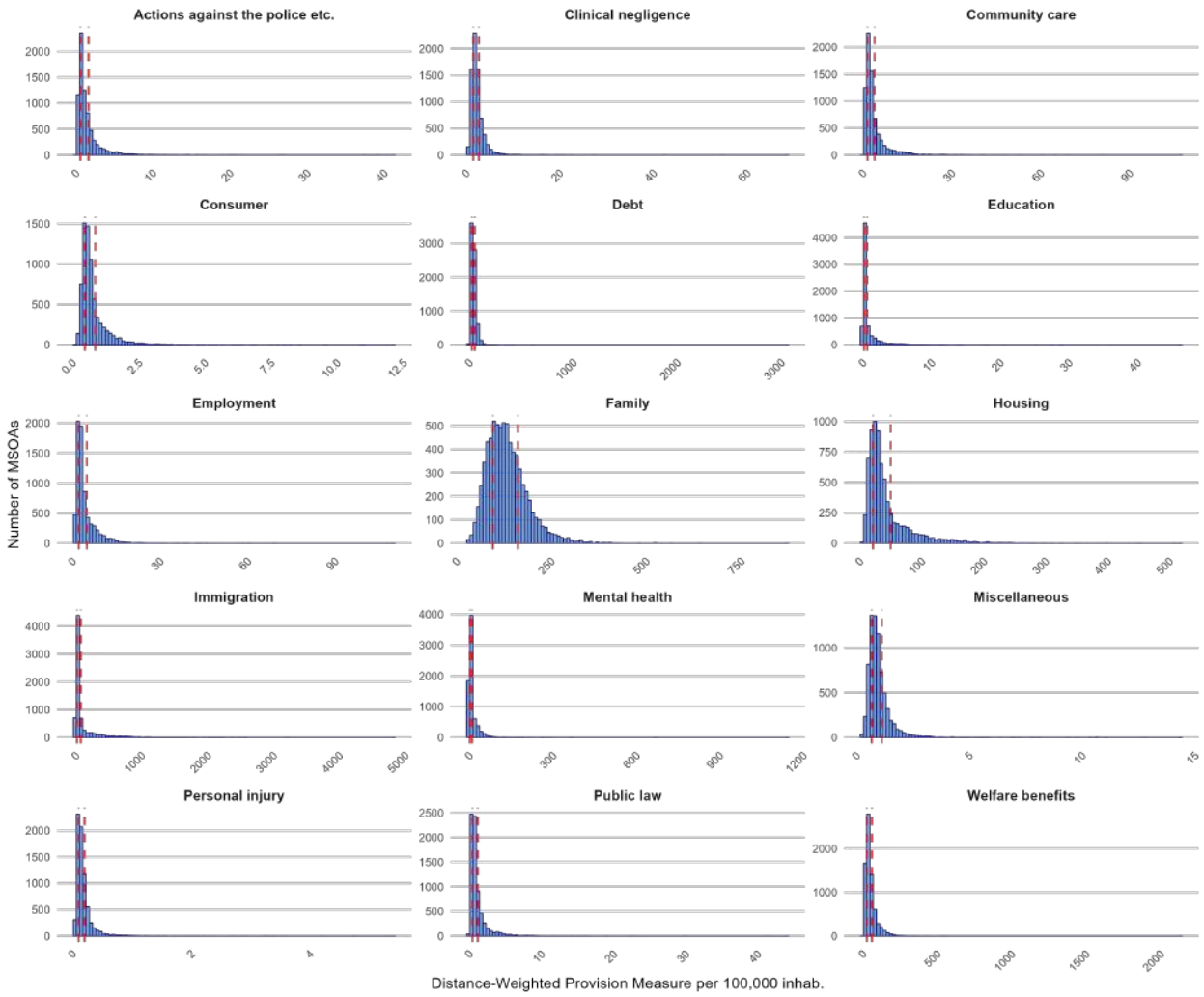


FIGURE 51: Distribution of Legal Aid Provision at Baseline by Area of Law

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: Each panel shows the distribution of baseline legal aid provision across MSOAs for a given area of law. Provision is measured using a distance-weighted capacity metric: for each area of law, we sum across all providers the inverse distance from the MSOA centroid to each provider in the 2009–10 financial year, weighted by the provider's case volume in that area in the same year. This measure corresponds to the share-side of our shift–share instrument, excluding the specialization adjustment and predicted shock components. Red dashed lines indicate the 25th and 75th percentiles of the distribution within each area of law.

E.3.1 Overall provision of legal aid in civil justice

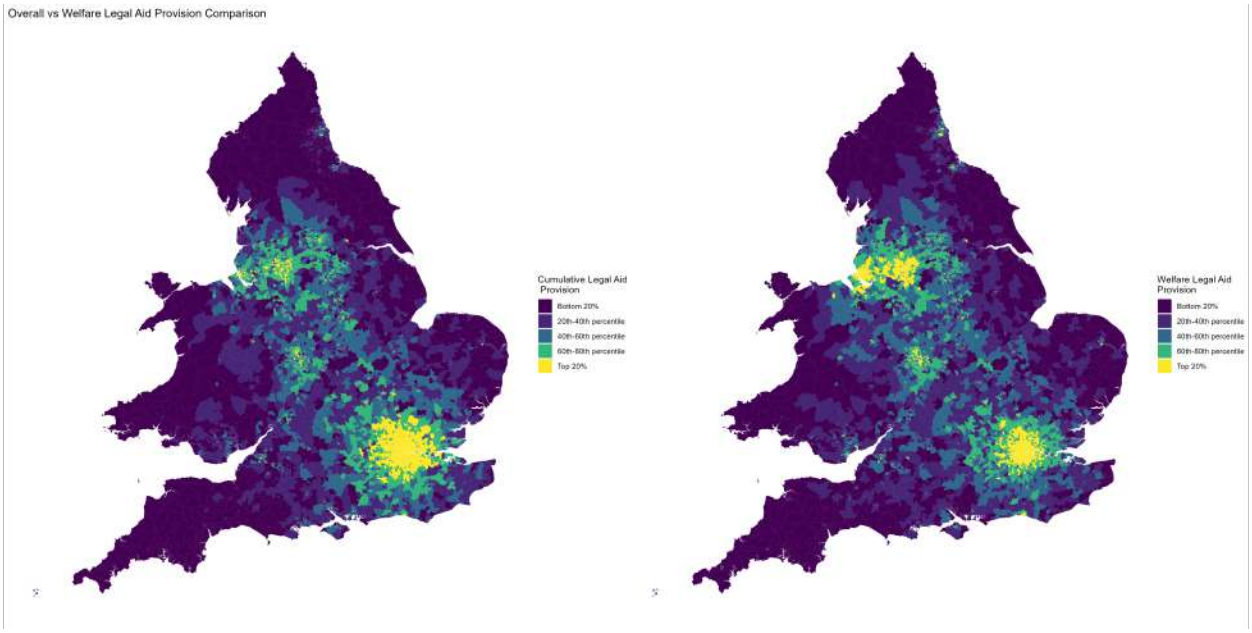


FIGURE 52: Baseline Distribution of Overall vs Welfare Legal Aid Provision

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: These maps compare the geographic distribution of overall legal aid provision (left panel) with welfare-specific provision (right panel) at baseline. The overall provision index (left) ranks MSOAs by their cumulative level of legal aid provision across all 15 areas of law, while the welfare provision index (right) focuses on three welfare-related services: Housing, Debt, and Welfare Benefits. For both measures, provision in each area of law is calculated using the distance-weighted capacity metric from financial year 2009-10, scaled per 100,000 population, with MSOAs ranked into quintiles based on their position in the respective national distributions. Lower provision (darker shading) indicates lower cumulative provision relative to other MSOAs in each measure. This composite measure captures geographic disparities in legal aid provision but does not reflect the diversity or breadth of services available locally — an area may rank highly nationally due to strong provision in few domains while lacking provision in others. The side-by-side comparison reveals the extent to which welfare legal aid provision mirrors overall provision patterns versus exhibiting distinct geographic concentration. Areas with similar shading across both maps indicate consistent provision patterns, while areas with divergent shading suggest specialization in either welfare-focused services or non-welfare legal domains.

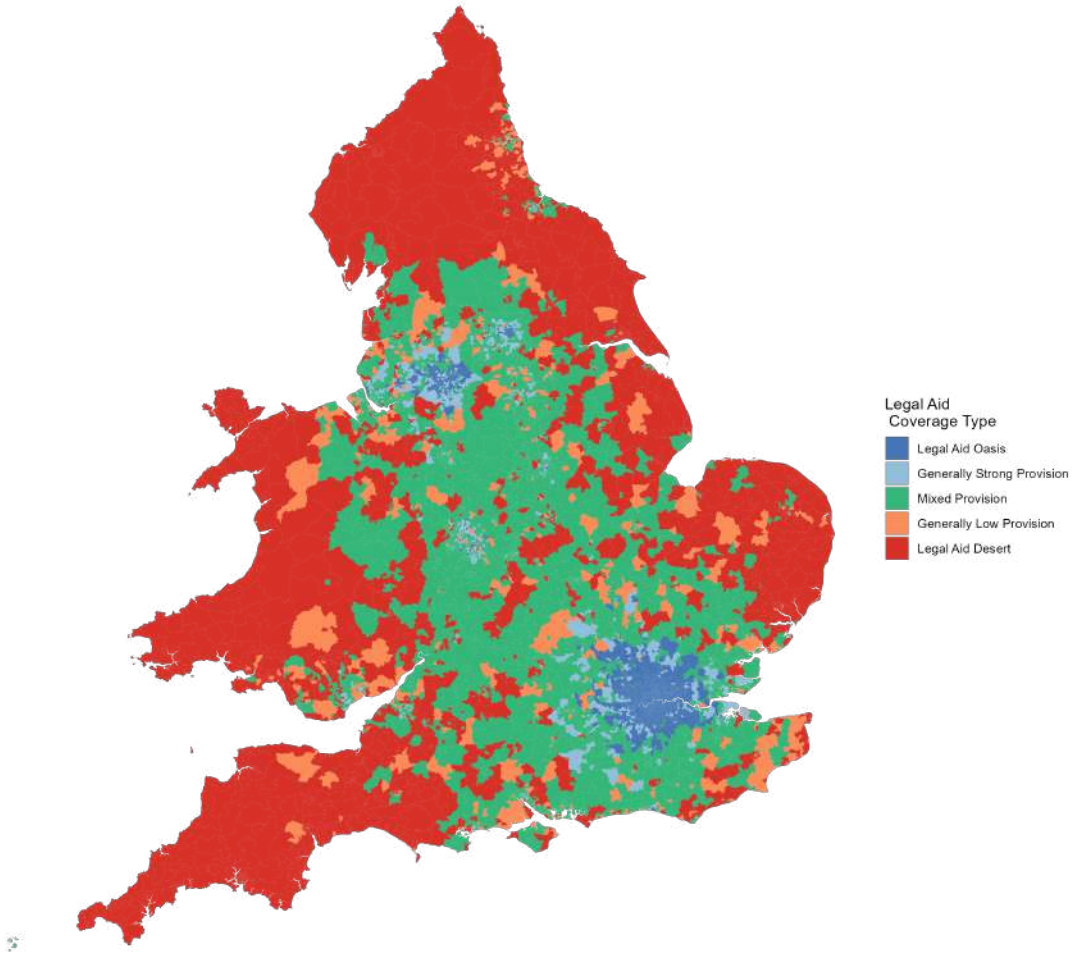


FIGURE 53: Geographical Classification of Areas Based on Abundance and Diversity of Legal Aid Services

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map classifies MSOAs into five categories based on their level and diversity of legal aid provision across multiple areas of law at baseline. Provision abundance is measured by identifying whether an MSOA's provision level for each area of law lies within the top quartile of the national distribution for that legal domain. Diversity is captured by counting the number of areas of law for which an MSOA has such high provision. "Legal Aid Oasis" areas have high provision (top quartile) in eight or more legal domains, representing diverse and abundant access. "Legal Aid Desert" areas have both zero high-provision in any area of law and low-provision (bottom quartile) in two or more areas of low, indicating systematically limited access "Generally High Access" areas have high provision in five or more areas of law without meeting the oasis threshold. "Generally Low Access" areas have low provision in five or more domains without meeting the desert criteria. "Mixed Access" areas exhibit heterogeneous patterns without clear dominance of high or low provision. This classification provides insight into both the breadth and concentration of legal aid services but does not directly account for the absolute level of services or demand in each area.

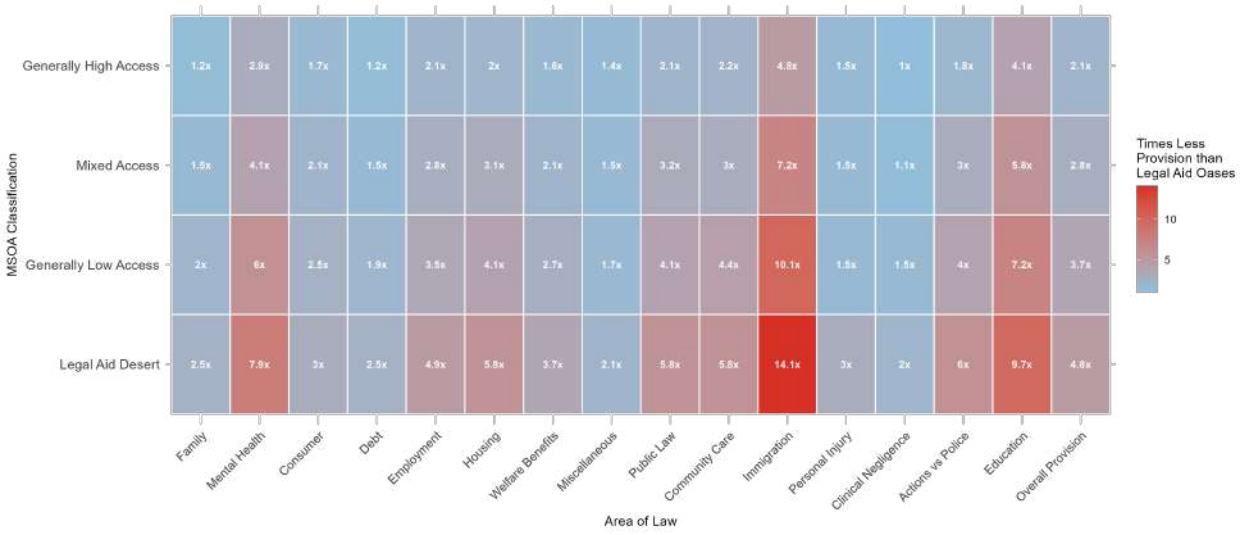


FIGURE 54: Legal Aid Provision Inequality: Gaps Relative to Legal Aid Oases

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: Note: Note: This heatmap presents provision gaps across area classified by service portfolio breadth and abundance. Each cell shows how many times less provision an area type has compared to Legal Aid Oases (19.8% of MSOAs with high provision in 8+ areas of law), calculated as ratios of mean provision levels. The classification captures different dimensions of legal aid inequality: Legal Aid Oases represent areas with comprehensive service portfolios; Generally High Access areas (10.2%) have strong provision in 5+ domains; Mixed Access areas (38.9%) show heterogeneous patterns; Generally Low Access areas (7.4%) have limited provision in multiple domains; and Legal Aid Deserts (23.8%) have systematically poor provision across most services. The heatmap reveals that even areas classified as "Generally High Access" have 1.2-2.4× less provision than Oases across most services, while Legal Aid Deserts face 2-6× gaps. Immigration and Mental Health services show the most extreme concentration in Oases (4-8× gaps), while Family and Debt services are more evenly distributed. This portfolio-based classification illustrates that geographic inequality operates through both overall provision levels and service specialization patterns, with nearly one-quarter of areas lacking adequate provision across multiple legal domains.

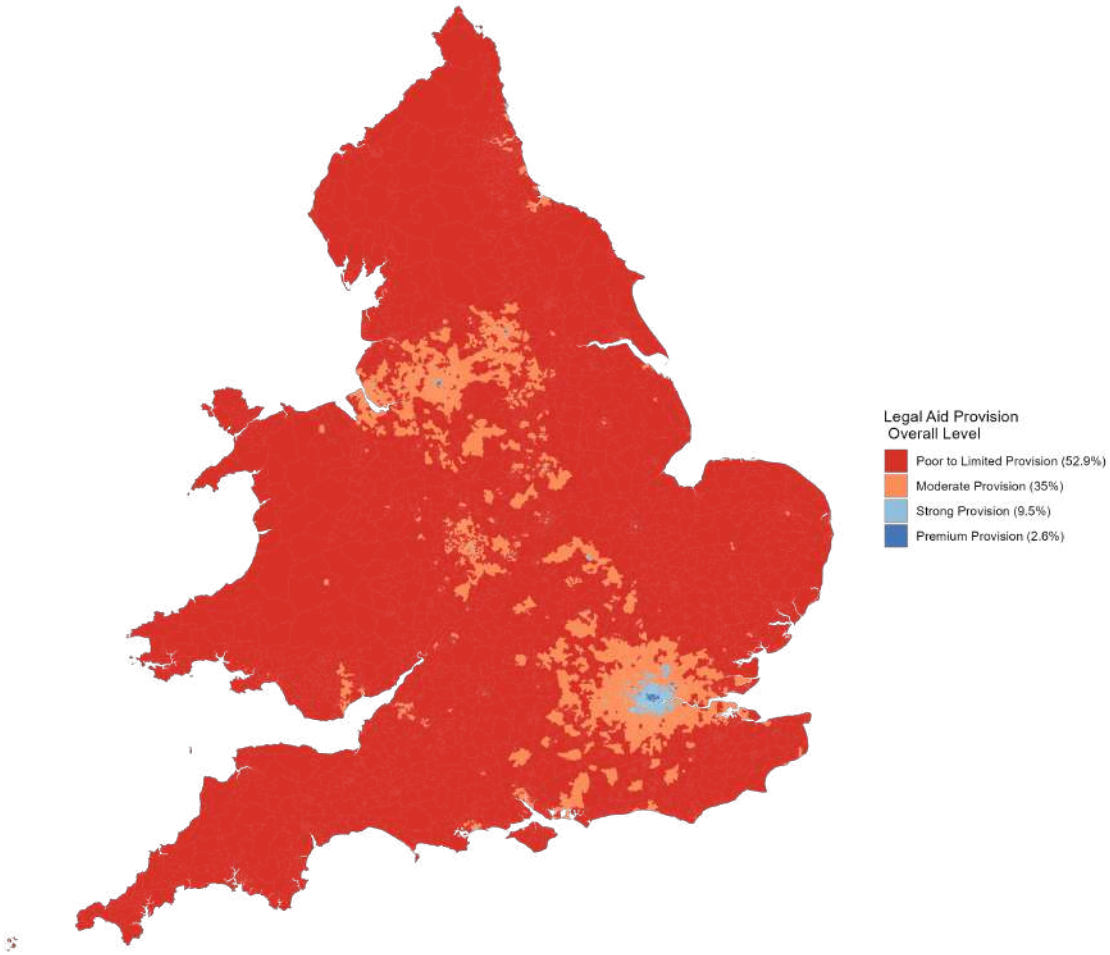


FIGURE 55: Geographic Classification of Legal Aid Provision Based on Multi-dimensional Clustering

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map presents a data-driven classification of MSOAs based on their legal aid provision patterns across all areas of law at baseline (2009-10). We apply k-means clustering to the full vector of distance-weighted provision measures for each MSOA, allowing the algorithm to identify natural groupings based on similarities in multi-dimensional provision patterns. Each cluster represents areas with similar provision profiles across the 15 legal service areas, capturing both the overall level and the pattern of specialization in legal aid services. The resulting classification reveals four distinct provision regimes: Premium areas (2.6% of MSOAs) with consistently high provision across most services; Strong areas (9.5%) with above-average provision; Moderate areas (35%) with below-average but variable provision; and Poor to Very Limited areas (52.9%) with systematically low provision across most legal domains.

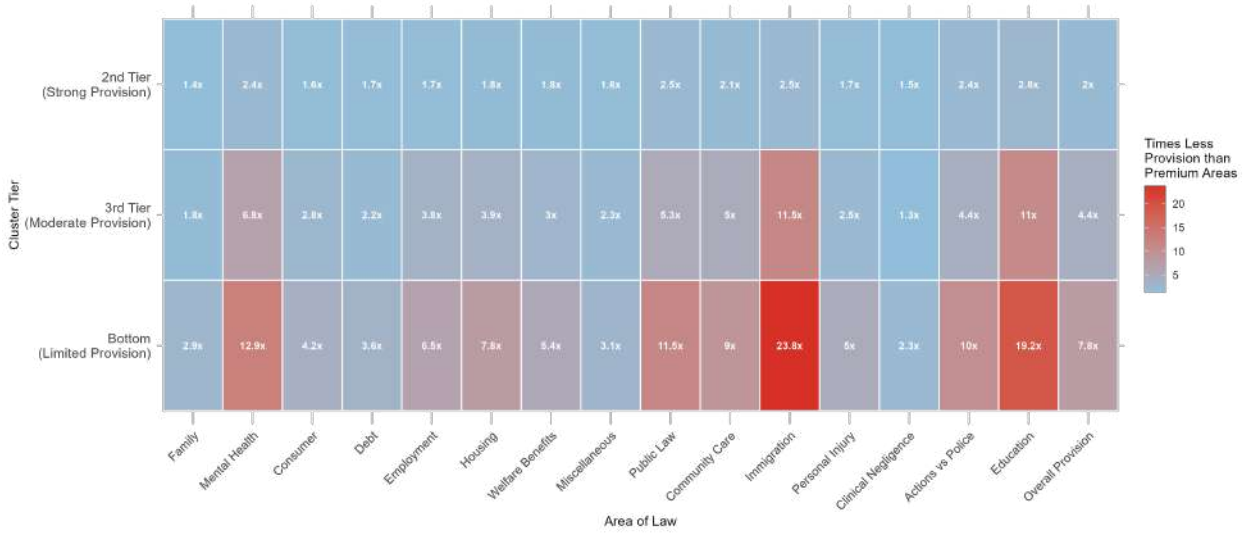


FIGURE 56: Legal Aid Provision Inequality: Cluster-Specific Gaps Relative to Premium Areas

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: Note: This heatmap quantifies the magnitude of legal aid provision inequality across areas of law and geographic clusters. Each cell shows how many times less provision a cluster has compared to Premium areas (the top 2.6% of MSOAs) for each area of law. Values are calculated as the ratio of mean provision in Premium areas to mean provision in each respective cluster. Darker red indicates more severe inequality. The analysis reveals substantial variation in inequality across both legal domains and geographic tiers. Immigration and Education services exhibit the most extreme geographic concentration, with Bottom-tier areas having 24× and 19× less provision respectively than Premium areas. Even common areas like Housing and Welfare Benefits show 8× and 5× gaps respectively. The rightmost column shows overall provision inequality, where Bottom-tier areas have nearly 8× less cumulative legal aid provision than Premium areas. This systematic inequality demonstrates that legal aid access varies not just in overall level but also in the degree of specialization available locally, with some areas facing compounded disadvantage across multiple legal domains.

E.3.2 Provision of legal aid in social welfare law

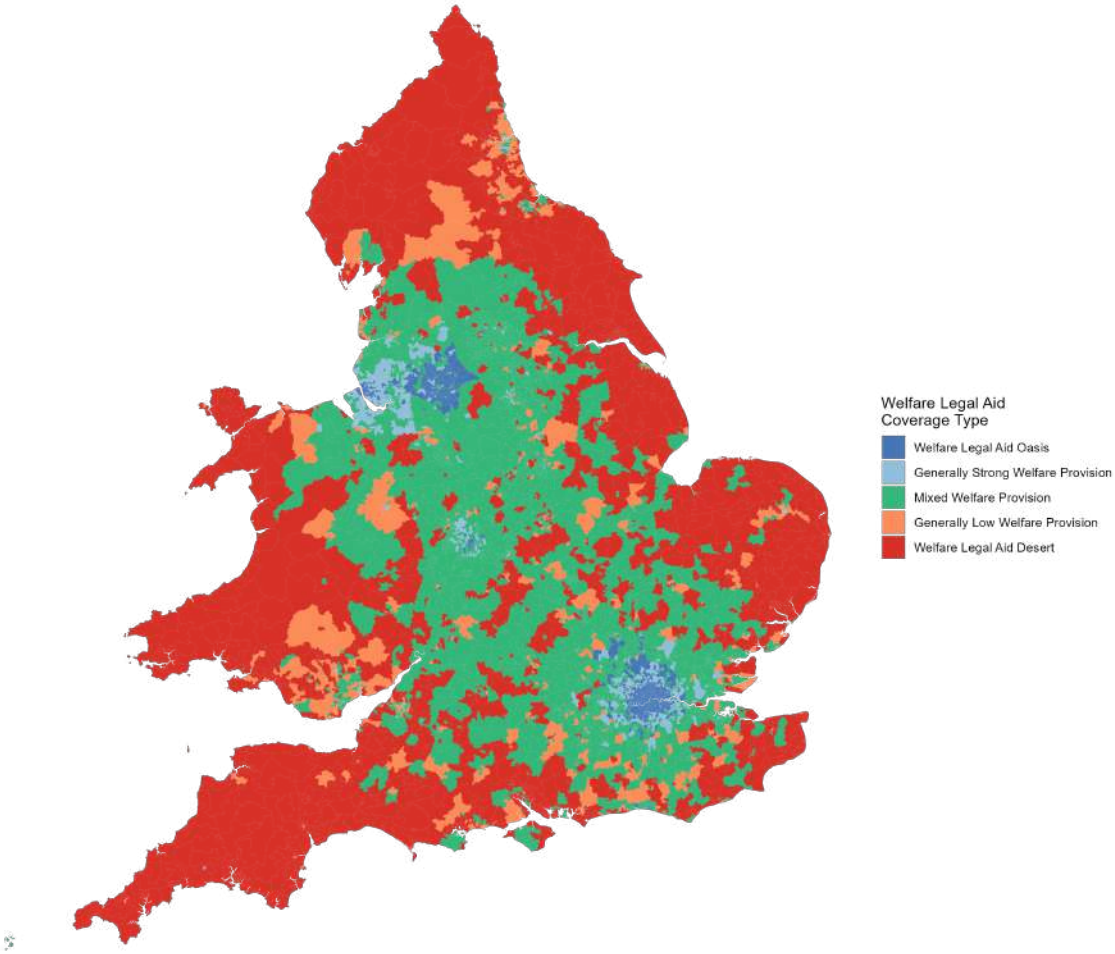


FIGURE 57: Welfare Legal Aid Coverage Classification

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map classifies MSOAs based on their welfare legal aid provision portfolio across Housing, Debt, and Welfare Benefits services. The classification combines the number of welfare services where an MSOA ranks in the top quartile nationally with the number where it ranks in the bottom quartile. "Welfare Legal Aid Oasis" areas (15.1% of MSOAs) rank in the top quartile for all three welfare services, representing comprehensive welfare legal aid coverage. "Welfare Legal Aid Desert" areas (24.6%) rank in the bottom quartile for at least two welfare services while ranking in the top quartile for none, indicating systematically limited welfare provision. "Generally Strong Welfare Provision" areas (9.8%) rank in the top quartile for at least two welfare services, while "Generally Low Welfare Provision" areas (8.5%) rank in the bottom quartile for at least one service without meeting the desert criteria. "Mixed Welfare Provision" areas (42%) exhibit heterogeneous patterns without clear dominance. This classification reveals the geographic distribution of comprehensive versus limited welfare legal aid infrastructure.

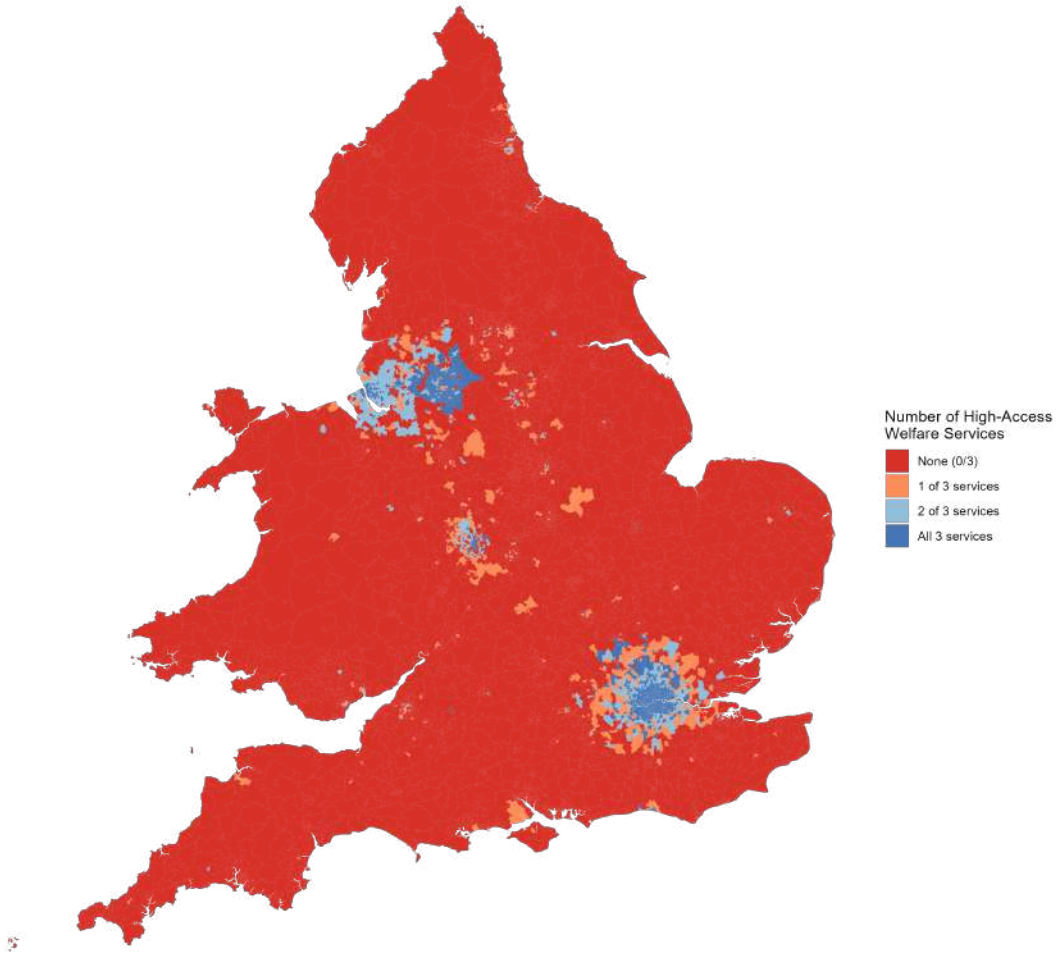


FIGURE 58: Number of Welfare Legal Services with Top Quartile Provision

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map displays the number of welfare legal aid services (Housing, Debt, and Welfare Benefits) for which each MSOA ranks in the top quartile of the national provision distribution. For each service area, the top quartile represents MSOAs with the highest 25% of distance-weighted capacity levels, scaled per 100,000 population, measured at baseline (2009-10). Areas shaded in dark blue rank in the top quartile for all three welfare services, indicating comprehensive high-level welfare legal aid provision across Housing, Debts and Welfare Benefits. Areas in red rank in the top quartile for none of the three services, suggesting limited access to welfare legal aid across all domains. This measure captures the breadth of welfare legal aid abundance, revealing areas where households have access to consistently strong legal support across multiple welfare-related legal issues versus areas where such comprehensive coverage is limited.

E.3.3 How the provision of social welfare legal aid relates to overall provision

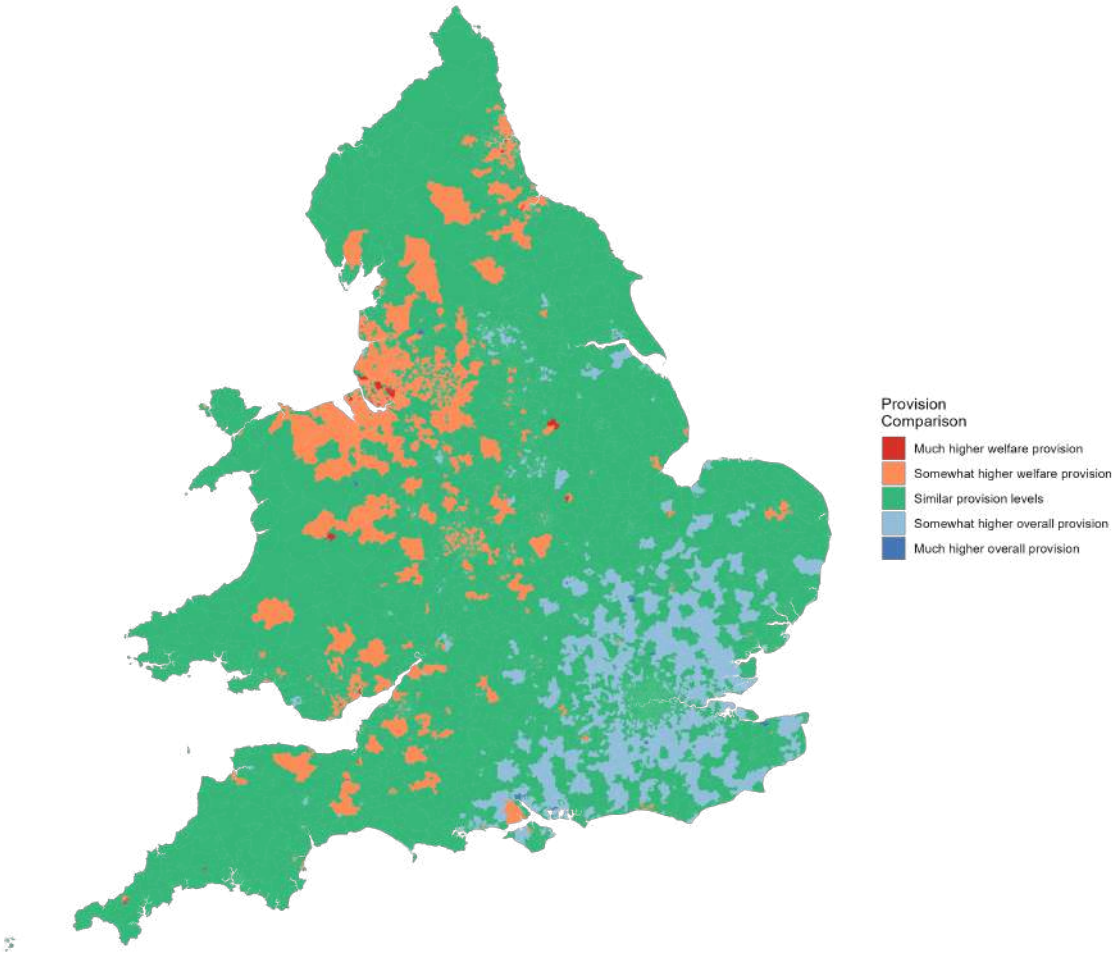


FIGURE 59: Difference Between Overall and Welfare Legal Aid Provision Quintile Classifications

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map compares each MSOA's quintile classification for overall legal aid provision (cumulative provision per 100,000 inhabitants across all 15 areas of law) with its quintile classification for welfare-specific provision (cumulative provision across Housing, Debt, and Welfare Benefits services). The difference is calculated as overall provision quintile minus welfare provision quintile. Areas in blue ("much higher overall provision") fall into higher quintiles for overall provision than for welfare provision, indicating that while they have relatively strong legal aid infrastructure overall, their welfare-focused services are proportionally weaker. Areas in red ("much higher welfare provision") show the opposite pattern - they perform better in welfare provision quintiles than in overall provision quintiles, suggesting concentration of resources in welfare-related legal services. Yellow areas ("similar provision levels") fall into similar quintiles for both measures, indicating consistent provision patterns across legal domains. This analysis reveals whether areas with strong (or weak) overall legal aid provision also have correspondingly strong (or weak) welfare legal aid provision, or whether there are systematic differences in service concentration patterns.

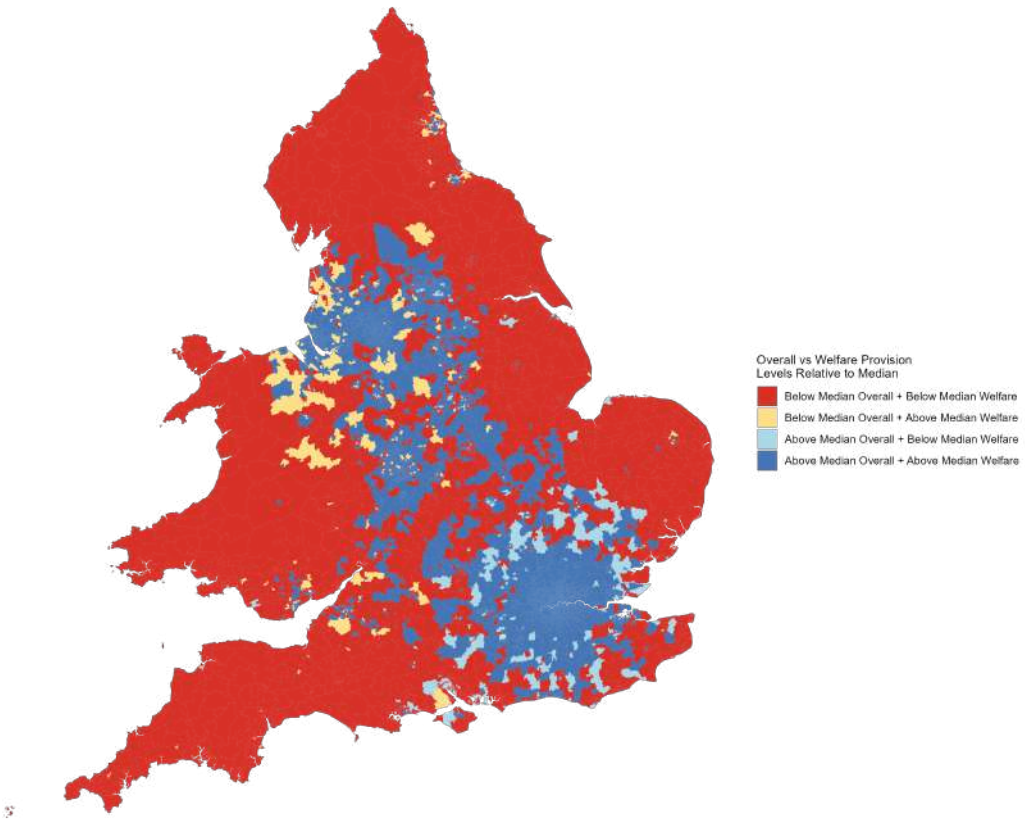


FIGURE 60: Bivariate Classification of Overall vs Welfare Legal Aid Provision

Source: Ministry of Justice Legal Aid Statistics, author's calculations

Note: This map presents a bivariate classification comparing overall legal aid provision (across all 15 areas of law) with welfare-specific provision (Housing, Debt, and Welfare Benefits services). Each MSOA is classified as above or below the national median for both dimensions, creating four distinct categories. "Both Above Median" areas (56% of MSOAs) have strong provision across all legal services including welfare areas. "Both Below Median" areas (36%) have systematically limited provision across all domains. "Overall Below, Welfare Above" areas (4%) represent welfare specialists with concentrated provision in housing, debt, and benefits services despite limited overall provision. "Overall Above, Welfare Below" areas (4%) have relatively abundant legal aid provision but relatively weak welfare-focused services. This classification reveals the extent to which welfare legal aid provision mirrors overall provision patterns versus exhibiting distinct geographic specialization.

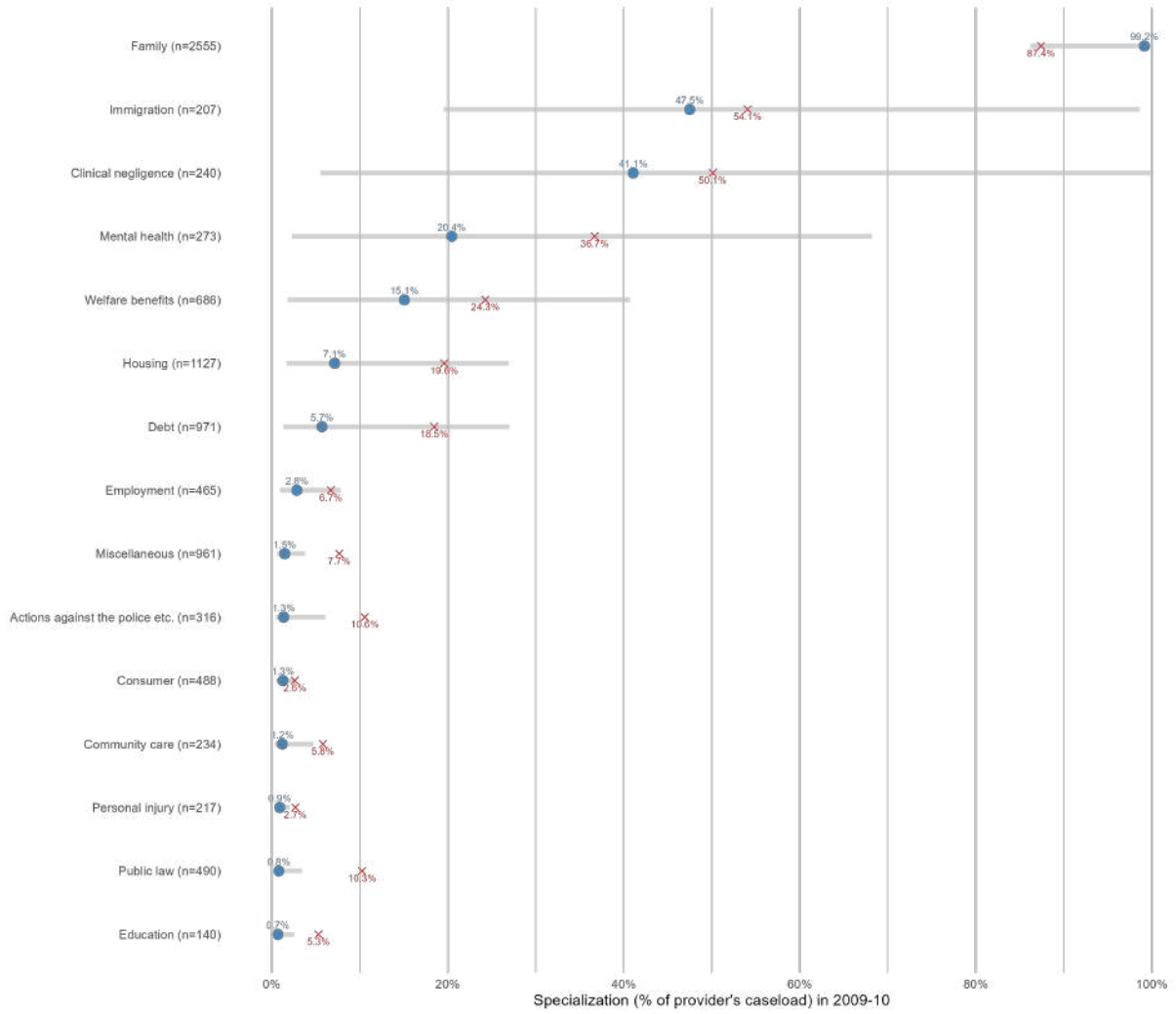


FIGURE 61: Provider Specialization Patterns by Area of Law at Baseline (2009-10)

Notes: Points show the distribution of provider specialization levels across 15 areas of law. Blue dots indicate individual providers; red X marks the mean specialization level. Horizontal gray bars show interquartile range. Sample sizes (n) indicate the number of providers active in each area. Specialization is measured as the percentage of a provider's total caseload dedicated to each area of law. Family law shows the highest specialization (mean 82%), with most providers focusing exclusively on this area. Immigration (47%) and Clinical negligence (41%) also show concentrated specialization. In contrast, poverty law areas—Housing (19%), Debt (16%), and Welfare benefits (22%)—exhibit lower specialization, reflecting providers' tendency to handle these interconnected issues jointly. This pattern reveals the distinct service models: specialist provision in family and immigration law versus generalist provision in social welfare law.



FIGURE 62: Evolution of the number of legal aid cases opened by type of legal assistance, other legal domains

Source: Ministry of Justice Legal Aid Statistics
Note: These graphs depict the evolution of the number of legal aid cases opened in England and Wales using aggregate statistics available from financial year 2000-01 to 2022-23, providing a longer time horizon than the panel dataset used in the main analysis, which begins in 2009-10. This extended period illustrates the demand-driven component of the scheme and the magnitude of the 2011-13 cuts from a longer-term perspective. Breakdown by type of legal assistance is only fully available starting in financial year 2006-07; the earlier period only covers legal help cases. Legal aid work is categorized into two main types: legal help and civil representation. According to the Ministry of Justice definitions, legal help includes advice and assistance about a legal problem but does not include representation or advocacy in proceedings, while civil representation is representation by solicitors and barristers for civil cases that could go to court. The vertical dashed lines mark key reform milestones: proposals (November 2010), bill introduction (June 2011), fees cut (October 2011), royal assent (May 2012), and implementation of LASPO (April 2013). The shaded areas highlight the transition period (light gray) and the period of highest change (dark gray).

E.4 Dynamic of entries and exits of providers across areas of law

E.4.1 Flows of providers over time

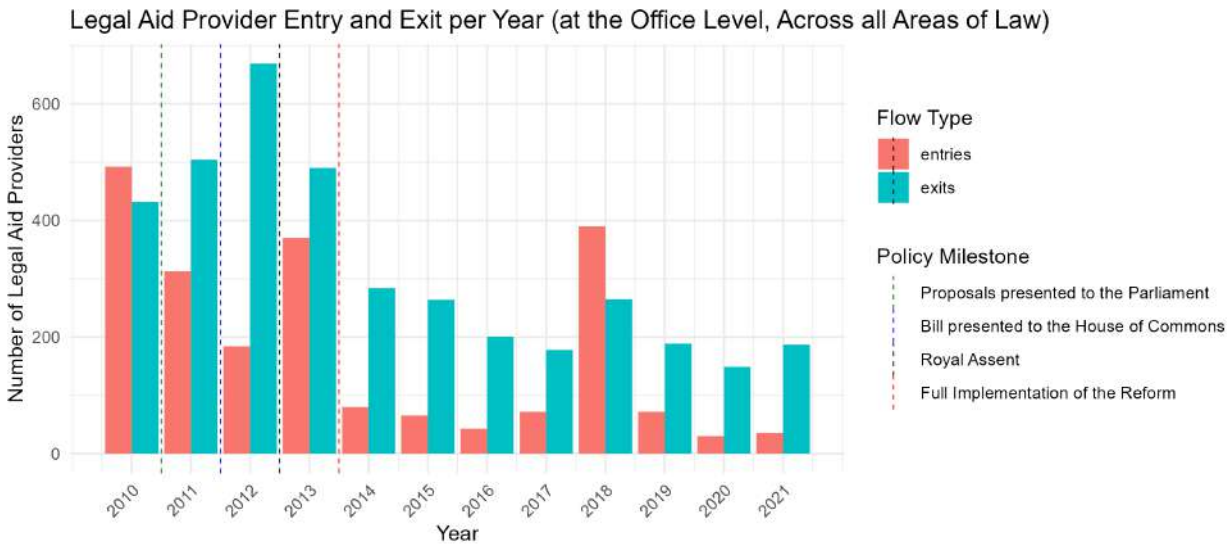


FIGURE 63: Entry and Exit Flows of Legal Aid Providers Over Time

Note: This graph displays the number of legal aid providers entering or exiting the market in each financial year, across all areas of law. A provider is considered to have entered the market when they open at least one legal aid case in a given location within the financial year, having not done so in any previous year. Conversely, a provider is classified as having exited when they do not initiate any new legal aid cases in all subsequent years. The dashed line indicates key policy milestones related to the adoption and implementation of the LASPO reform.

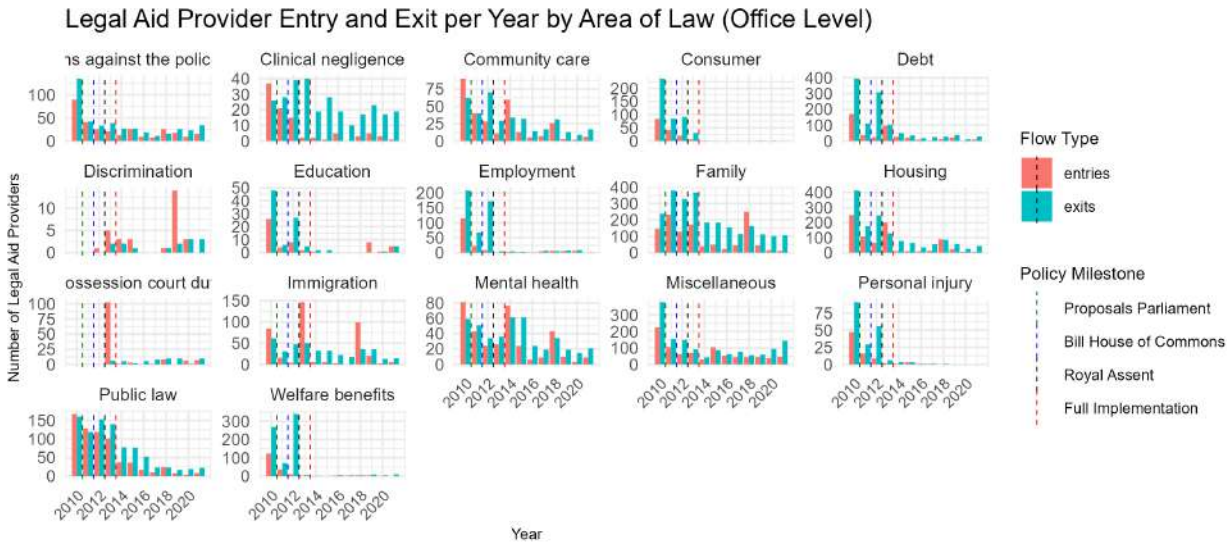


FIGURE 64: Entry and Exit Flows of Legal Aid Providers Over Time, by Area of Law

Note: This graph shows the number of legal aid providers entering or exiting the market in each financial year, broken down by area of law, using the classification adopted by the Ministry of Justice in the Legal Aid Statistics. A provider is considered to have entered the market in a specific area of law when they open at least one legal aid case in that domain and location during the financial year, having not done so in any previous year. Conversely, a provider is classified as having exited an area of law when they do not initiate any new legal aid cases in that domain in all subsequent years. The dashed line marks key policy milestones related to the adoption and implementation of the LASPO reform. Note that providers may operate across multiple areas of law; as such, figures across categories should not be summed to obtain total market entries or exits.

E.4.2 Specialisation and Consolidation Dynamics

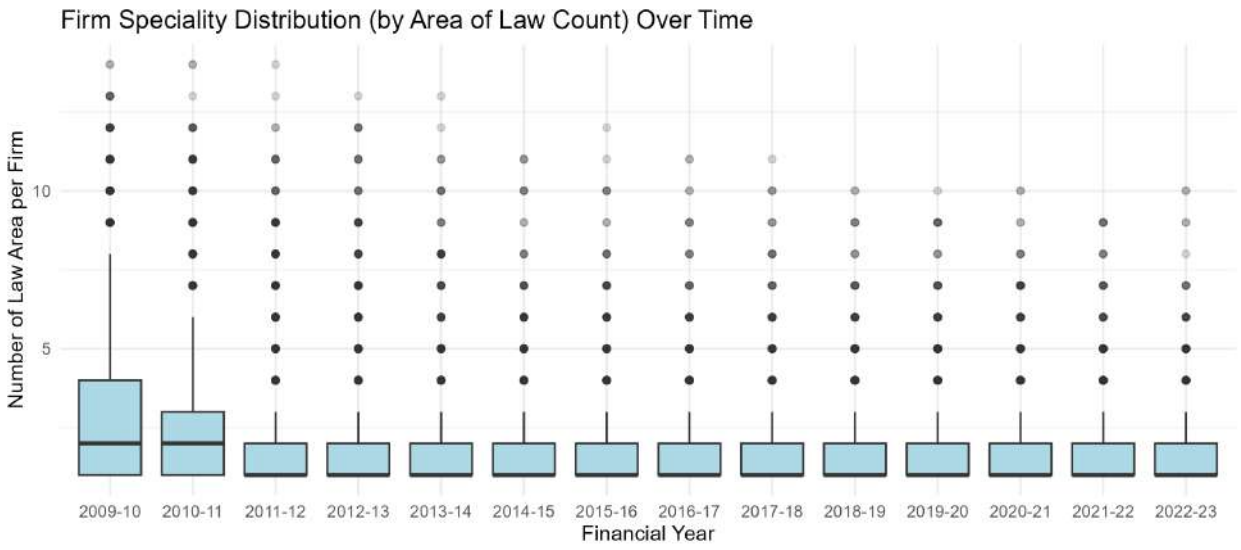


FIGURE 65: Boxplots Illustrating the Level of Legal Aid Provider Specialisation Over Time

Note: This graph displays boxplots showing the distribution of the number of areas of law covered by each legal aid provider across financial years from 2009–10 to 2022–23. Each box represents the interquartile range (IQR), with the line inside showing the median number of areas. Individual dots indicate outliers—providers with unusually high or low numbers of areas relative to others in that year. The graph allows for comparison of both central tendency and variation in provider specialisation over time.

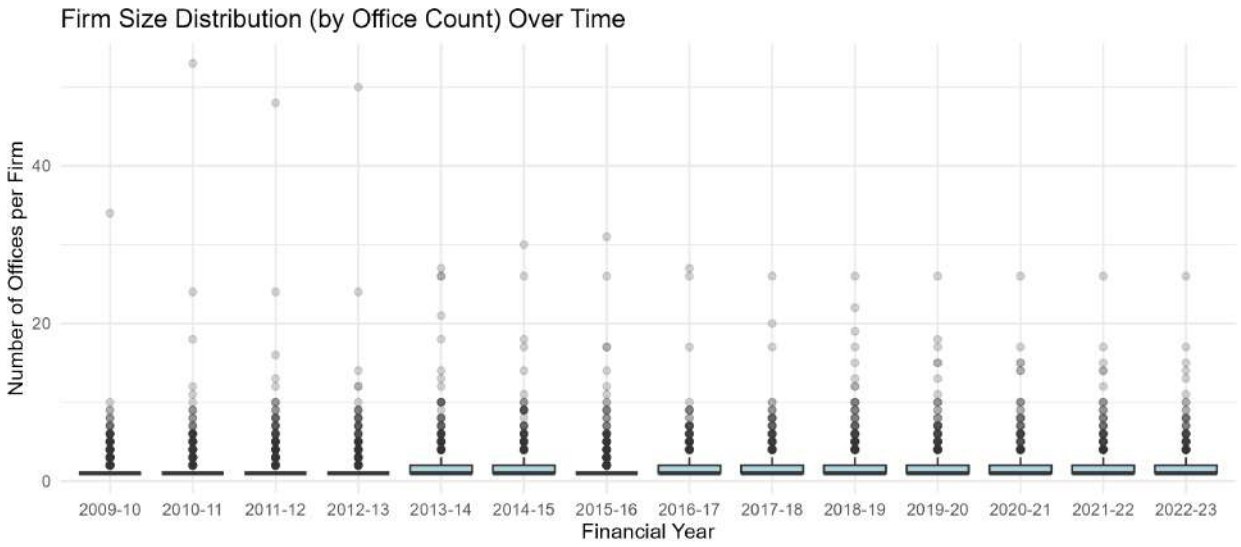
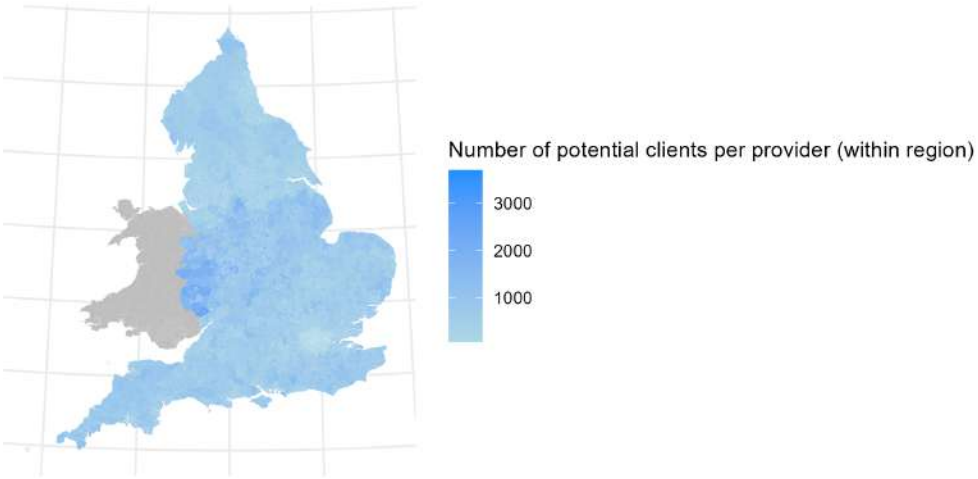


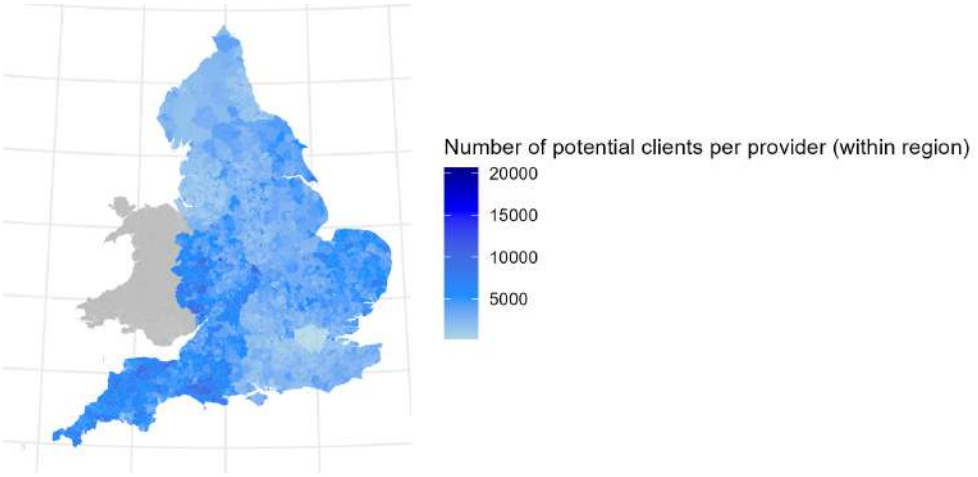
FIGURE 66: Boxplots Illustrating the Level of Legal Aid Firm Consolidation Over Time

Note: This graph displays boxplots showing the distribution of the number of offices operated by each legal aid firm across financial years from 2009–10 to 2022–23. Each box represents the interquartile range (IQR), with the line inside indicating the median number of offices. Individual dots represent outliers—firms with unusually high or low office counts relative to others in that year. The graph allows for comparison of both central tendency and variation in firm consolidation over time.

E.5 Dynamics of needs for legal assistance relative to change in provision



(a) 2011



(b) 2021

FIGURE 67: Estimated Clients to Providers Ratio for Housing Legal Aid by Region, in England

Source: Activities of legal aid providers based on the Ministry of Justice Legal Aid Statistics; Possession claims based on Freedom of Information data from the Ministry of Justice; Statutory Homelessness Statistics; Fuel Poverty Statistics.
Note: These maps depict the relative demand for housing legal aid across regions in 2011 and 2021, respectively. For each of the ten regions defined by the 2011 Census, we consider the number of active housing legal aid providers in 2011 and 2021. At the MSOA level, we estimate the average number of households in needs of legal assistance, proxied by the frequency of three events: possession claims, fuel poverty and assessment for homelessness prevention or duty. We then compare the potential demand for legal aid with the actual provision at the regional level to estimate the number of potential clients per provider for each region.

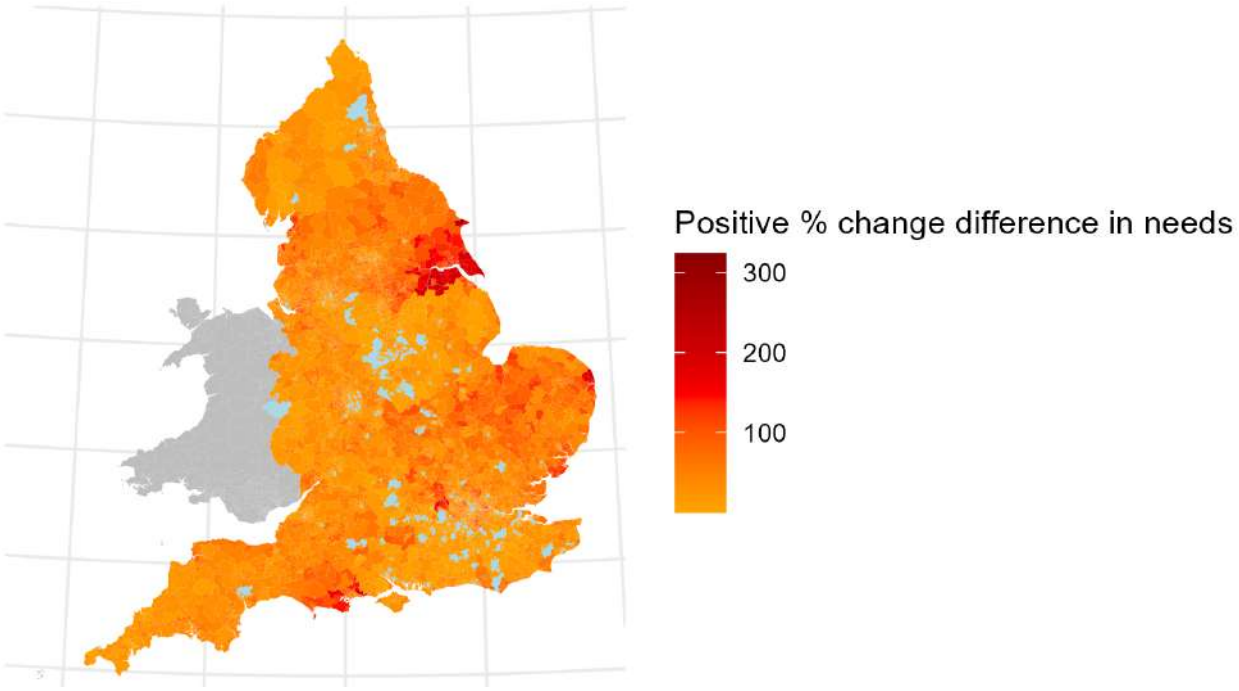


FIGURE 68: Evolution of housing legal needs between 2011 and 2021

Note: This map depicts the relative increase in demand for housing legal aid in England between 2011 and 2021. To measure the demand, we estimate the average number of households at the MSOA (Middle Layer Super Output) level that experience one of the following events: facing a possession claim, being in fuel poverty, or having been assessed for homelessness prevention or relief by the local authority. These events serve as small-area proxies for housing legal aid needs. Areas in light blue have experienced a slight decrease in demand over time. Our indicators are scaled by population density at the MSOA level, using mid-year population estimates in 2011 and 2020 from the ONS.

E.6 Evolution of the number of cases and providers by area of law

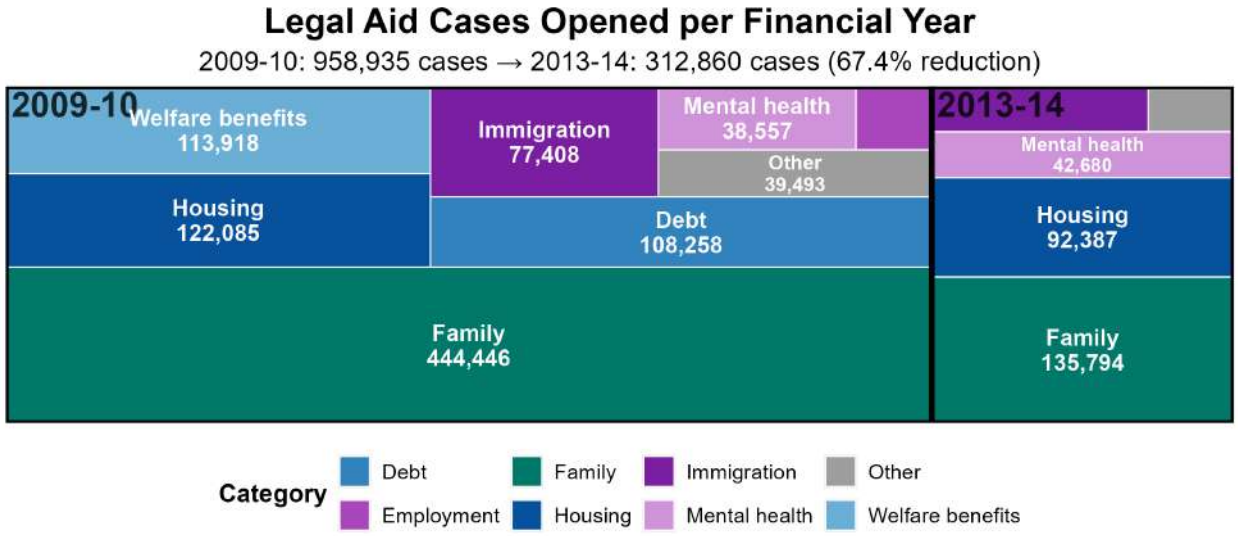


FIGURE 69: Impact of Legal Aid Reform on Case Volume by Area of Law

Source: Ministry of Justice Legal Aid Statistics, author's calculations.
Note: Treemaps show the composition and volume of legal aid cases opened in the baseline year (2009-10, left) and first year post-reform (2013-14, right). Area sizes are proportional to case volumes within each year. The reform resulted in a 67.4% reduction in total cases, from 958,935 to 312,860. Welfare benefits cases fell from 113,918 to effectively zero (area too small to display), while debt cases declined by over 90%. Family law cases, though reduced from 444,446 to 135,794, became proportionally dominant post-reform. Housing cases shown primarily represent eviction proceedings, the only housing matters remaining in scope. Mental health retained funding but with reduced volumes. The visual starkly illustrates the reform's transformation from broad preventive coverage to narrow crisis intervention.

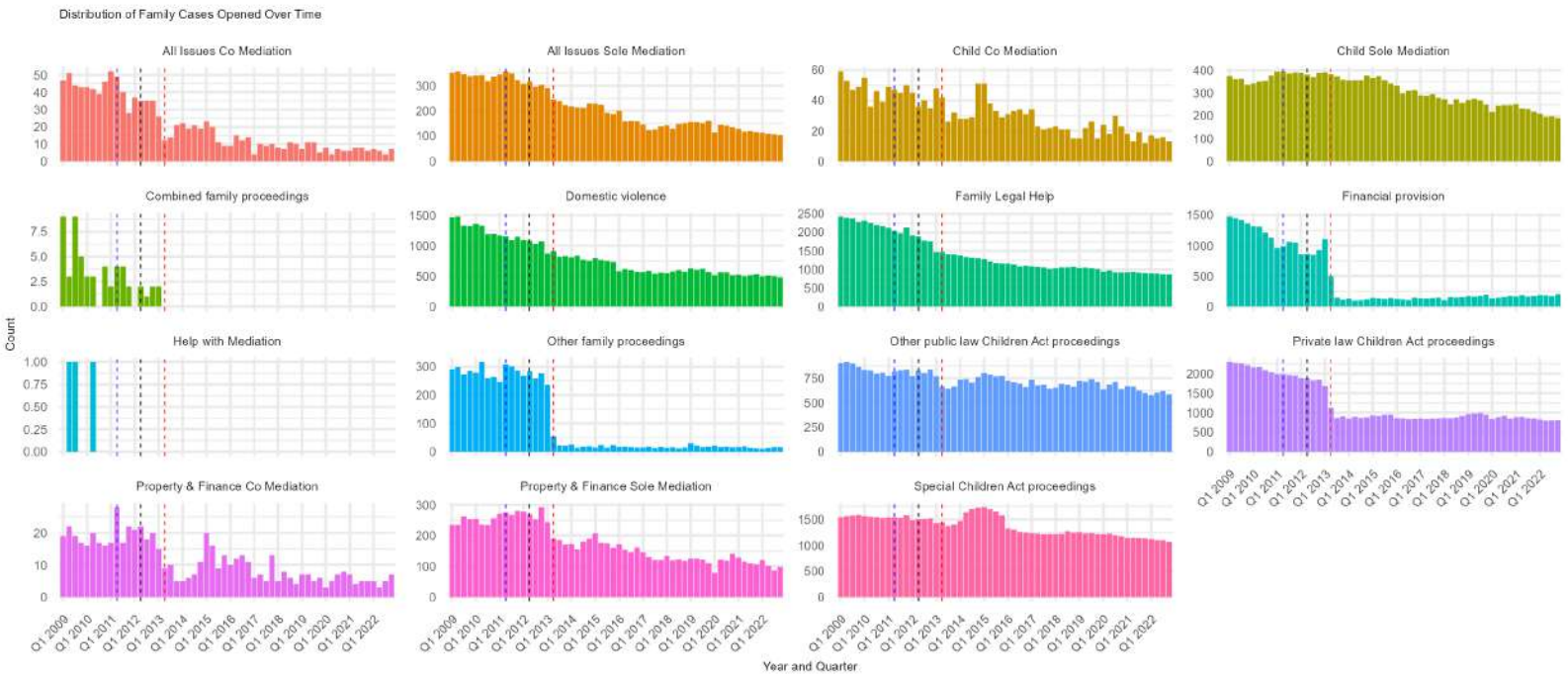


FIGURE 70: Trends in Legal Aid Cases Opened Each Quarter in Family Law, by Area of Law

Source: Ministry of Justice Legal Aid Statistics, author's calculations.

Note: These graphs show the number of legal aid cases opened each quarter by all providers in England and Wales, broken down by area of law. The classification as "Family Law" follows the Ministry of Justice classification (Ministry of Justice of the United Kingdom (2024) pages 48-49).

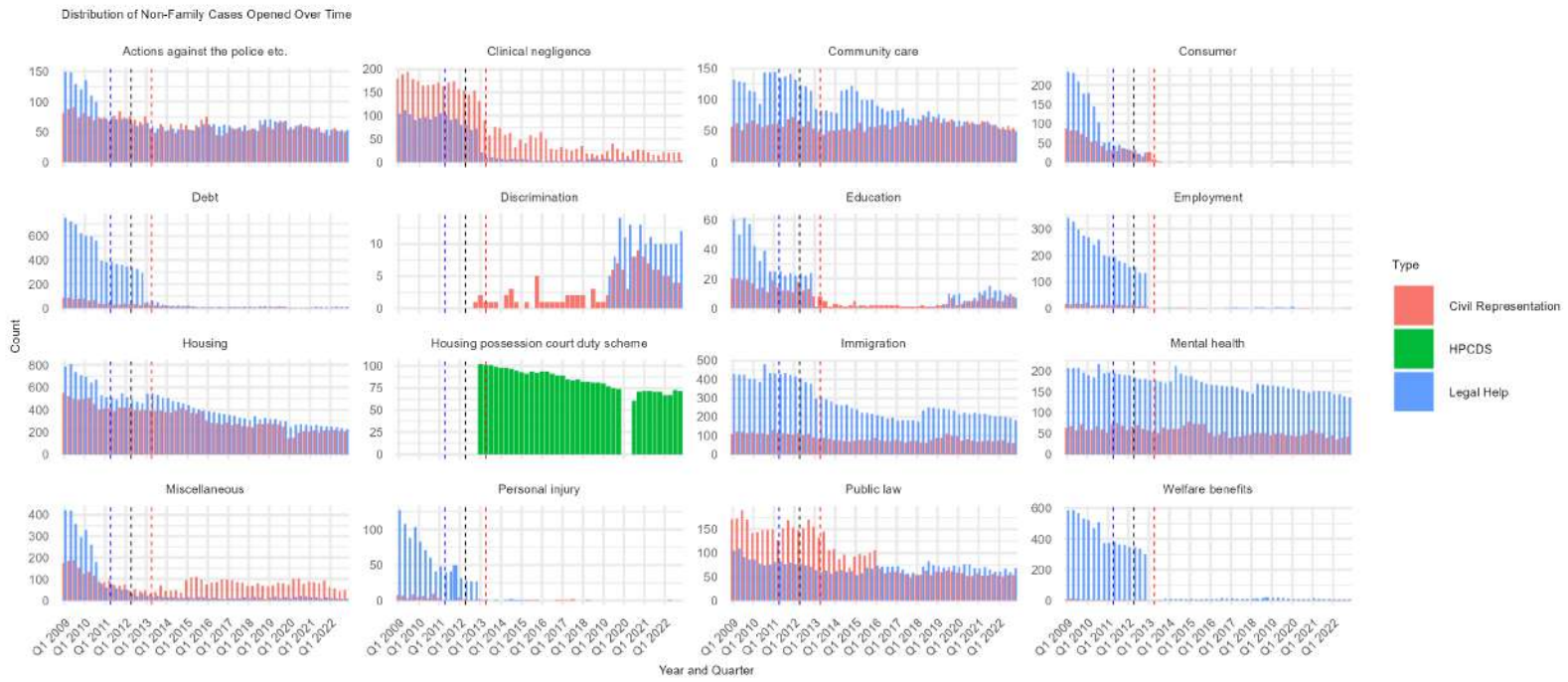


FIGURE 71: Trends in Legal Aid Cases Opened Each Quarter in Non-Family Civil Law, by Area of Law and Type of Assistance

Source: Ministry of Justice Legal Aid Statistics, author's calculations.

Note: These graphs show the number of legal aid cases opened each quarter by all providers in England and Wales. The data is broken down by area of law and type of assistance: legal help, civil representation, or the Housing Possession Court Duty Scheme (HPCDS) — a scheme available only in possession proceedings, where a lawyer present in the courtroom can provide immediate advice and representation, regardless of legal aid eligibility.

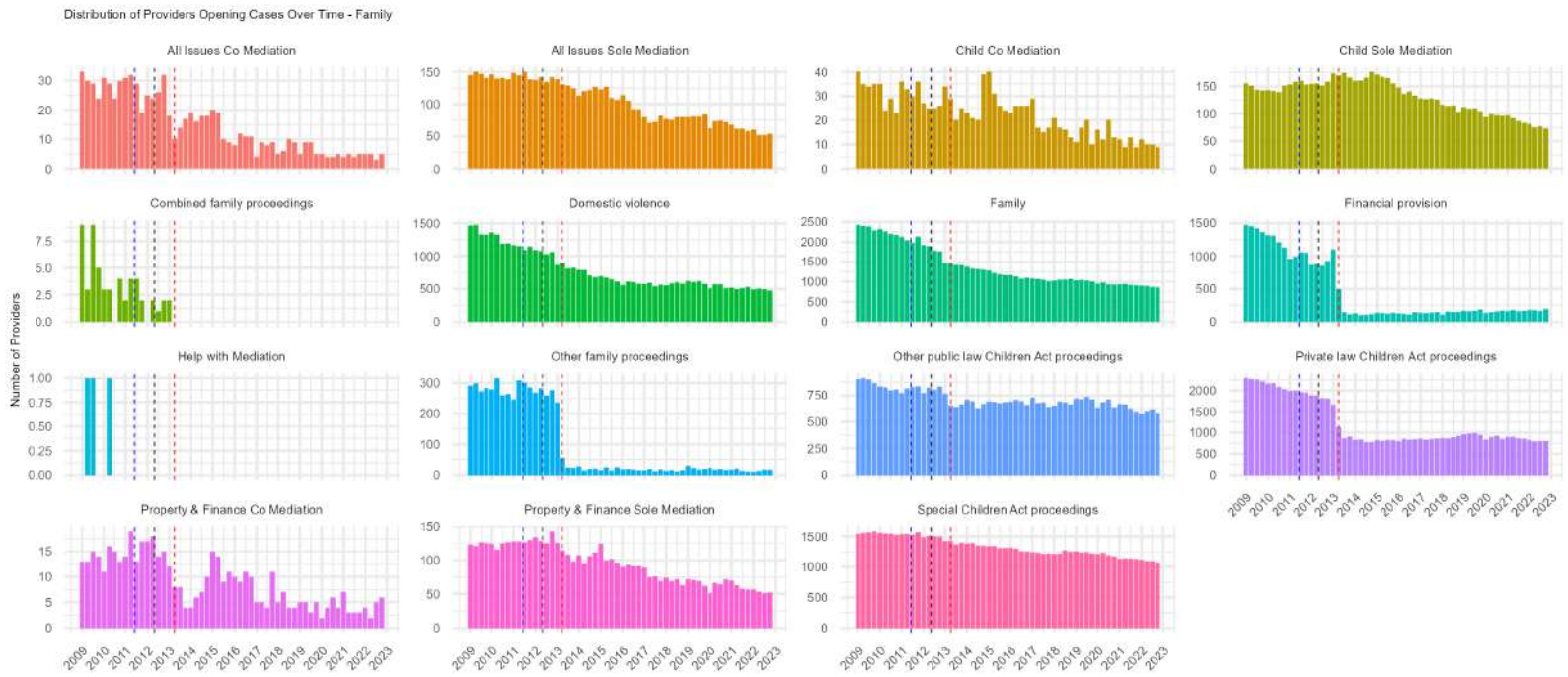


FIGURE 72: Number of Legal Aid Providers Opening Cases Each Quarter in Family Law, by Area of Law

Source: Ministry of Justice Legal Aid Statistics, author's calculations.

Note: These graphs show the number of providers opening new legal aid cases in England and Wales, broken down by area of law. The classification as "Family Law" follows the classification listed in the **??** pages 48-49.

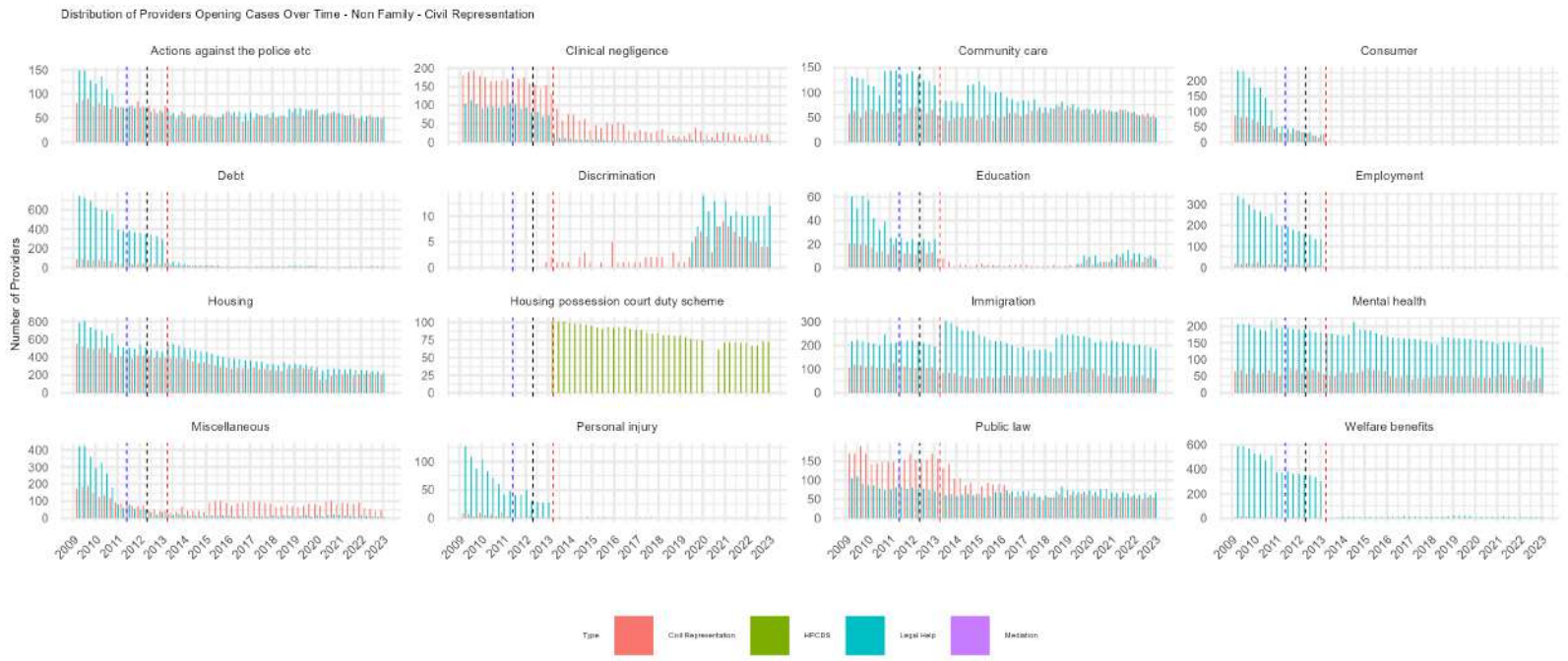


FIGURE 73: Number of Legal Aid Providers Opening Cases Each Quarter in Non-Family Civil Law, by Area of Law and Type of Assistance

Source: Ministry of Justice Legal Aid Statistics, author's calculations.

Note: These graphs show the number of providers opening new legal aid cases in England and Wales. Data is broken down by area of law and type of assistance: legal help, civil representation, or the Housing Possession Court Duty Scheme (HPCDS) — a scheme available only in possession proceedings, where a lawyer present in the courtroom can provide immediate advice and representation, regardless of legal aid eligibility.

E.6.1 Evolution of Legal Aid expenditures



FIGURE 74: Evolution of Legal Aid Expenditure by Area of Law and Scheme Type, 2008-2022 (Part 1 of 4)

Notes: Bars show quarterly total net legal aid expenditure in millions of pounds, disaggregated by scheme type. Blue dashed line indicates Legal Aid Bill introduction to the Parliament (Q2 2011), black dashed line marks Royal Assent (Q2 2012), and red dashed line shows full implementation of LASPO (Q2 2013). Scheme types include Legal Help (early intervention without representation), Legal Representation (full court representation), Mediation and Civil Legal Advice (CLA – the telephone-based legal advice service introduced as part of the LASPO reforms). Y-axes vary by area to show relative changes within each legal domain. The dramatic reduction in Legal Help expenditure (light blue bars) after Q2 2013 reflects the removal of most social welfare areas from scope, while Legal Representation (red bars) shows more gradual declines. Family law aggregates 15 sub-categories including private and public Children Act proceedings, domestic violence, and family mediation services.

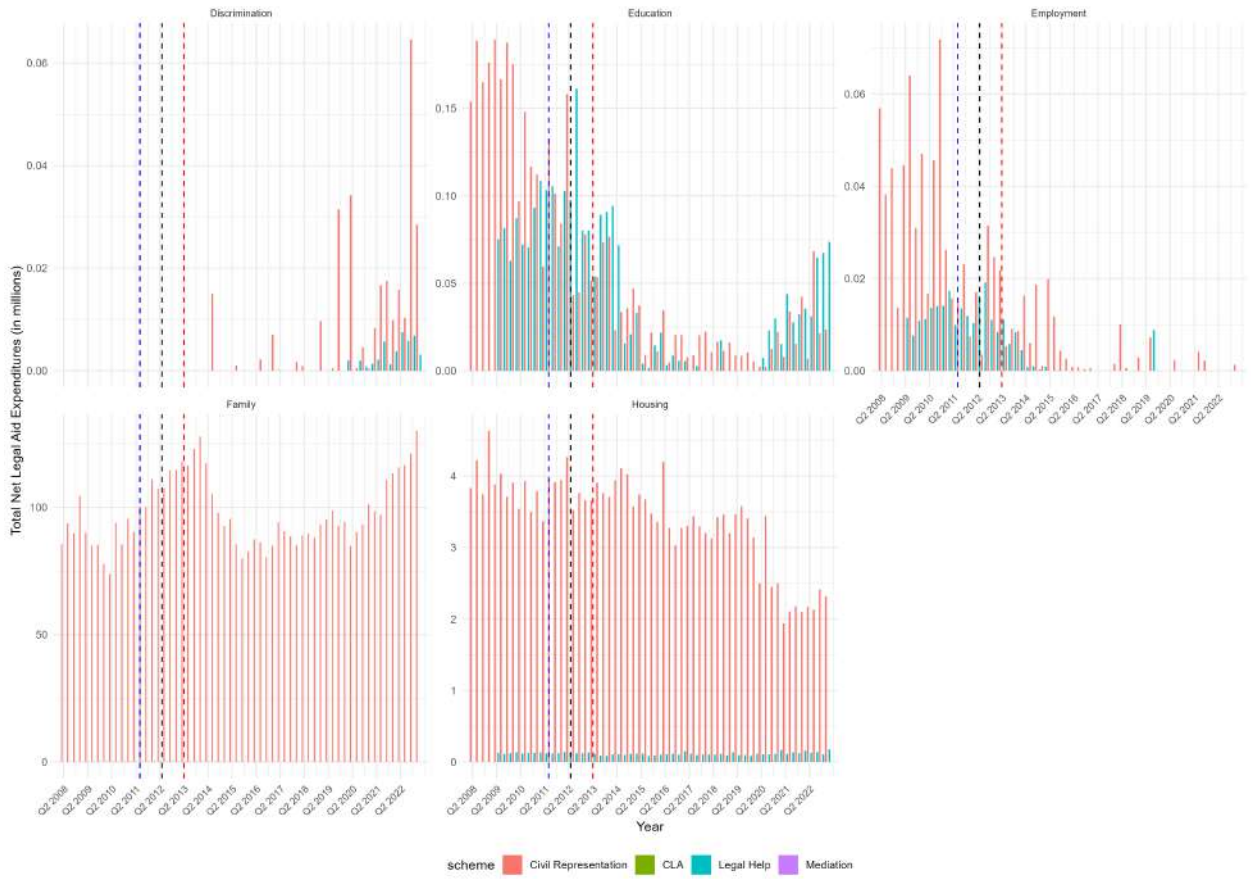


FIGURE 75: Evolution of Legal Aid Expenditure by Area of Law and Scheme Type, 2009-2018 (Part 2 of 4)



FIGURE 76: Evolution of Legal Aid Expenditure by Area of Law and Scheme Type, 2009-2018 (Part 3 of 4)



FIGURE 77: Evolution of Legal Aid Expenditure by Area of Law and Scheme Type, 2009-2018 (Part 4 of 4)

E.7 Alternative Definitions of Treatment

We explore the sensitivity of our treatment definition by changing the cluster of providers of interest. In our main analysis, we examine providers of social welfare law, meaning legal aid providers who cover either housing, debts, or benefits disputes (in isolation or in combination). They may hold legal aid contracts for any or all of these legal domains. In this section, we show the distribution of the treatment for alternative definitions, taking either a less conservative definition extending to providers covering any case of civil law—at the exclusion of family law proceedings, which tend to be a separate specialization**—or a more conservative definition restricting to providers covering housing disputes. They may bundle their provision with other areas of legal expertise.

E.7.1 Distance to the nearest Housing or Civil provider

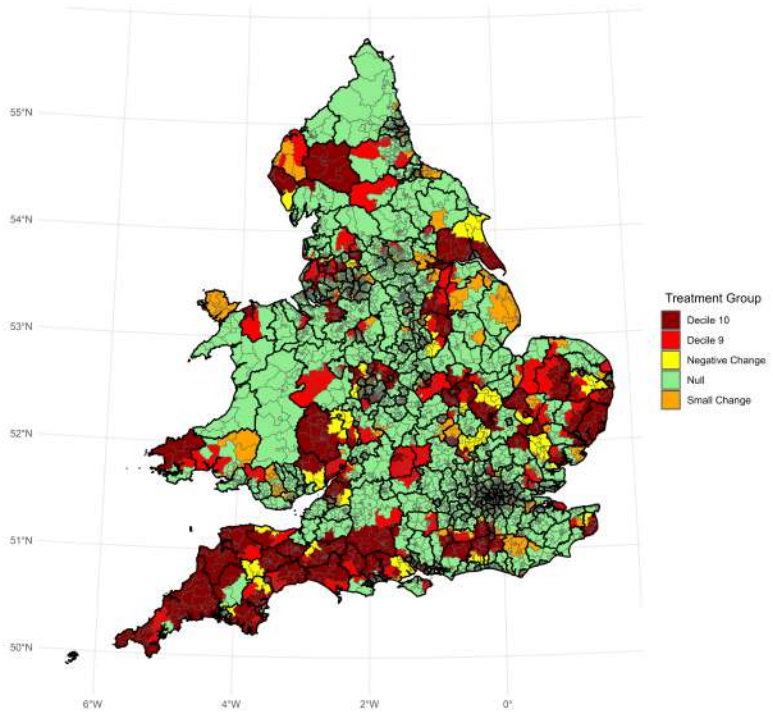


FIGURE 78: Distribution of the Treatment Group across MSOAs - Housing Providers - Static

TABLE 21: Summary statistics - Treatment Intensity Groups - Housing Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	7430.16	10978.92	20659.64	12905.56	28470.55	58138.30	726
Decile 9	1004.47	1830.49	3504.36	1887.89	5207.02	7423.62	726
Small Change	0.08	84.75	289.49	250.40	442.79	1003.47	920
Negative Change	-62762.41	-3971.80	-3408.82	5657.56	-215.03	-0.16	1284
Null Change	0.00	0.00	0.00	0.00	0.00	0.00	3608

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, geographic coordinates of MSOA population centroids were obtained from the ONS using the 2021 Census boundaries.
Note: The treatment variable represents the change in distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the nearest legal aid provider covering housing cases. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. The change in distance is measured between Q2 2012 and Q1 2014. Distance is measured in meters. A small change means that the change in distance is strictly positive but does not belong to the 5th quintile of the change-in-distance variable distribution.

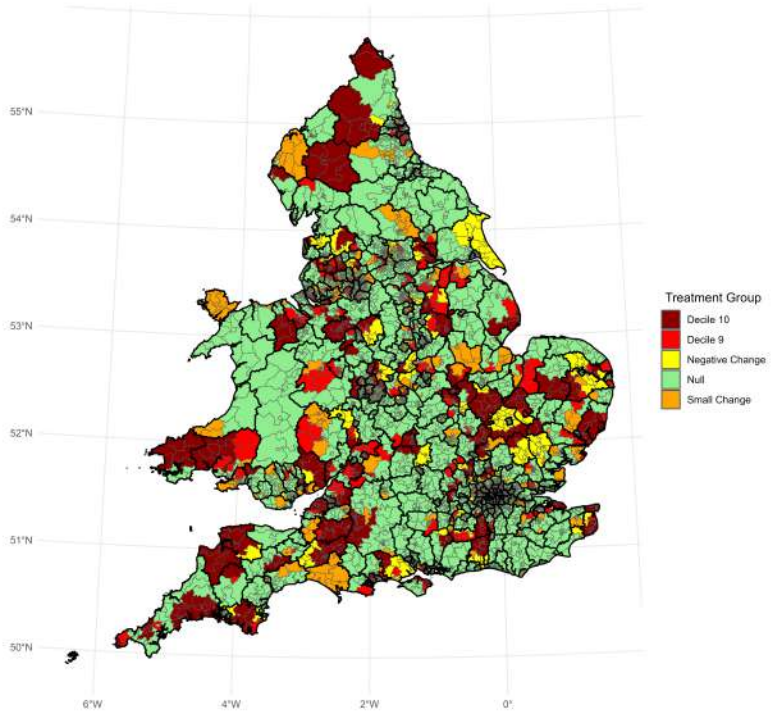


FIGURE 79: Distribution of the Treatment Group across MSOAs - Civil Providers - Static

TABLE 22: Summary statistics - Treatment Intensity Groups - Civil Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	4087.49	7362.09	16137.46	11879.23	21808.56	57572.77	726
Decile 9	473.29	773.17	1712.75	1014.25	2450.53	4075.41	726
Small Change	0.08	60.14	176.49	128.74	273.41	472.97	1024
Negative Change	-32421.24	-2986.81	-2713.56	4847.96	-162.29	-0.06	824
Null Change	0.00	0.00	0.00	0.00	0.00	0.00	3964

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics, geographic coordinates of MSOA population centroids were obtained from the ONS using the 2021 Census boundaries.
Note: The treatment variable represents the change in distance from the population-weighted centroid of each Middle Layer Super Output Area (MSOA) to the nearest legal aid provider cover cases in civil law, outside of family law proceedings. At each time point, if there is at least one active provider within the local authority, we calculate the distance to the nearest provider within that local authority. The change in distance is measured between Q2 2012 and Q1 2014. Distance is measured in meters. A small change means that the change in distance is strictly positive but does not belong to the 5th quintile of the change-in-distance variable distribution.

E.7.2 Dynamic Treatment Effects using Staggered Change in Distance

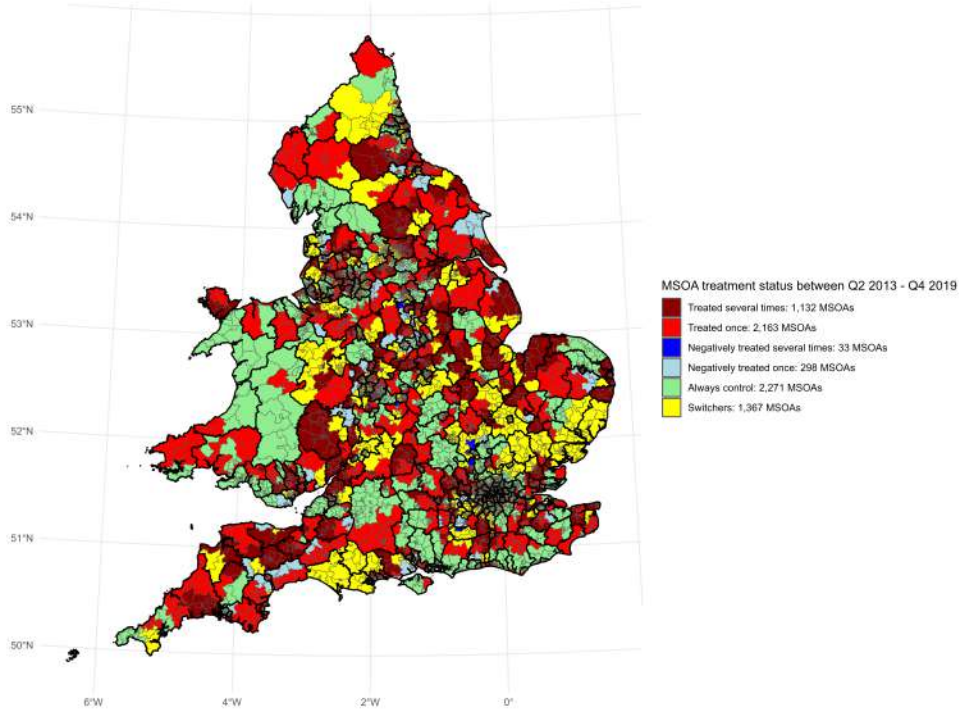


FIGURE 80: Dynamic Treatment Distribution Across England and Wales

Source: Provider operations data were compiled by the author based on the Ministry of Justice Legal Aid Statistics.
*Note:*A Middle Layer Super Output Area (MSOA) is classified as "Treated several times" or "Negatively treated several times" if it experiences several increases or decreases, respectively, between the distance to the nearest legal aid provider and its population-weighted centroid, between Q1 2013 and Q4 2019. "Treated once" or "Negatively treated once" areas only experience a single increase or decrease in distance. An area is classified as "Switchers" if it experiences at least one increase and one decrease in distance over this time period. "Always control" areas do not experience any change in the distance to their nearest provider, as measured in Q1 2013.