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

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Abstract

In 2013, England and Wales implemented a sweeping legal aid reform that drastically reduced public funding for civil legal assistance. The policy caused an 80% funding shock, triggering uneven provider closures and congestion. It eliminated early-stage support, and restricted legal 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 2011–2023 on legal aid provider locations, service volumes, court filings, healthcare use, and mortality, we 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, we test whether legal aid reductions amplified the impacts of concurrent welfare retrenchments. Preliminary findings suggest that the most affected areas experienced increases in eviction filings, court orders, and mortality. By limiting funding to narrowly defined legal areas, the reform curtailed lawyers' ability to address interconnected issues, such as benefits disputes and eviction. By crowding out generalist firms, it undermined the integrative function previously served by multi-area legal experts. Using a Marginal Value for Public Funds framework approach, this study underscores the role of access to justice in social protection. It shows how cutting preventive legal services can displace costs onto local authorities and healthcare systems, providing new evidence on the unintended long-term consequences of shrinking the social safety net through legal reform.

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

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1 Introduction

Legal aid programs aims to improve access to justice by subsidizing legal services for low-income individuals. While traditionally focused on criminal defence, there is growing recognition of the role civil legal aid plays in mitigating socioeconomic disadvantage, particularly in disputes over housing, debt, and welfare benefits. In the United States, several cities have recently begun funding legal representation for tenants facing eviction (Roumiantseva (2022)). The United Kingdom, by contrast, has a longer and more expansive history with civil legal aid. Introduced shortly after World War II, the UK's legal aid system initially supported veterans seeking divorce but quickly evolved into a comprehensive, means-tested program covering most areas of civil law. Until recent reforms, it provided low-income households with state-funded legal advice and representation to help them navigate the complexities of the everyday justice system.

Despite the centrality of legal aid programs to the rule of law, little is known about their broader impacts on welfare and public health, and even less about the consequences of restricting their scope and eligibility in times of austerity. Recent global surveys have revealed widespread unmet needs for legal assistance World Justice Project (2019), yet empirical evidence on the effects of reduced access remains limited. The inclusion of Access to Justice in the United Nations Sustainable Development Goals (Goal 16.3) underscores the need for evidence on the welfare implications of expanding or curtailing these programs (OECD (2019), World Bank (2019)). Emerging studies – primarily from the United States – have begun to document the benefits of expanding legal assistance in narrowly defined domains, such as eviction cases. This paper examines the inverse scenario: what happens when the provision of civil legal assistance is sharply curtailed across multiple areas of law. Focusing on the 2013 legal aid reform in England and Wales, this study provides new national-level evidence on the downstream consequences of restricted access. Specifically, it investigates how reduced legal support for social welfare disputes affected court outcomes, housing market pressures, healthcare services utilization, and mortality.

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 global financial crisis, the UK gov-

ernment implemented sweeping public spending cuts 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. It also restructured the system into a last-resort model by eliminating funding for early-stage legal advice - services that had previously helped resolve disputes before they escalated into formal court proceedings. As a result, legal aid, which had once offered state-funded advice and representation throughout the legal process, was drastically curtailed.

The reform led to a 70% decline in the number of supported cases and triggered an 80% funding shock to legal aid providers. Applied uniformly across regions, 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 setting to estimate the Marginal Value of Public Funds (MVPF) associated with civil legal aid.

In this paper, we adopt a dual empirical strategy to document the dynamic causal effects of this reduced access to legal assistance: (i) a generalised difference-in-difference using changes in distance to legal aid providers as a treatment variable. and (ii), a Bartik (shift-share) instrumental variable (IV) approach based on legal providers' caseload composition. We exploit the shock the reform induced in the distance to the nearest legal aid providers and changes in congestion due to provider closures. Constructing panel data on the activity of publicly-funded legal aid providers in England and Wales between 2011 and 2023, we track the interruption of providers' services over time, using the number of cases they open every quarter. Our analysis focuses on three sets of outcomes aggregated at a small area level: (i) eviction and debt court case prevalence and outcomes, (ii) housing market tension indices including housing affordability and risks of homelessness, and (iii), healthcare services use and health outcomes in terms of hospital beds occupancy,

uses of emergency services and age-adjusted mortality rate.

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. To account for treatment effect heterogeneity across groups, we follow Callaway and Sant'Anna (2021) and retrieve doubly robust estimators.

Next, we 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 address treatment heterogeneity over time, we apply the imputation-based estimators of Borusyak et al. (2021).

To address concerns about endogeneity in distance changes and spatial autocorrelation, we turn to a Bartik-style instrument to predict change in access to legal assistance. Following **?**, 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 and value, we assess how the reform differentially affected providers' ability to continue delivering legal services based on their characteristics (eg. not-for-profit vs. private firms) and the types of cases they handled pre-reform, categorized by area of law (eg. housing vs. debt) and type of assistance (eg. early vs. late-stage). We construct the instrument using providers' baseline caseload portfolios along with Ministry of Justice projections on the expected reform impact by case type. Lastly, leveraging local variation in exposure to broader austerity measures—including welfare cuts and recession intensity—we test whether reductions

in legal aid amplified the socioeconomic impact of concurrent shocks, shedding light on the overlooked fiscal multiplier role of legal aid programs.

Our preliminary results 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 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 13.9 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.

When examining the pooled effect of a *continuous* definition of the change in distance, we find that eviction filing rates increase by 0.35 per 1 km increase in distance to the nearest legal aid provider, eviction orders increase by 0.18, and age-adjusted mortality increases by 2.22 per 100,000 inhabitants. Adopting a more dynamic perspective —where we consider changes in distance at any point in time, regardless of the intensity of the changes— 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 by the reform early on may have taken longer to adjust to the new landscape. Over time, one would expect actors to adjust more quickly

to changes in provisions, which raises reservations about grouping areas that experienced a change in distance at any point in time. We are currently retrieving cohort-specific treatment effects to test this hypothesis.

In this paper, we explore the impacts of removing a program that targeted households vulnerable to homelessness and over-indebtedness, exploiting the change in local access to legal assistance triggered by the reform. 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.

In fact, 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 (Commission (2014)). Last, the reform did not introduce any perfect substitutes for in-person services. Despite efforts to transition to digital and telephone provision of legal assistance, the reform created online tools that have been documented to fail in adequately addressing the legal needs of the most vulnerable populations (Balmer et al. (2012), Smith et al. (2013), Burton (2018)). However, 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), Network (2018)).

This project addresses the limited evidence on the welfare cost and benefits of legal assistance programs from a value-for-money perspective (Abel and Vignola (2010), Stout

Risius Ross, Inc. (2016)). It adds to welfare analyses conducted in the US context by Abramson (2022) and van Dijk et al. (2024), which focus on frictional costs on the housing market, while extending the analysis to homelessness and health outcomes. Moreover, it contributes to a recent literature that evaluates the effects of legal counsel in eviction proceedings on court outcomes. Primarily centered on the U.S., this literature has documented the positive impacts of introducing intensive forms of legal assistance, such as legal representation, to avoid the issuance of eviction orders. Initial evidence generated through randomized controlled trials in NYC Housing Courts (Seron et al. (2001)) and in Massachusetts Housing Court (Greiner and Pattanayak (2011), Greiner et al. (2013)) showed that personalised legal assistance improved court outcomes compared to generic information provision. Further evaluations of the right-to-counsel initiative in NYC (Ellen et al. (2021), Cassidy and Curie (2022), van Dijk et al. (2024)) found that beneficiaries were less likely to be evicted and incurred lower monetary judgments when offered legal representation.

In contrast, this paper provides the first national-scale evidence outside the U.S. of a *negative* supply shock in legal assistance. It offers a comprehensive perspective on the effects of removing legal assistance across all social welfare cases simultaneously, rather than focusing solely on eviction proceedings, quantifying the multifaceted impacts of transforming legal aid into a last-resort scheme. By including effects on mortality, this paper also links with sociolegal and public health literatures that connect unmet legal needs, meaning the experience of not resolving a legal problem, to health status. In particular, studies documenting health-justice partnerships have evidenced the potential of accessing legal assistance for advancing physical and mental health outcomes of vulnerable households facing accumulating legal problems (for a review see: Tobin-Tyler et al. (2011)). International comparisons looking at the UK, US and Australia (Tobin-Tyler et al. (2023)) have stressed the importance of collaborations between healthcare providers and legal advisors to reach health equity. 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. Lastly, this project builds

on emerging evidence indicating that legal assistance is more effective when integrated with other services (Caspi and Rafkin (2024), **?**) and can impact welfare participation over the long-run Cunningham and Goodman-Bacon (2025). Joint models of legal provision and benefits support are particularly common for welfare cases (Newton et al. (2020)). It connect with studies documenting the impacts of rental assistance on self-rated health (Keene et al. (2020)), mental health (Denary et al. (2021)), nutrition (Denary et al. (2023)) and Hemoglobin levels (Fenelon et al. (2022)). By evaluating legal aid as a welfare policy, it provides a legal assistance perspective on the dynamic relationship between eviction and poverty, a connection extensively documented by Desmond (2016) and recently quantified by Collinson et al. (2024) in the US context. It quantifies the role that legal assistance can play in amplifying austerity effects demonstrated in voting behaviours (Fetzer (2019)), homelessness (Fetzer et al. (2023), and mortality (Berman and Hovland (2024)).

This paper explores two mechanisms. First, legal aid professionals often serve as navigators of the welfare state. They identify client needs critical to both legal resolution and welfare — such as unclaimed benefits or untreated mental health conditions. 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, 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 services.

Second, the reform distorted the legal aid provider market by crowding out generalist firms who worked across multiple legal domains. While the policy aimed to consolidate the sector, it hollowed out the legal aid landscape, reducing geographic access, increasing congestion, and weakening the supply of in-person legal help. The remaining specialists were often ill-equipped to assist clients with complex, multi-faceted problems - such as eviction intertwined with domestic violence or immigration issues. These structural shifts undermined the integrative function that generalist lawyers previously served, including referrals across the legal and social support system.

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 preliminary 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 solicitors and barristers 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, they were not yet facing a court hearing and did not require representation by a lawyer. After the reform, the same household would have had to wait until the formal eviction process commenced before qualifying for legal aid. Similarly, while an eviction case may originate from a benefit entitlement issue, the household would no longer be able to access benefit advice that fell outside 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.

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, 2019). 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.

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 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 [1].

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

[1]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 perspective by its scope and magnitude.

for services over time (for a case study see: Organ and Sigafoos (2017); for a review see: of Commons Justice Committee (2015), Society (2017), Futures and team (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[2]. 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 5 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

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

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.

3 Data and Descriptive Evidence

Tables 7 and 8 report the summary statistics for all covariates in the baseline period, 2013, and at the end of the period respectively.

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. Additionally, it omits organisations that stopped providing legal aid after the implementation of LASPO but remained active in other areas of legal assistance for which legal aid funding was no longer available.

At the time of writing, no comprehensive time series exist for the activities of legal advice providers operating outside the legal aid system. Existing literature (Society (2017); Wilding (2021)) relies exclusively on the Ministry of Justice dataset and is therefore limited to providers within the legal aid scheme. To address this gap, we are currently constructing

a new database to track the locations and operational periods of all organisations delivering legal support 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 solicitors. This contrasts with more generic, lower-intensity forms of advice typically offered by volunteers or paralegals. To further mitigate this concern, we cluster the analysis 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 housing may also cover other areas of law, but must 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 2, the treatment variables defined in Section 4 are especially sensitive to how broadly legal aid provision is defined. We further explore this issue 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 that they 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 will implement the *georoute* module developed by Weber and Peclat (2016), which estimates road distance and travel time between two addresses. We also plan to test the sensitivity of our results using alternative distance measures that explicitly account for spatial (in)accessibility, following Page et al. (2018). This will allow us to better capture provider clustering in urban centres and disparities in access between rural and metropolitan areas.

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 is conditional on the legal specialisation of providers. 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 (see Figure 15). While a stock analysis reveals a general decline in the number of active providers (Figure 3) 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 (Figure 7), encouraged in part by the competitive tendering process for legal aid contracts.

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 (see Figures 17 and 18). 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 19). Meanwhile, the largest providers reduced their footprint: while some firms operated more than 30 offices

in 2009–10, none did so by 2013–14 (Figure 20).

These structural shifts motivate the construction of a second measure of access to legal aid at the MSOA level. While still accounting for geographic distance, this measure introduces variation by area of law and adjusts for spatial clustering of providers—particularly in metropolitan areas. Instead of considering only the distance to the nearest provider, it incorporates all providers p active in a given legal domain a at baseline (defined as the 2009–10 financial year), calculating the distance from each MSOA's population-weighted centroid to each provider.

To reflect provider capacity, the measure is weighted by the number of legal aid cases handled in the relevant area of law $Volume_{a,p}$. It also accounts for each provider's degree of specialisation —measured as the share of their total legal aid workload devoted to that area of law $Specialisation_{a,p}$. In an alternative specification (not yet implemented at the time of writing), specialisation could be proxied by the share of legal aid income derived from that area relative to the provider's total legal aid revenue.

Drawing on a gravity model framework, the resulting provider-level exposure is then interacted with the size of the low-income population in each MSOA, $Population_{MSOA}$, — which serves as a proxy for legal need or "market size." The resulting composite metric estimates the baseline flow of legal aid services into each MSOA, or more precisely, provides a relative measure of access to legal aid for a given legal issue in area a. While not interpretable in absolute terms, the metrics allows for consistent comparison and ranking of MSOAs by their level of access.

$$Access_{MSOA,a} = \sum_{p} \frac{1}{Distance_{MSOA,p}} \times Volume_{a,p} \times Specialisation_{a,p} \times Population_{MSOA}$$

In our instrumental variable strategy, we use the access weights described above to construct a Bartik-style instrument that varies at the MSOA level. 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 legal domains and provider types (of Justice of the United Kingdom (2011)). Using these domain-specific predictions,

along with provider-level data on case volumes and revenues, we compute a predicted shock for each provider —capturing the anticipated impact of the reform given their pre-reform distribution of legal aid work.

These predicted provider-level shocks are then aggregated to the MSOA level by weighting each provider's predicted shock by their relative contribution to the provision of legal aid in that area of law. The resulting MSOA-level instrument captures the expected change in access due to the reform, based on both the geographic distribution and legal-area exposure of each community. Table E.1.2 in the Appendix reports the estimated percentage impact by area of law, as outlined in the EIA.

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.

We take the population-weighted number of court cases per 100,000 inhabitants as well as the number of court outcomes that we define as favourable for each of the following variables. When defining an outcome as favourable, we implicitly assume that the beneficiary of legal aid would be either the tenant or the indebted in each successive situation.

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 54 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. 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 3 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. [3]

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: [4] 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

[5] **Attendance and Emergency weekly admissions:** 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

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

[3] Forthcoming
[4] Forthcoming
[5] Forthcoming
[6] Forthcoming

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. [7]

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.

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 two 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

[7] This project aims to next include rental transactions data from property search website.

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. Figures 6 and 7 show that the first quintile is the most significantly affected by the reform. In that group, the change in distance is an average of 14.3 km (cf. Tables 11 and 10). 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 12 shows the distribution of the change in distance variable among our favourite specification of treatment intensity. Table 13 and 14 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 [8]. 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 geographical entity with roughly the same average number of residents for which outcome data is available. Table 9 shows the distribution of population across MSOAs. The homogeneity of MSOAs

[8] Forthcoming

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 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 subsection 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.[9] Figure 8 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.

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 (Figure **??**). However, we exclude them as soon as they switch off to not rely on additional assumptions [10]. Figure 9 shows the size of the sample after these

[9]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.

[10]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 $t-1$.

modifications. Last, to further simplify the analysis, we exclude from the sample "Treated several times" units after they experience a further increase in distance.[11] Figure 10 and Figure 11 show the final composition of my sample.

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.

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

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

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

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

Estimating Equation **??** 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

[11] 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.

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

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 how much of 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.

By isolating the impact of the reform through differential supply-side provider exposure, the Bartik IV strategy further ensures that observed changes in outcomes are not driven by 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 is computed for each MSOA and is inspired by a gravity framework to model legal aid flows. It captures each MSOA's degree of exposure to changes in legal aid access by reflecting both providers resilience and their spatial distribution.

Specifically, we first construct a provider-level measure of baseline capacity in each area of law a, defined as the product of the number of legal aid cases handled in that area ($\text{Volume}_{a,p}$) and the provider's degree of specialisation in that area ($\text{Specialisation}_{a,p}$).

This 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 low-income population in the MSOA (a proxy for the demand for legal need). Aggregating over providers yields an MSOA-level measure of access to legal aid in area a at baseline:

$$W_{i,a} = \sum_{p} \frac{1}{Distance_{i,p}^{\theta}} \cdot Volume_{a,p} \cdot Specialisation_{a,p} \cdot Population_i$$

This gravity-based access measure reflects how strongly MSOA i was connected to legal aid provision in area a prior to the reform, as a function of provider capacity, distance decay, and local market size. To construct the instrument, we interact these MSOA-level access weights with the predicted shocks to each area of law to obtain an overall exposure index:

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

This Bartik-style instrument varies across MSOAs based on their pre-reform exposure to legal aid provision in more vs. less affected domains, 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 domains shocks.

To assess the relevance of the instrument, we first evaluate the instrument's explanatory strength for post-reform legal aid access by regressing it on MSOA-level access weights.

These weights are constructed using the same definition as in the instrument but are calculated using observed provider activity and spatial distribution from the 2013–14 financial year—after the implementation of the reform. The resulting R^2 values, presented graphically, provide evidence of instrument relevance (see Figure 21). We also examine the instrument's predictive power for one of the most extreme provider-level outcomes: exit from the legal aid market. Regressing provider exit on the instrument yields statistically significant coefficients in key areas affected by the reform, including housing, debt, and welfare benefits (see Figure 22).

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 E.1.1). 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 15), particularly those specialising in family, housing, debt, or welfare benefits law (see Figure 16).

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. Please note that 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 6.4, 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.[12]

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

[12]Forthcoming

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.[13] 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.[14]

Local level of needs for legal assistance

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 Figure 12. 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. In Table 1, we report the results of pair-wise t-tests comparing the means of covariates between treated and untreated areas at baseline, when taking a static

[13] 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

[14] To estimate the robustness of our results, we will closely follow Rambachan and Roth (2022) 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.

definition. Results suggest that areas differ on most dimensions. However, this is only a problem if differing area characteristics affect the evolution of local outcomes *to a different extent* over time. Moreover, it is worth noting that the 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. [15]

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 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 13. This supports the claim that the reform changed the geography of the provision of assistance, to a large extent irrespective of local needs. Figure 14 additionally shows that needs, measured based on this definition, have relatively evolved at a same pace across the country, except in few localised areas. As a robustness checks, we are testing the sensitivity of our estimates when excluding areas which experienced the greatest change in needs.[16]

[15] To investigate this further, we are testing the extent to which the list of covariates included in Table 7 predict changes in distance in 2013.

[16] As robustness checks, we are testing the sensitivity of our estimates when controlling for a set of indicators. To mitigate concerns of "bad control" problems, meaning when covariates may themselves be affected by the treatment and have a time-varying effect, we take pre-reform values for all covariates. To proxy the share of vulnerable populations unlikely to afford legal services at the market rate, we take the share of households on fuel poverty from the Sub-regional fuel poverty data and the share of benefits recipients available on the DWP Benefits statistics (Statsexplore). Alternatively, we take the share of households on low income as a proxy for legal aid eligibility. In addition, we control for the initial level of provision, measured as the number of providers within the local authority in the year before the reform.

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 13.9 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 28, 29, 30, 31, and 50 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 34, 35, and 36 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 12.

Figures 37, 38, 39, and 40 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 17, 18, 19, 20 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 7. Tables 21, 22, 23 and 24 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 45, 46, 49 and 50 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: change in composite measure of access - Forthcoming

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 47 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.

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 lacking 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 (**?**). 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.

Preliminary findings suggest that mortality effects are concentrated among the elderly, although this is still under investigation (32, 40). 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 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 conducting 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.

6.4 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 16). Furthermore, the relationship between the change in distance and austerity exposure appears to be weakly negatively correlated (Figure 27). 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 15, Figures **??**, **??** and, **??**,). Nonetheless, to address additional concerns, we are currently constructing indices of exposure to the austerity and to the financial crisis at the MSOA level.

6.5 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. To this end, 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 approaches.

These quasi-experimental estimates enable us to calculate the excess numbers of court cases, deaths and emergency services usage attributable to the reform. To translate these effects into monetary values, we will: (1) estimate the additional costs borne by the pub-

lic 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 from increased 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 guidance on structuring legal aid funding to maximize public welfare, informing policymakers on the welfare trade-offs of legal aid cuts.

7 Conclusion

Access to justice is a cornerstone of the rule of law, yet its practical implications remain insufficiently understood. Interventions aimed at helping individuals understand, navigate, and utilize the legal system to protect their rights have rarely been evaluated outside the courtrooms. This study underscores the importance of adopting an empirical approach to assess the functioning and broader impacts of legal assistance programs.

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 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.

Future research could track more closely the health outcomes of households with unmet legal needs, which might offer valuable insights into the mechanisms at play. 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.

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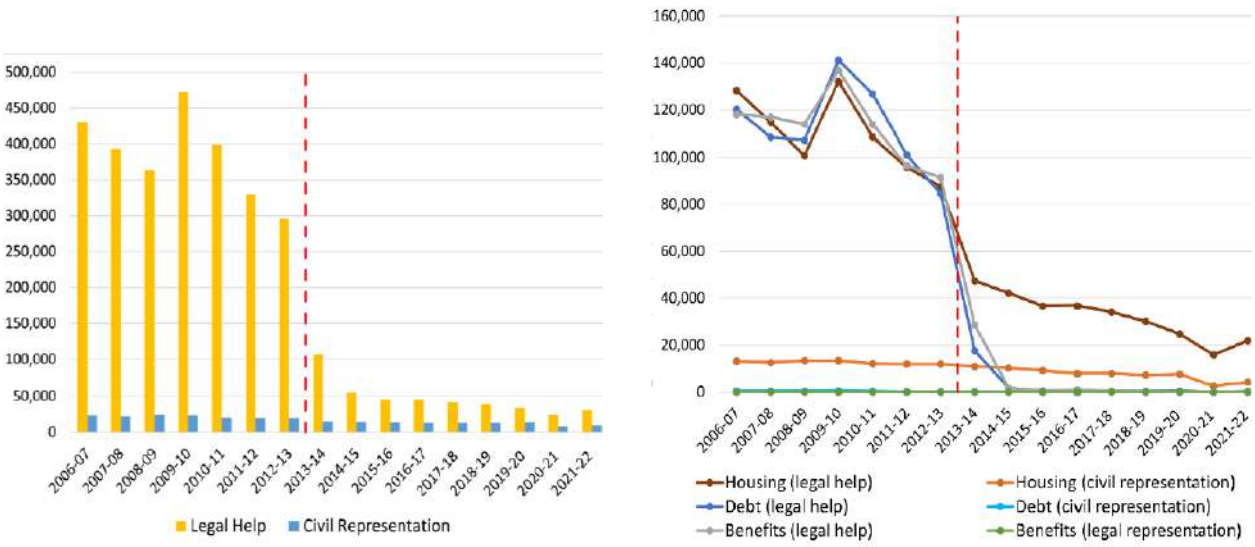
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A Figures

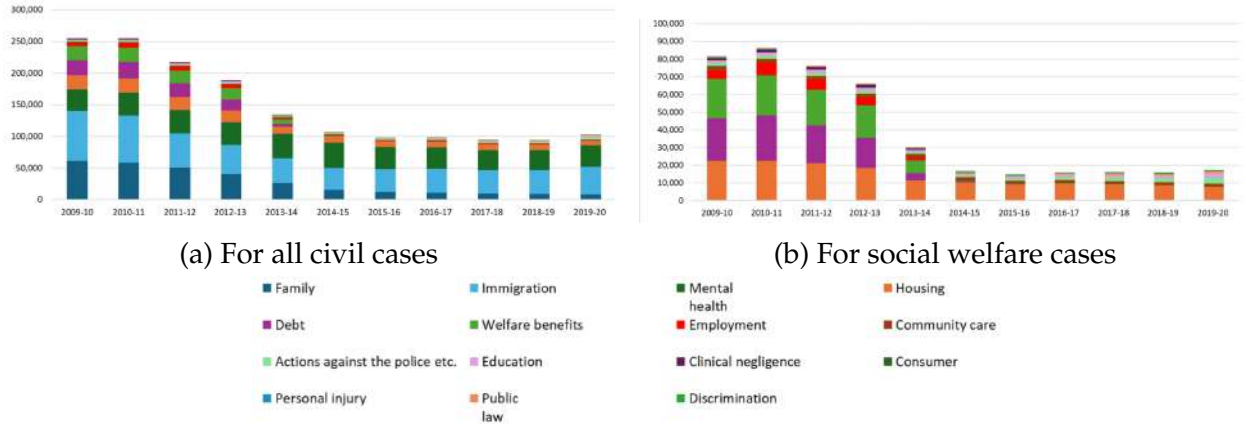


(a) By type of legal aid work

(b) By areas of law

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

Source: Ministry of Justice Legal Aid Statistics
Note: These graphs depict the evolution of the number of legal aid cases completed in England and Wales between 2006 and 2022. A case is considered completed when there are no further interactions between a client and their legal aid lawyer, and the lawyer has claimed their fees from the government. 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 lines representing the number of legal representation cases for debt and benefits cases overlap. The dashed red line indicates when the legal aid reform came into effect in April 2013.



(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, own computation.
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.

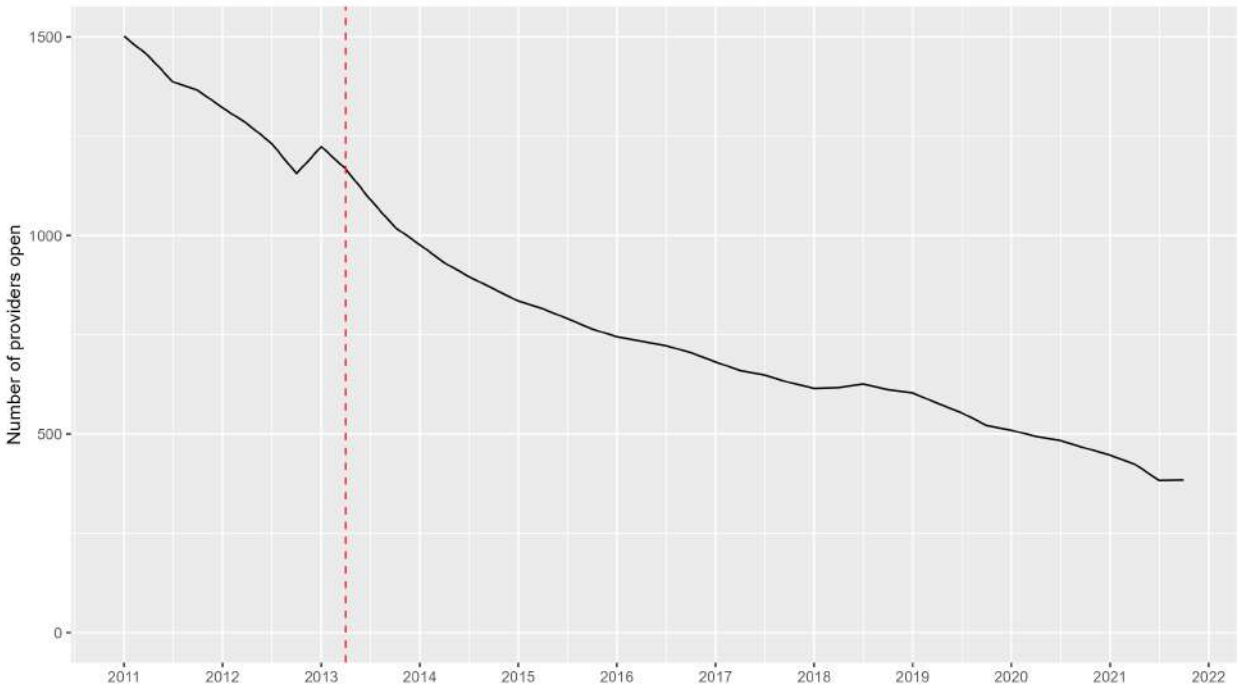


FIGURE 3: Number of active legal aid providers over time

Source: Ministry of Justice Legal Aid Statistics, own computation.
Note: We define an active legal aid provider as a provider that completes or takes on new cases. In this paper, a provider is considered to cease being active at time t if they do not complete or 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.

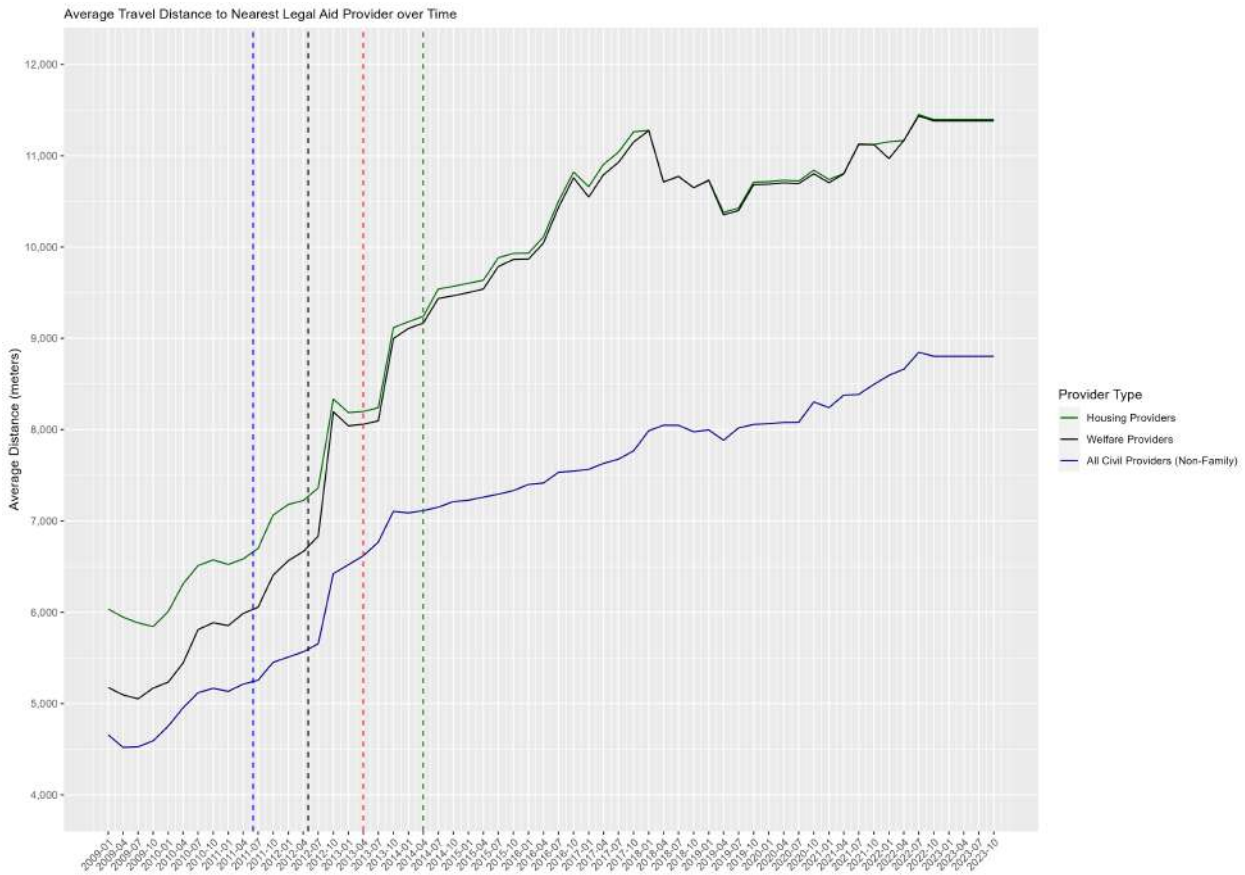


FIGURE 4: Average Distance to Nearest Legal Aid Provider over Time

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: The dashed lines illustrate the timeline of the implementation of the LASPO Act, from its introduction to Parliament—the date on which its content was released to the general public—through to its full enforcement. The blue dashed line represents the date the Bill was introduced in the House of Commons (June 21, 2011). The **black dashed line** represents the date the Act received Royal Assent (May 1, 2012). The red dashed line represents the date the Act came fully into force (April 1, 2013). The green dashed lines indicate the window chosen to measure the change in distance: from the Royal Assent date to one financial year post-implementation (Q2 2012 to Q1 2014). 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.

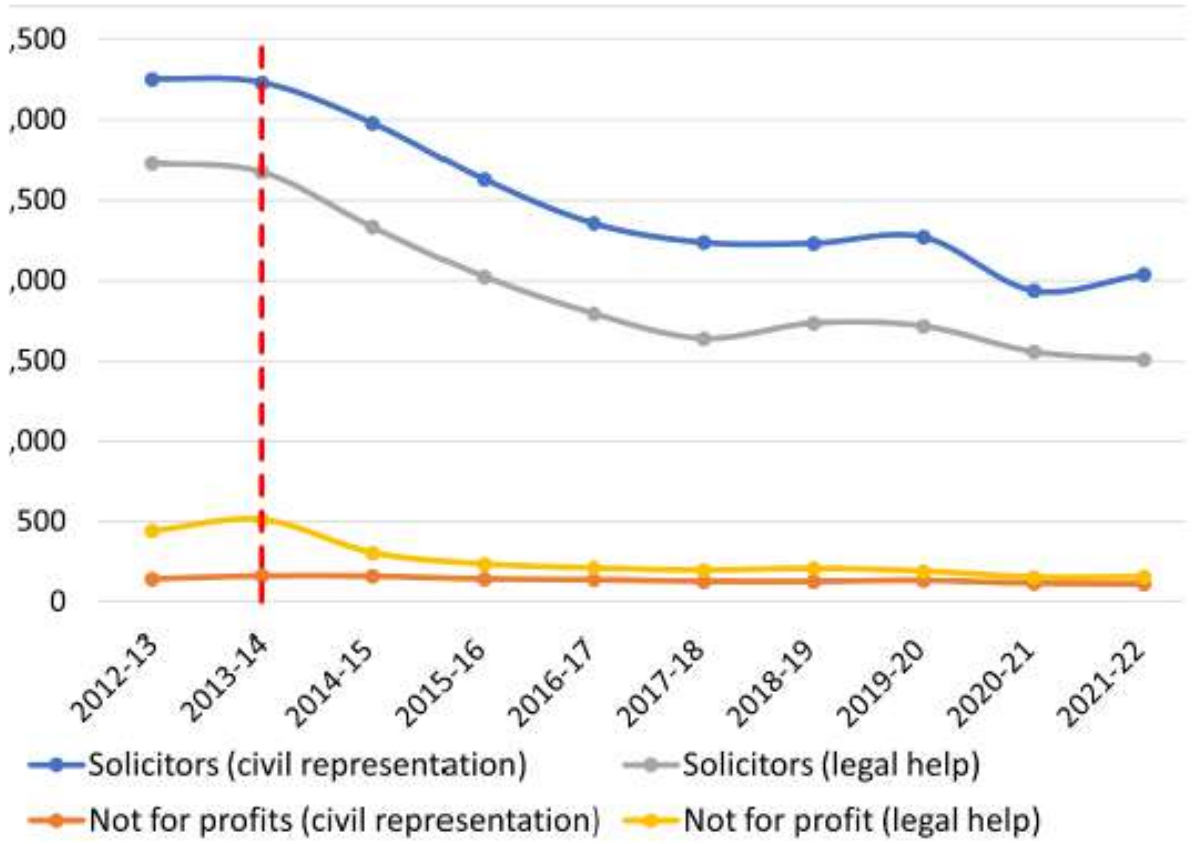


FIGURE 5: Number of active legal aid providers over time, by type of providers

Source: Ministry of Justice Legal Aid Statistics, own computation.
Note: We define an active legal aid provider as a provider that completes or takes on new cases. In this paper, a provider is considered to cease being active at time t if they do not complete or 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.

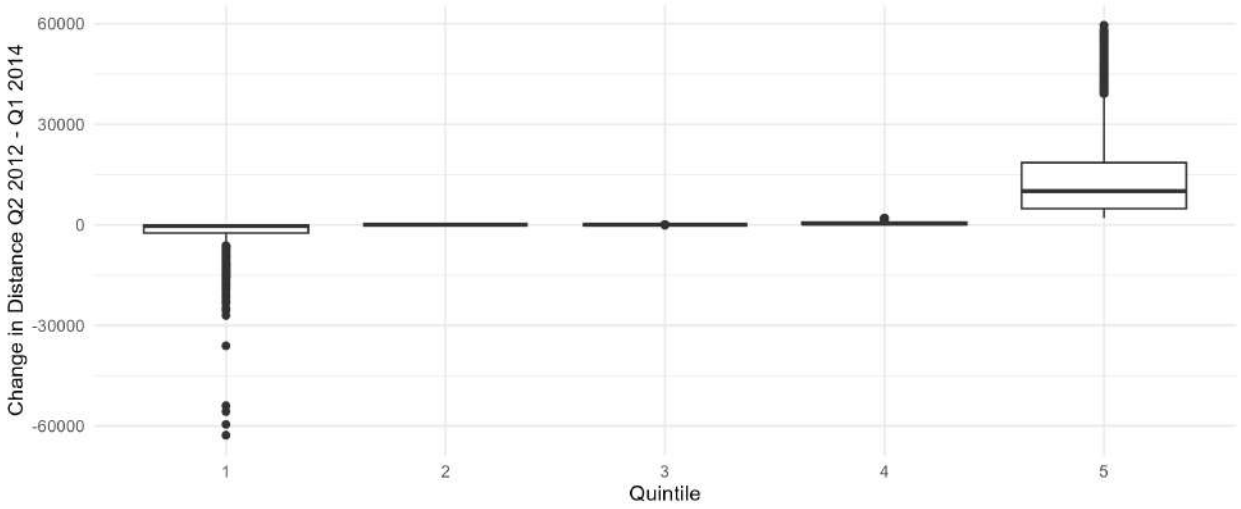


FIGURE 6: Distribution of the Treatment Variable across Quintiles

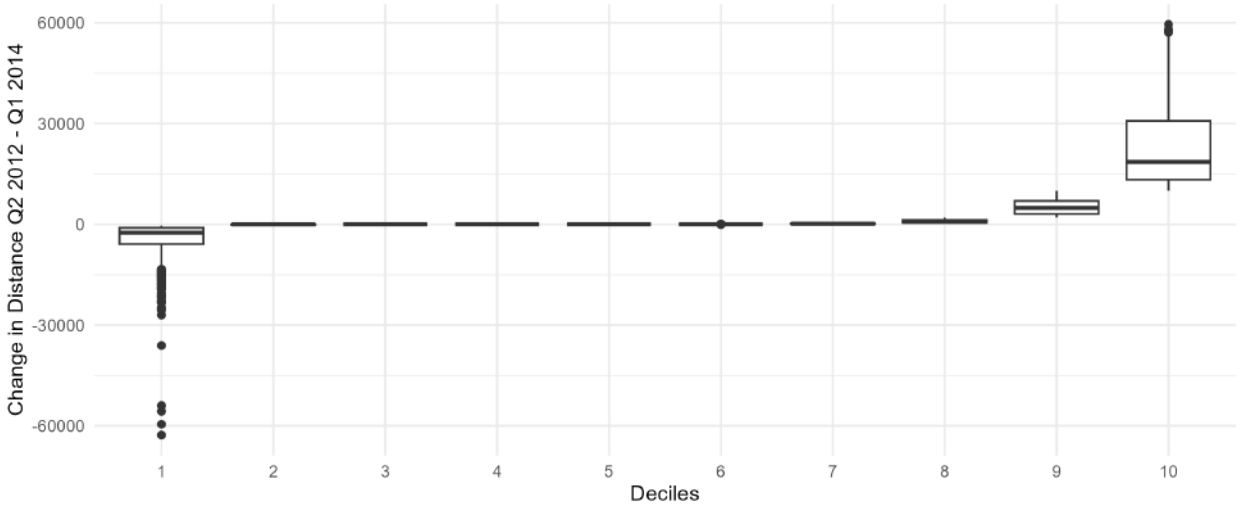


FIGURE 7: Distribution of the Treatment Variable across Deciles

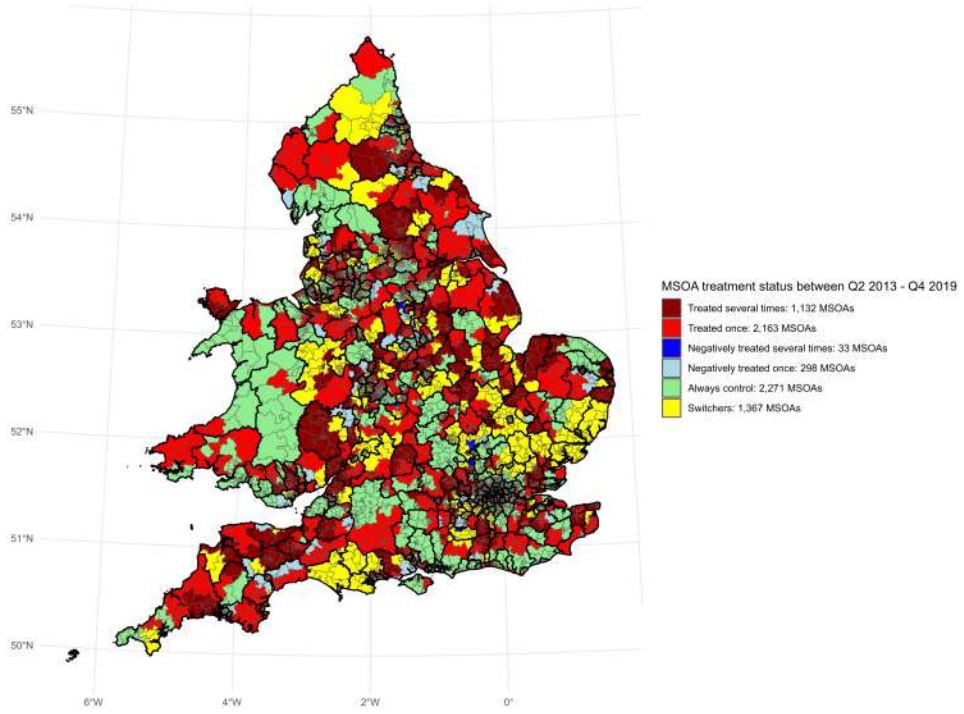


FIGURE 8: Dynamic Treatment Distribution Across England and Wales

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.

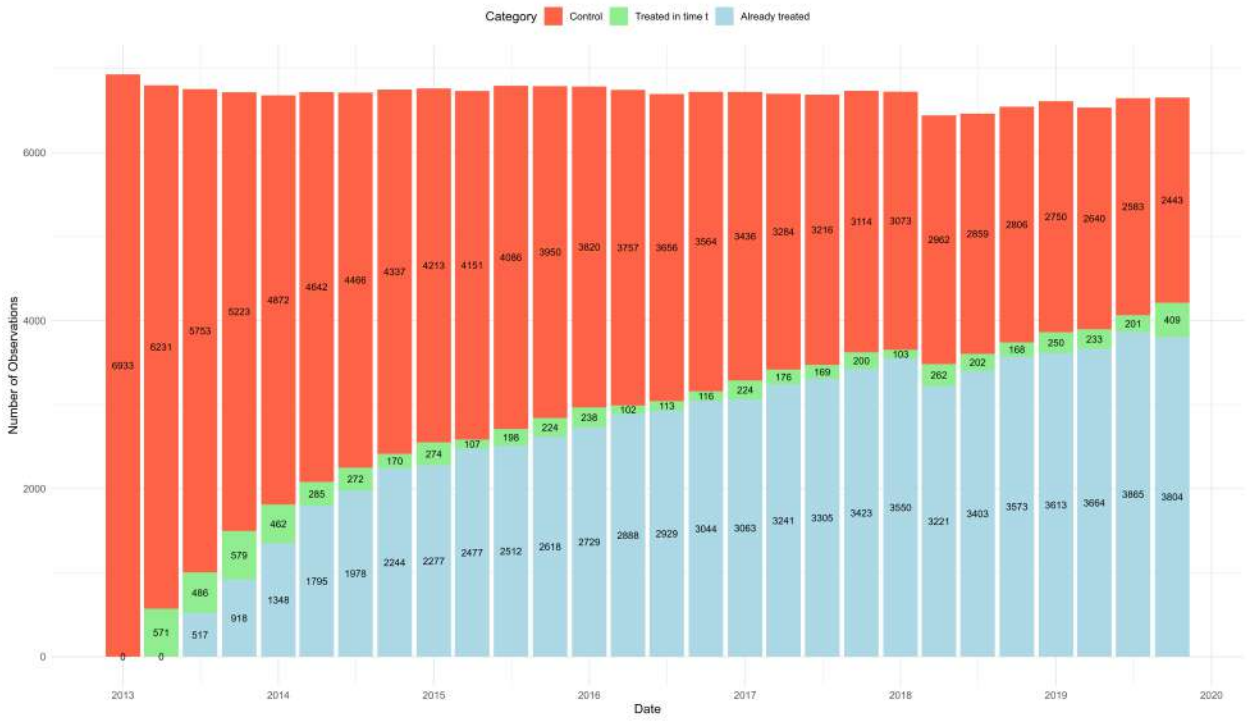


FIGURE 9: Number of Observations over Time

Note: The graph displays the distribution of treatment statuses across Middle Super Layer Output (MSOA) areas at each point in time between Q1 2013, the baseline period, and Q4 2019. The red bar represents the control units that do not experience a change in distance to their nearest legal aid provider. The green bar displays the number of MSOAs that experienced an increase in distance compared to the baseline distance in Q1 2013. The blue bar represents the stock of units that were treated in previous periods and are still treated at that time. Note that the total number of observations is not stable over time, as "Switchers" areas —meaning areas that experience a decrease in distance after having experienced an increase— are removed from the sample after the decrease occurs.

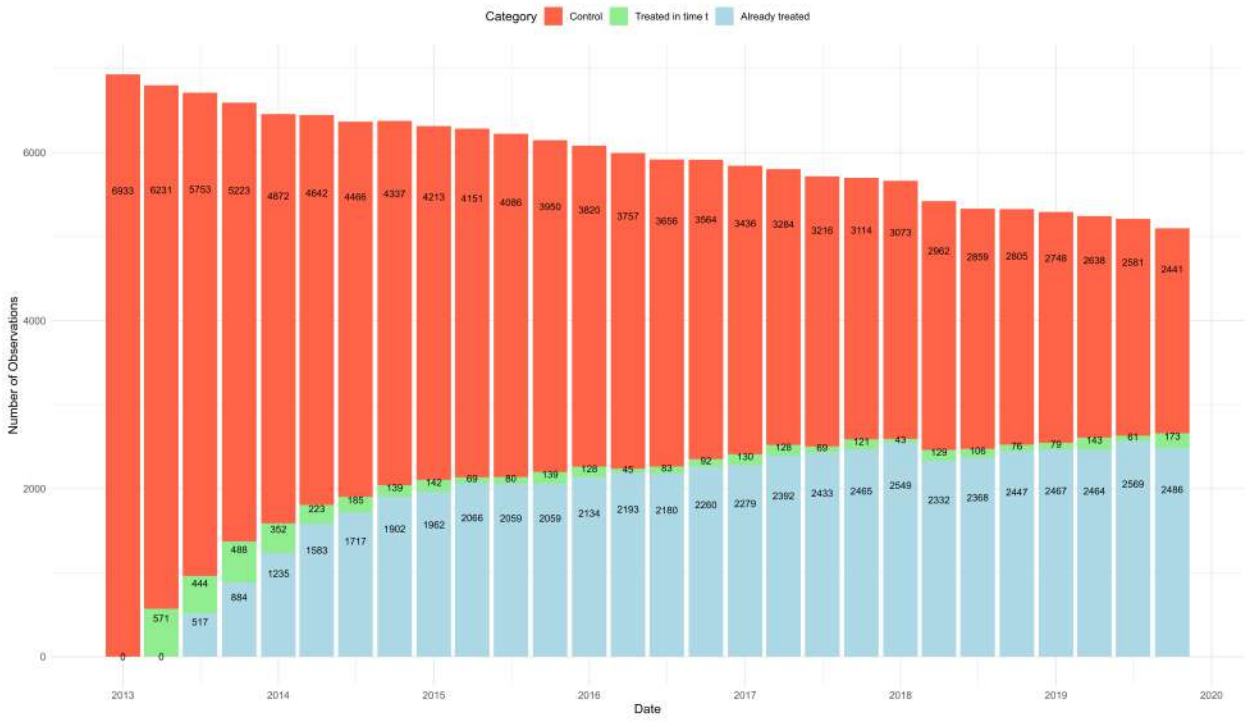


FIGURE 10: Number of Observations over Time - Excluding Higher than One Treatment Dose

Note: The graph displays the distribution of treatment statuses across Middle Super Layer Output (MSOA) areas at each point in time between Q1 2013, the baseline period, and Q4 2019. The red bar represents the control units that do not experience a change in distance to their nearest legal aid provider. The green bar displays the number of MSOAs that experienced an increase in distance compared to the baseline distance in Q1 2013. The blue bar represents the stock of units that were treated in previous periods and are still treated at that time. Note that the total number of observations is not stable over time, as "Switchers" areas —meaning areas that experience a decrease in distance after having experienced an increase— are removed from the sample after the decrease occurs. In addition, we remove areas defined as "Treated several times" when they experience a new increase in distance. Therefore, the sample is restricted to areas that experience at most an increase in distance once.

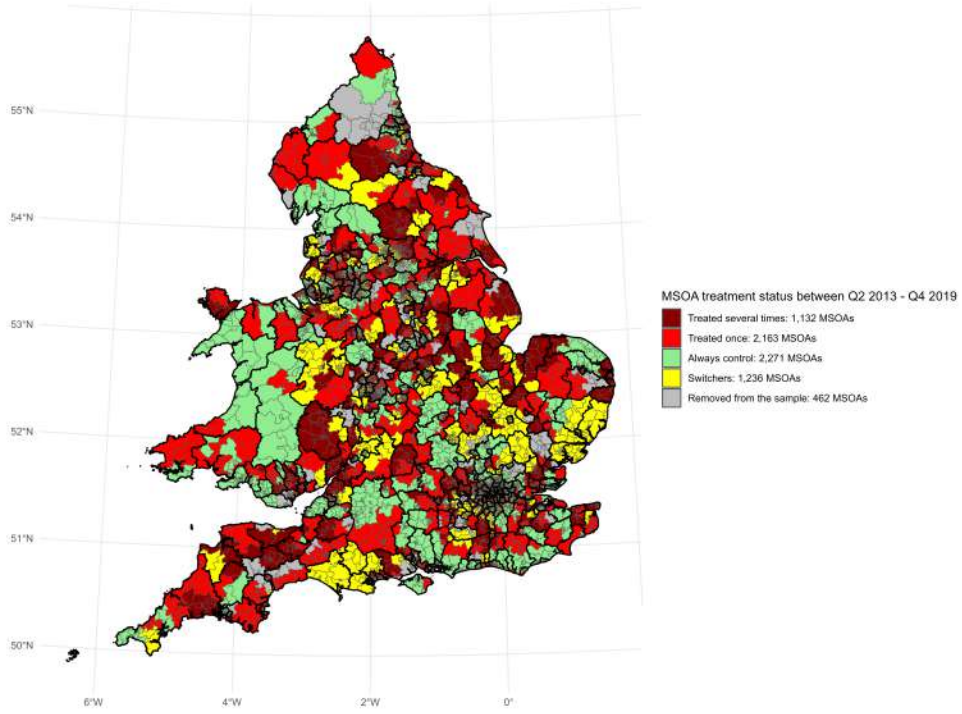


FIGURE 11: Dynamic Treatment Distribution Across England and Wales - without Negative Treatment

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" if it experiences several increases between the distance to the nearest legal aid provider and its population-weighted centroid, between Q1 2013 and Q4 2019. "Treated once" areas only experience a single increase in distance. An area is classified as "Switchers" if it experiences at least one increase in distance followed by a 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. Areas "removed from the sample" include those that solely experience decreases in distance and/or first encounter a decrease before an increase.

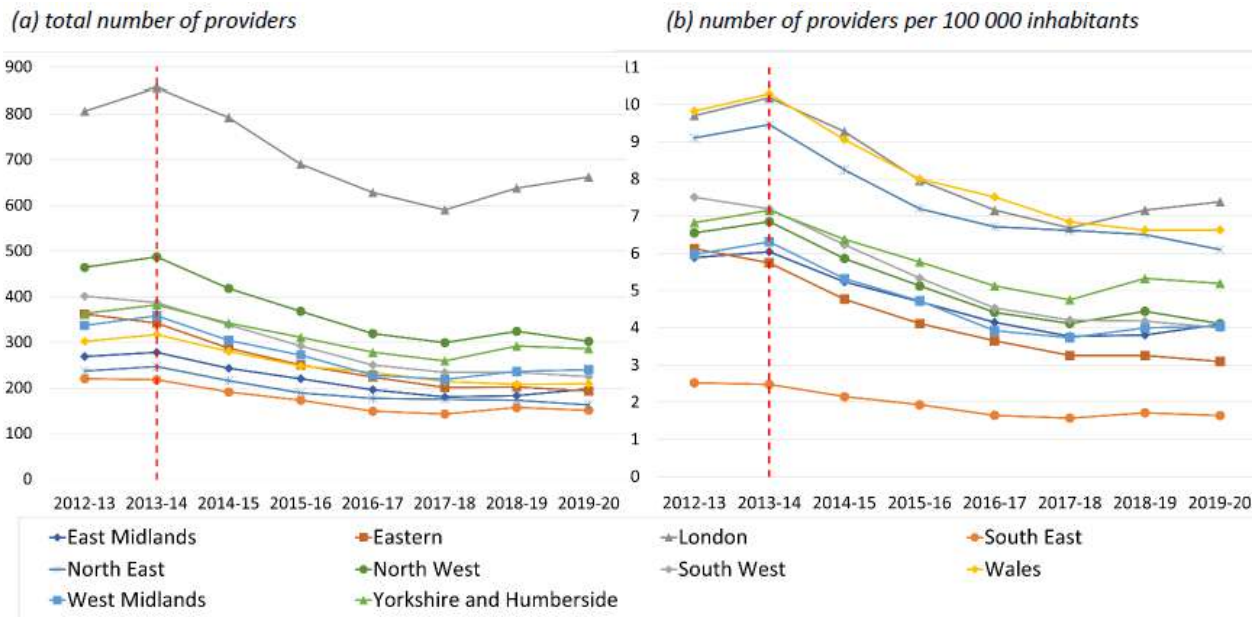
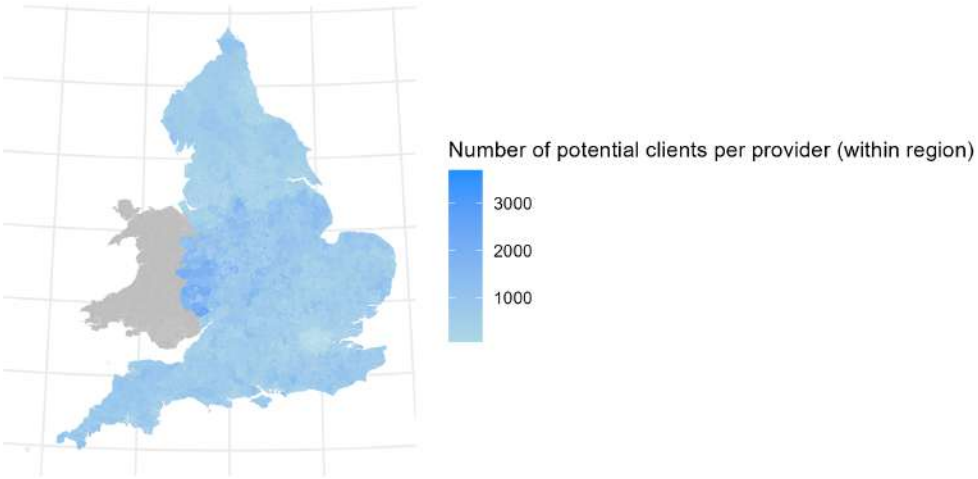
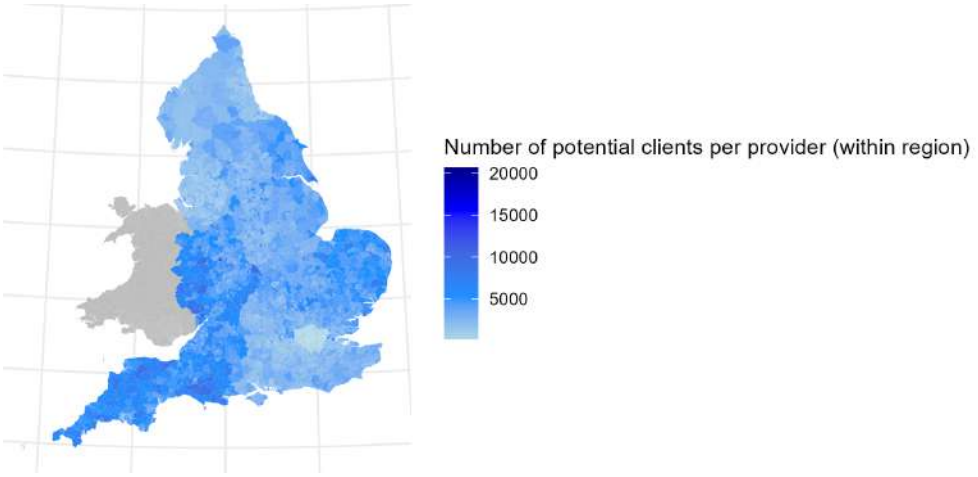


FIGURE 12: Number of legal aid providers across regions in England and Wales

Source: Ministry of Justice Legal Aid Statistics, own computation; Mid-year regional-level population estimates from Office for National Statistics (ONS).



(a) 2011



(b) 2021

FIGURE 13: 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.

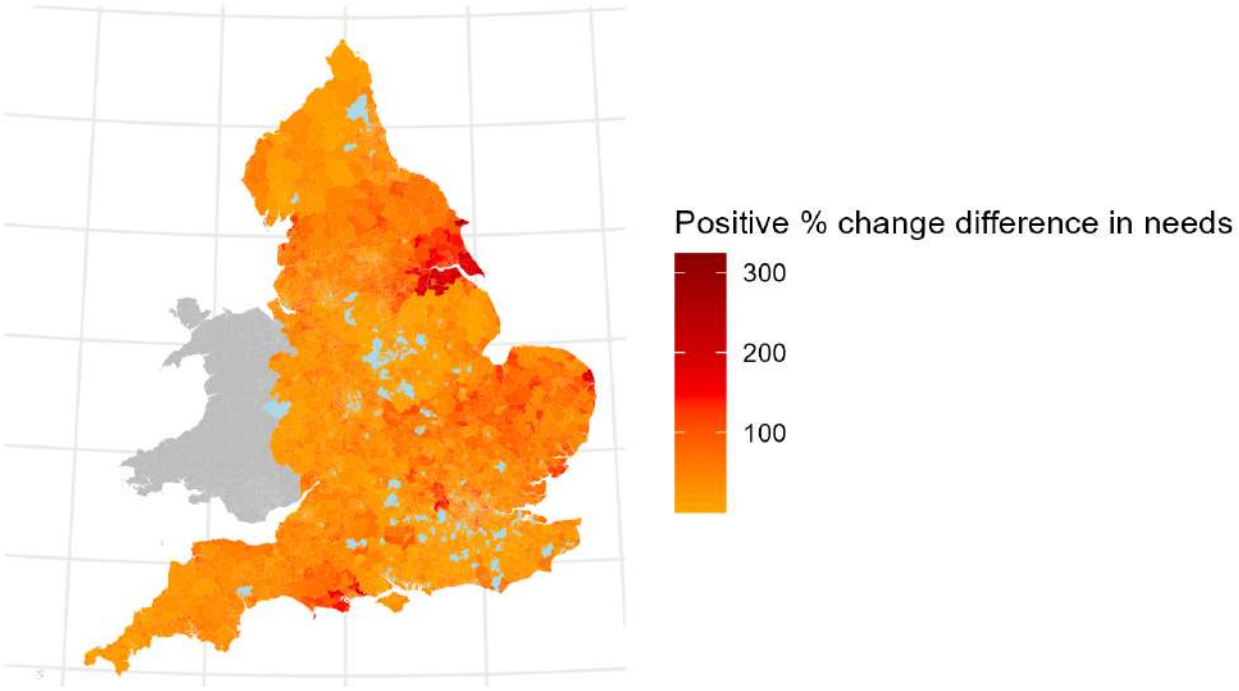


FIGURE 14: Evolution of housing legal needs between 2011 and 2021

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.

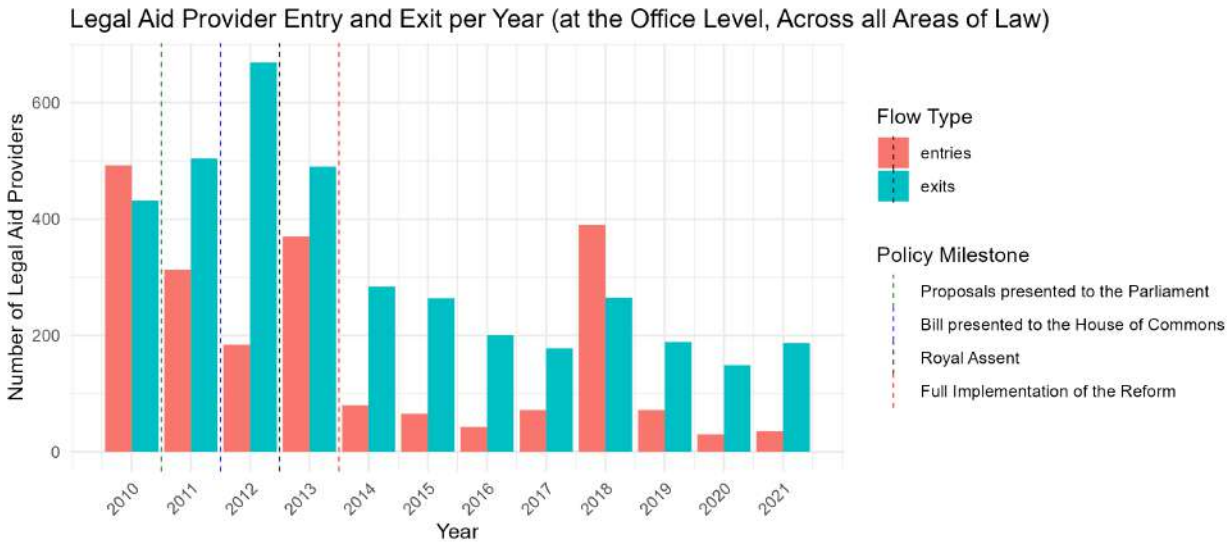


FIGURE 15: Entry and Exit Flows of Legal Aid Providers Over Time

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.

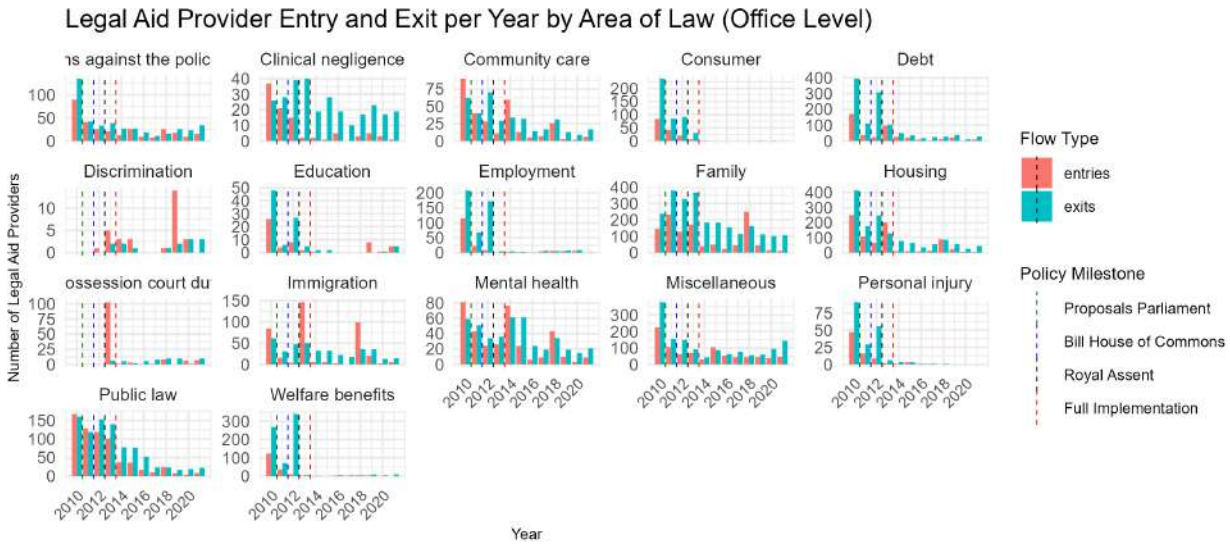


FIGURE 16: Entry and Exit Flows of Legal Aid Providers Over Time, by Area of Law

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.



FIGURE 17: 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.

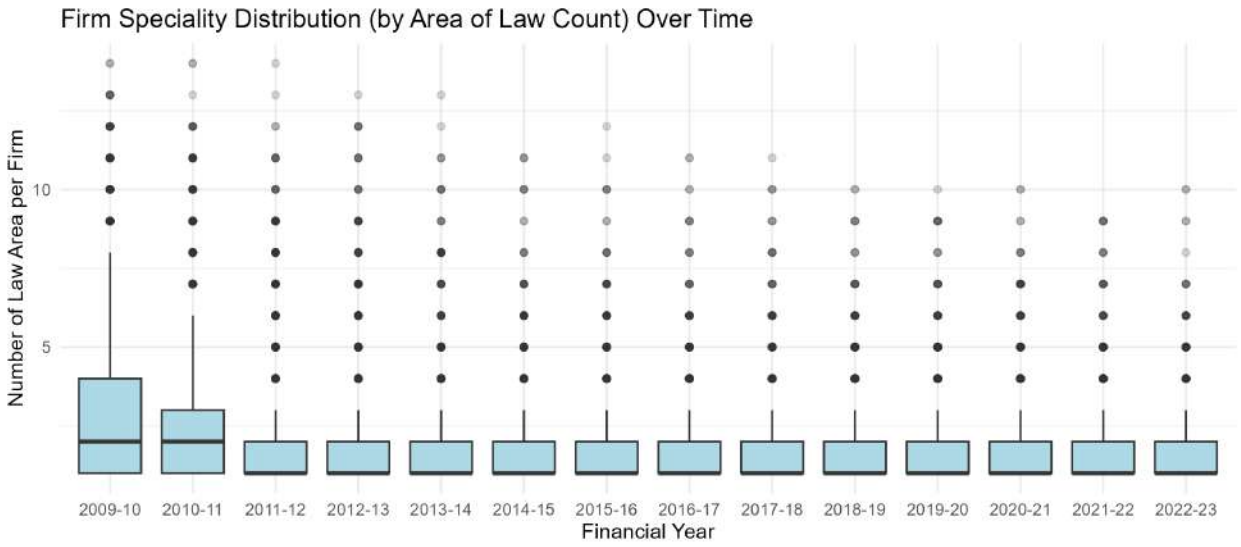


FIGURE 18: Boxplots Illustrating the Level of Legal Aid Provider Specialisation Over Time

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.

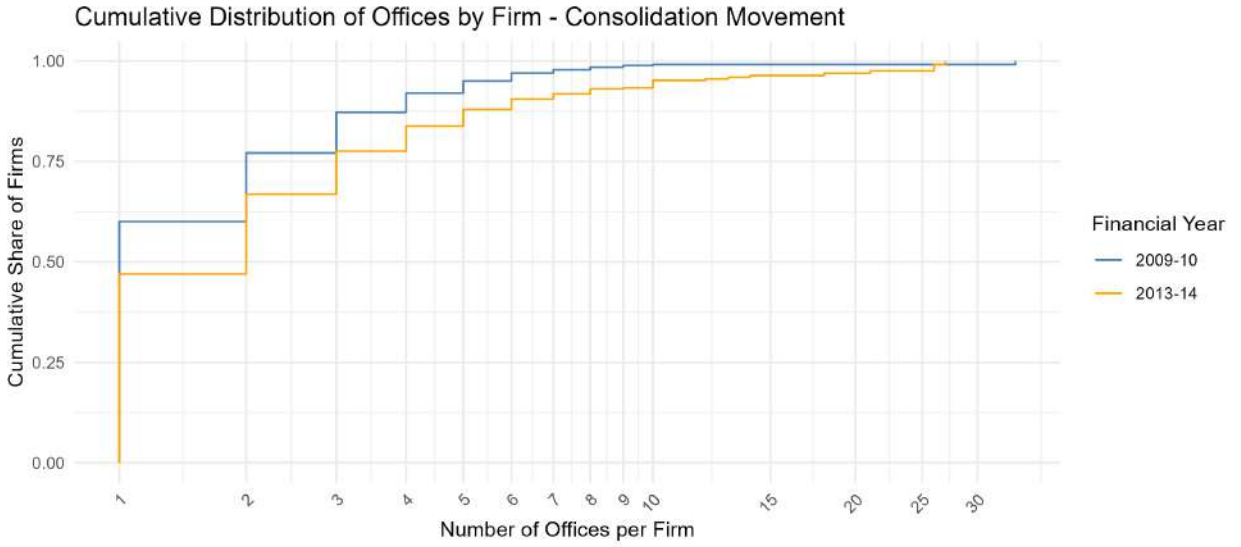


FIGURE 19: Cumulative Distribution of the Number of Provider Offices, 2009–2014

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.

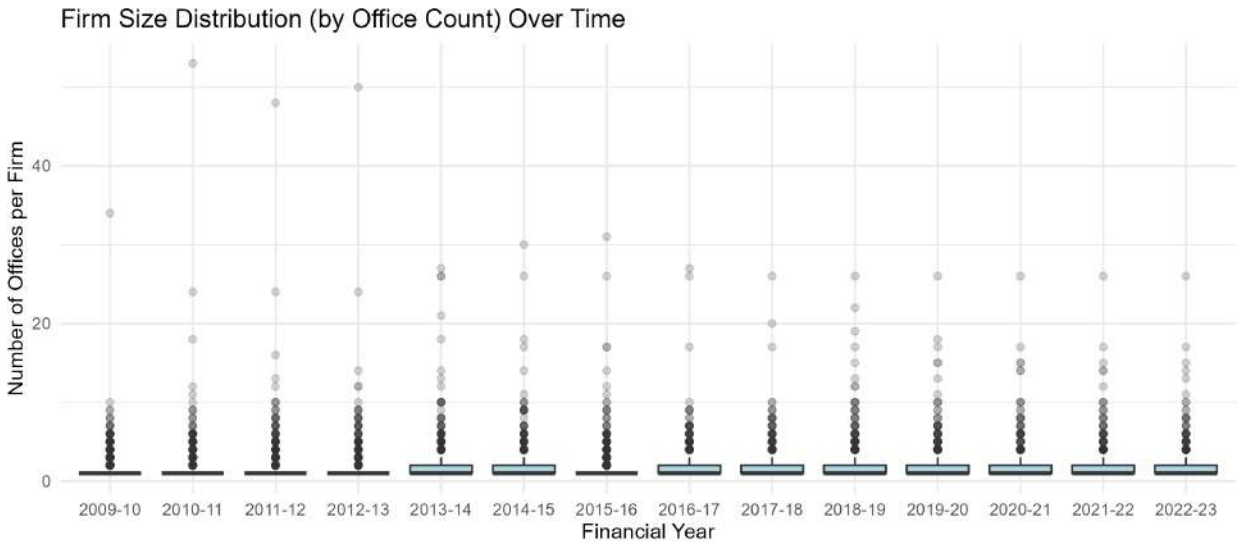


FIGURE 20: Boxplots Illustrating the Level of Legal Aid Firm Consolidation Over Time

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.

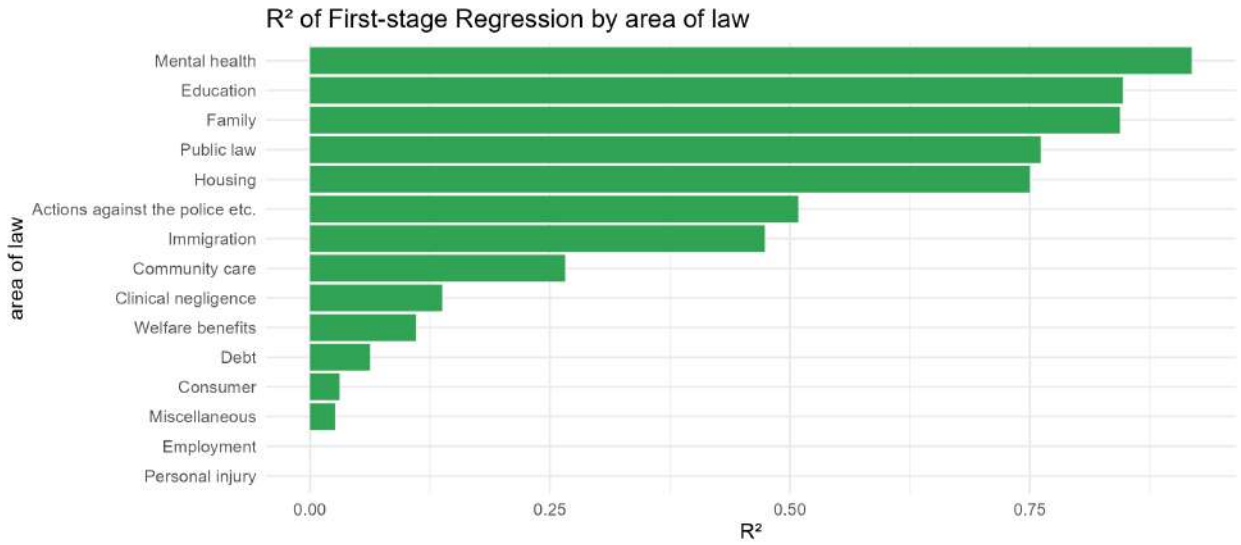


FIGURE 21: Instrument Relevance: R^2 from First-Stage Regressions by Area of Law

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.

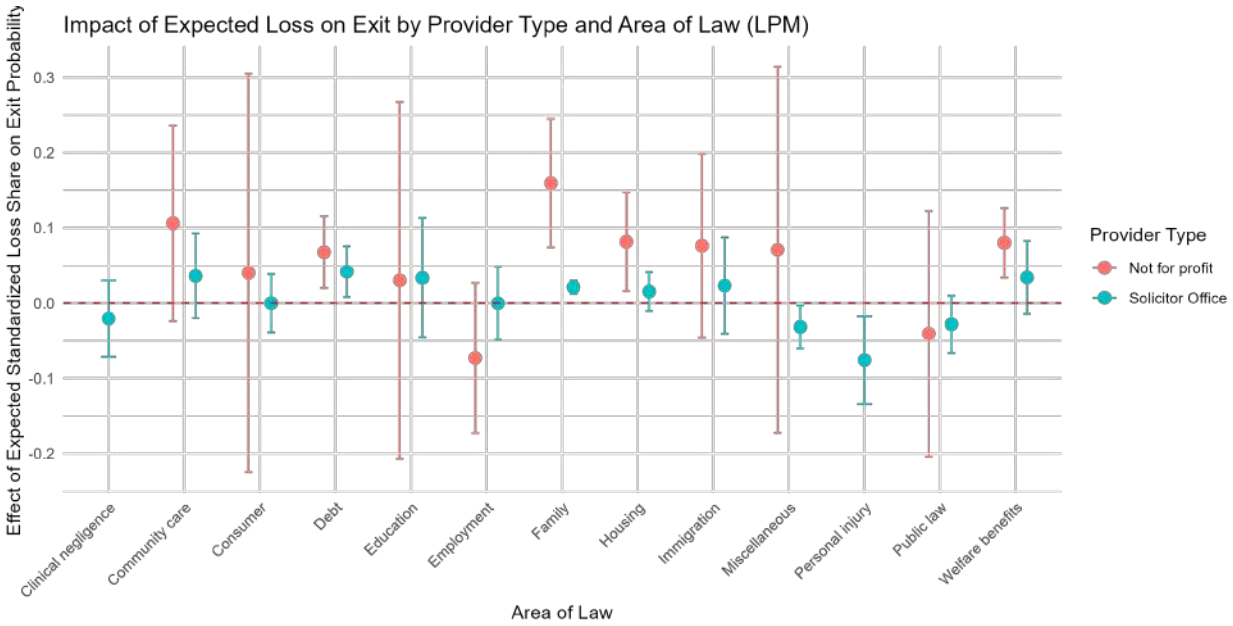


FIGURE 22: Instrument Predictiveness for Provider Exit by Area of Law and Provider Type

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.

B Additional Figures

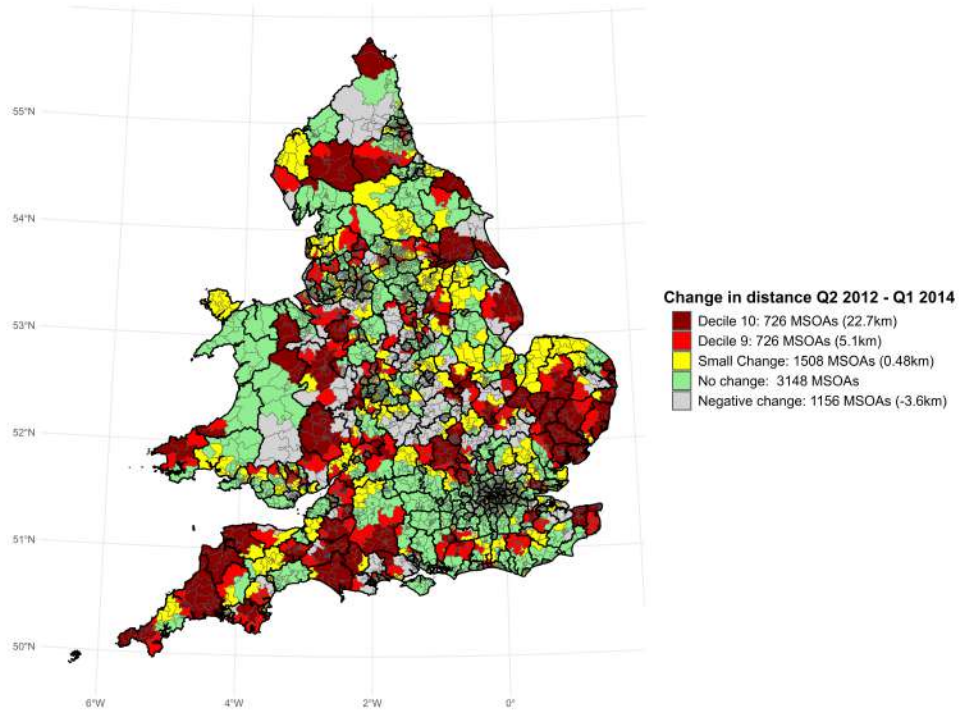


FIGURE 23: Distribution of the Treatment Group across MSOAs - Welfare Providers - Static

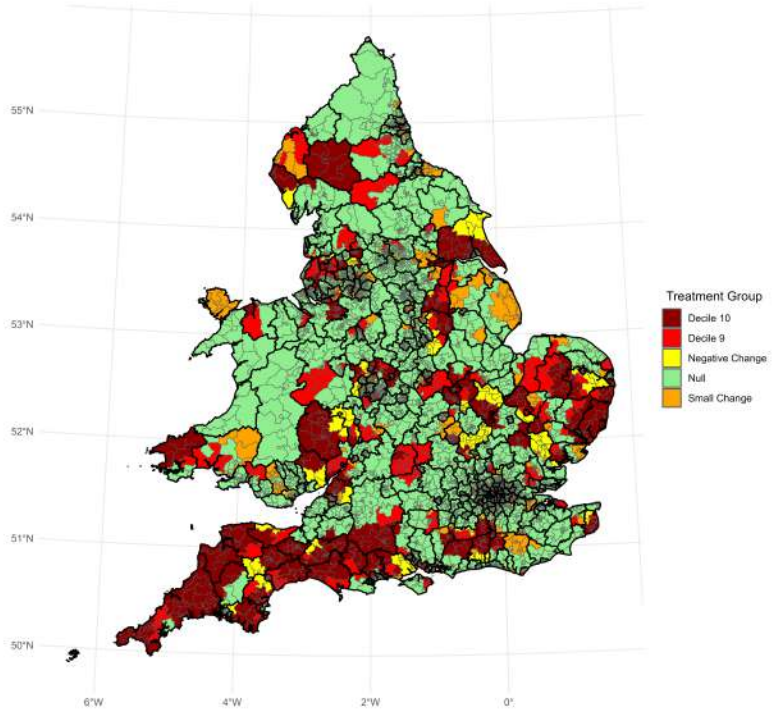


FIGURE 24: Distribution of the Treatment Group across MSOAs - Housing Providers - Static

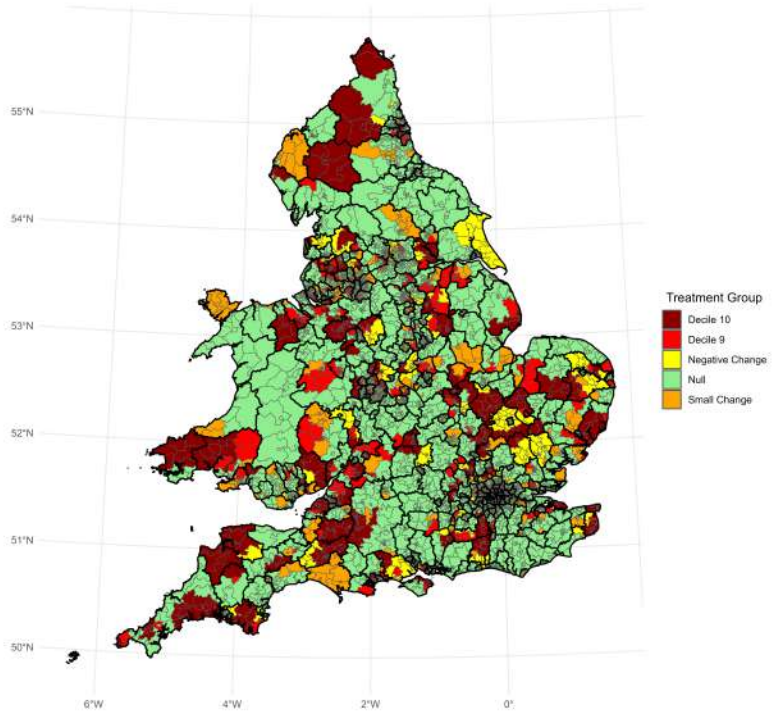


FIGURE 25: Distribution of the Treatment Group across MSOAs - Civil Providers - Static

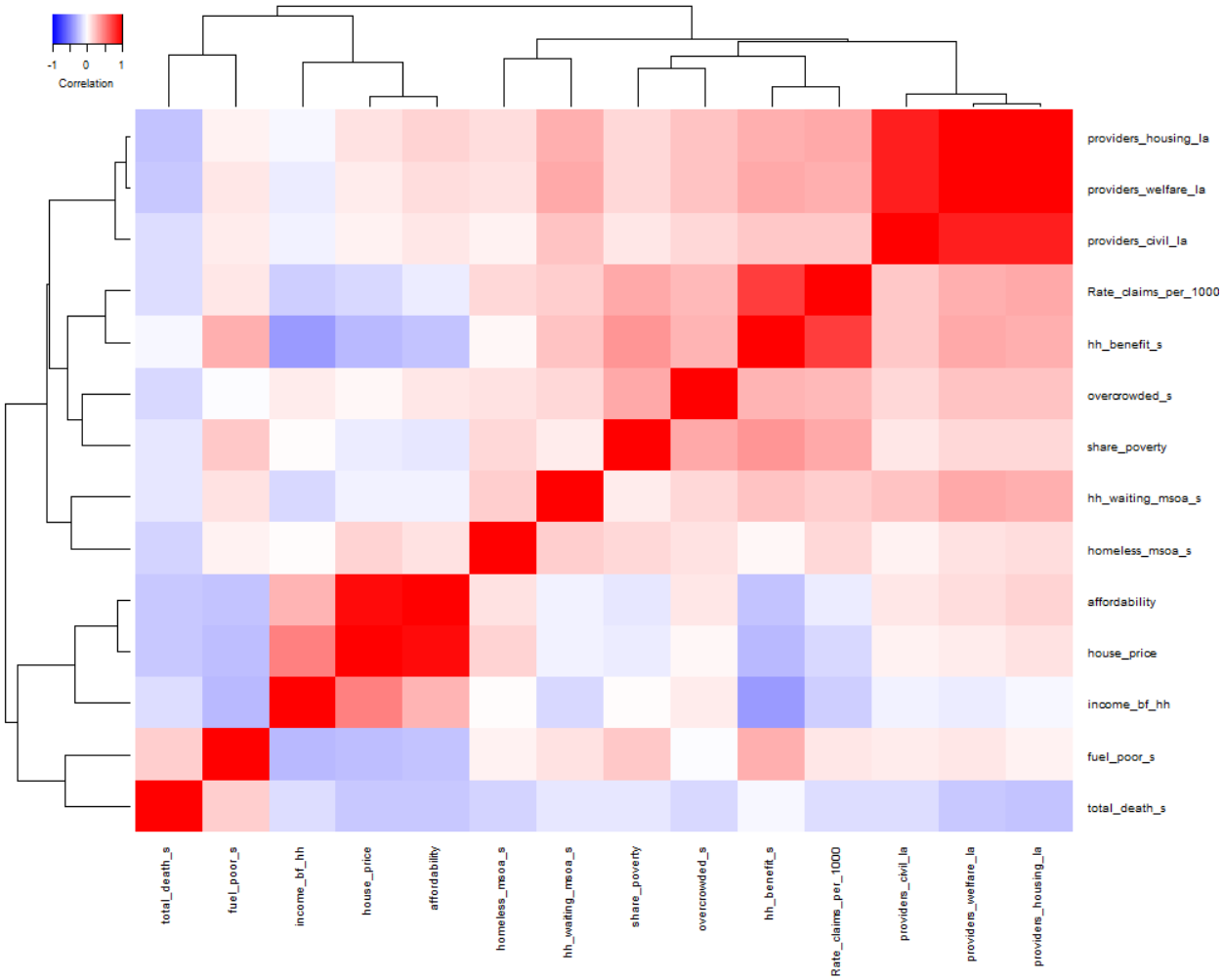


FIGURE 26: Correlation of variables

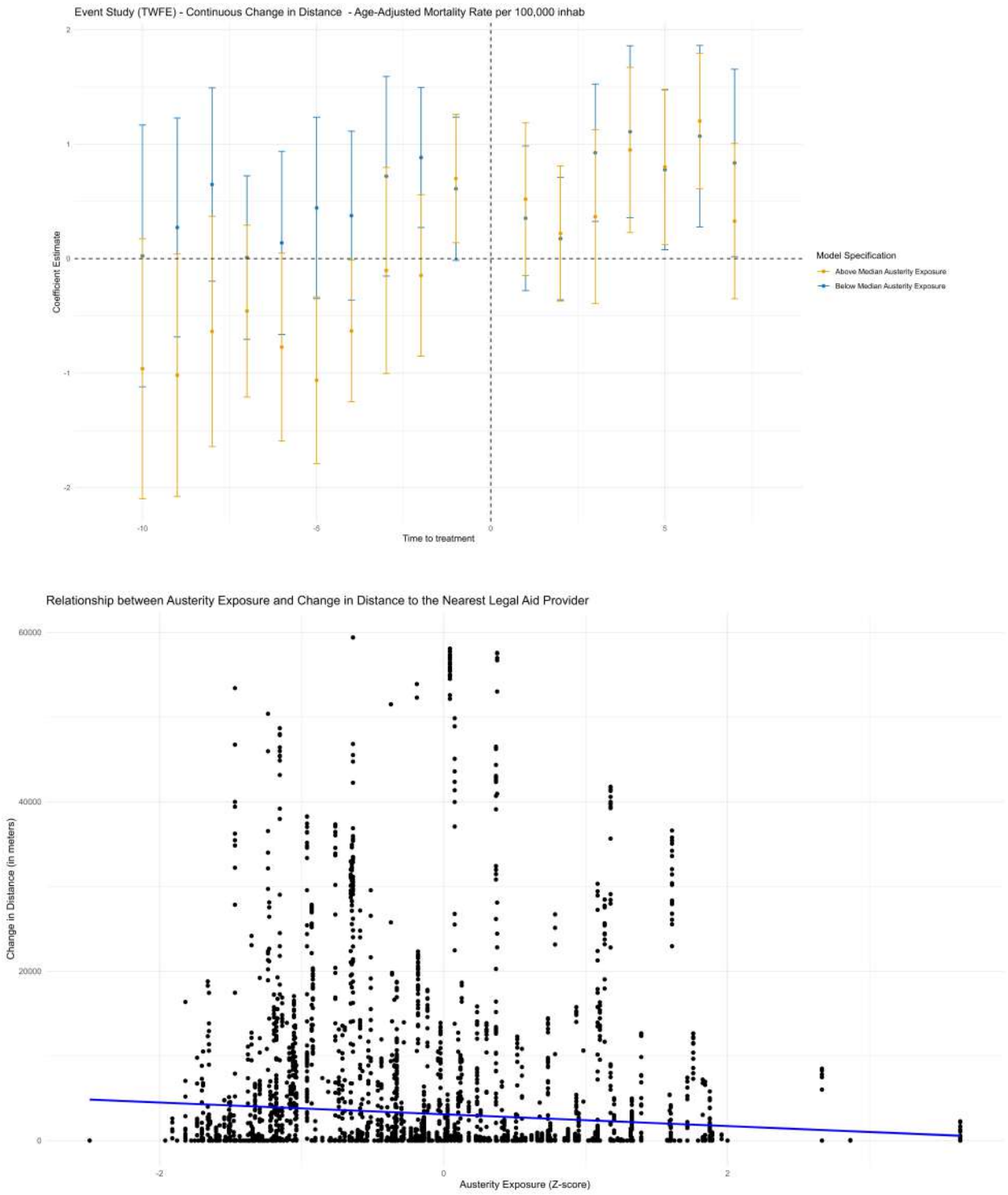


FIGURE 27: Relationship between austerity exposure and change in distance to the nearest legal aid provider

TABLE 1: Statistical difference in means between Treated and Untreated in 2013

Covariate	P-value	T-test Null Hypothesis
Static Treatment		
Change in distance - Welfare	0.00	REJECTED
Baseline distance - Welfare	0.00	REJECTED
Change in distance - Housing	0.00	REJECTED
Baseline distance - Housing	0.00	REJECTED
Change in distance - Civil	0.00	REJECTED
Baseline distance - Civil	0.00	REJECTED
Provision of legal services		
Nb Welfare Providers in LA	0.00	REJECTED
Nb Civil Providers in LA	0.00	REJECTED
Nb Housing Providers in LA	0.00	REJECTED
Eviction court cases		
Eviction Claims	0.00	REJECTED
Eviction Suspended PO	0.00	REJECTED
Eviction Outright Orders	0.00	REJECTED
Predictors of legal needs		
On Fuel Poverty	0.42	NULL
On Housing Benefit	0.00	REJECTED
Share on Poverty	0.12	NULL
Annual Income bf Housing	0.00	REJECTED
Housing market tensions indices		
In Overcrowded Housing	0.66	NULL
Housing Affordability index	0.00	REJECTED
Mean House Prices	0.00	REJECTED
On Housing Waiting List	0.03	REJECTED
Assessed Risk Homeless	0.00	REJECTED
Mortality		
Mortality rate	0.00	REJECTED

Note: The table displays the results of pair-wise t-test between areas defined as treated and untreated, using a static treatment definition. Areas are defined as treated if they are in the highest quintiles of areas that experience an increase in distance to their nearest legal aid provider between Q1 2013 and Q1 2015. Areas are classified as untreated if they experienced no change in distance over that time period. If the t-test is rejected, this means that the null hypothesis of no statistical difference between the two means is rejected at a significance level set at 95% (p-value below 0.05).

C Tables

TABLE 2: 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 3: Summary statistics: Distribution of Possession Claims and Orders Across MSOAs by Year

Year	Claims						Orders					
	Min	**Q1**	**Mean**	**Q3**	**Max**	**Sd**	**Min**	**Q1**	**Mean**	**Q3**	**Max**	**Sd**
2000	0	12.00	32.92	43.00	285	31.28	0	8.00	21.76	29.00	203	20.24
2001	0	12.00	32.12	42.00	323	31.09	0	8.00	22.18	30.00	208	21.06
2002	0	12.00	32.06	42.00	274	30.51	0	7.00	21.49	29.00	209	20.54
2003	0	12.00	31.91	42.00	222	28.63	0	8.00	21.10	29.00	169	19.26
2004	0	13.00	33.45	44.00	234	28.81	0	8.00	21.81	30.00	166	19.47
2005	0	17.00	37.81	51.00	248	28.92	0	10.00	24.70	34.00	148	19.38
2006	0	18.00	39.51	53.00	320	29.48	0	12.00	27.02	37.00	239	20.76
2007	1	19.00	39.34	53.00	366	28.41	0	14.00	31.11	42.00	359	24.14
2008	0	19.00	40.20	54.00	298	28.47	0	16.00	35.59	48.00	278	26.54
2009	0	15.00	31.85	42.00	210	24.11	0	12.00	26.90	37.00	207	20.66
2010	0	13.00	29.16	39.00	197	22.39	0	10.00	22.50	31.00	141	16.85
2011	0	14.00	29.83	39.00	226	23.12	0	10.00	22.77	31.00	187	17.41
2012	0	12.00	29.21	38.00	229	24.29	0	9.00	21.76	29.00	193	17.54
2013	0	13.00	31.02	40.00	311	26.91	0	9.00	22.50	30.00	224	19.61
2014	0	11.00	28.43	37.00	234	25.25	0	8.00	21.67	29.00	175	19.32
2015	0	8.00	23.97	31.00	268	23.48	0	6.00	18.29	25.00	213	17.95
2016	0	8.00	21.53	28.00	224	20.52	0	6.00	16.52	22.00	183	16.02
2017	0	8.00	21.08	29.00	199	18.96	0	6.00	15.77	21.00	187	14.46
2018	0	7.00	19.52	26.00	204	17.18	0	5.00	14.69	20.00	201	13.21
2019	0	8.00	18.86	25.00	157	15.78	0	6.00	14.47	20.00	118	12.56
2020	0	2.00	6.23	8.00	147	6.07	0	1.00	3.58	5.00	81	3.64
2021	0	2.00	6.41	8.00	73	5.94	0	1.00	3.67	5.00	31	3.70
2022	0	5.00	12.65	17.00	130	11.49	0	4.00	9.42	13.00	102	8.66

Source: Ministry of Justice data obtained through Freedom of Information
Note: 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.

TABLE 5: 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.

TABLE 7: Summary statistics: Baseline value 2013

Variable	Min	Q1	Mean	Q3	Max	SD	N_obs
Static Treatment							
Change in distance - Welfare	-39826.01	0.00	1570.82	235.68	53889.25	6128.35	7264
Baseline distance - Welfare	41.79	1411.11	5915.83	7723.89	63630.57	6959.92	7264
Change in distance - Housing	-25600.93	0.00	1426.37	64.27	63924.05	5969.25	7264
Baseline distance - Housing	45.04	1536.84	6430.65	8790.91	63630.57	7154.96	7264
Change in distance - Civil	-61230.45	0.00	959.54	84.40	62889.01	5277.53	7264
Baseline distance - Civil	41.79	1152.52	5026.36	6006.53	62855.71	6525.66	7264
Provision of legal services							
Nb Welfare Providers in LA	0.00	0.21	0.69	0.90	53.89	0.94	7264
Nb Civil Providers in LA	0.00	0.50	1.23	1.61	190.68	2.45	7264
Nb Housing Providers in LA	0.00	0.16	0.64	0.83	49.74	0.90	7264
Eviction court cases							
Eviction Claims	0.00	0.85	2.83	3.85	23.27	2.88	7264
Eviction Suspended PO	0.00	0.18	0.90	1.21	14.26	1.09	7264
Eviction Outright Orders	0.00	0.31	1.15	1.46	21.05	1.38	7264
Predictors of legal needs							
On Fuel Poverty	10.78	32.46	43.50	50.53	154.45	15.97	7264
On Housing Benefit	3.31	40.26	79.73	109.23	453.46	51.15	7264
Share on Poverty	5.10	14.90	21.62	26.70	59.80	8.87	7264
Annual Income bf Housing	15600.00	24440.00	29002.53	32760.00	57720.00	6089.45	7264
Housing market tensions indices							
In Overcrowded Housing	2.66	13.09	34.40	41.08	312.52	33.88	7264
Housing Affordability index	2.10	5.28	7.28	8.34	90.17	3.50	7264
Mean House Prices	38199.10	132044.62	224590.43	263893.35	4735734.28	168807.91	7264
On Housing Waiting List	0.07	18.29	32.21	40.53	217.54	21.45	7264
Assessed Risk Homeless	0.07	0.88	2.11	2.80	12.62	1.73	7264
Mortality							
Mortality rate	0.18	6.67	9.00	10.96	30.43	3.43	7264

Note: Distance is measured in meters. Change in distance is measured between Q1 2013 and Q1 2015. The number of active providers is measured for the year 2013. All relevant variables are population-adjusted and represent the rate for 1,000 inhabitants. The number of households living in overcrowded housing is taken from the 2011 Census, no data is available for 2013. Yearly averages are taken for quarterly variables.

TABLE 8: Summary statistics - 2019

Variable	Min	Q1	Mean	Q3	Max	SD	N_obs
Provision of legal services							
Nb Welfare Providers in LA	0.00	0.00	0.39	0.51	10.29	0.52	7264
Nb Civil Providers in LA	0.00	0.16	0.69	0.90	74.58	1.11	7264
Nb Housing Providers in LA	0.00	0.00	0.38	0.49	10.29	0.51	7264
Eviction court cases							
Eviction Claims	0.00	0.52	1.72	2.44	14.96	1.62	7264
Eviction Suspended PO	0.00	0.12	0.55	0.73	8.88	0.71	7264
Eviction Outright Orders	0.00	0.25	0.84	1.19	7.30	0.84	7264
Predictors of legal needs							
On Fuel Poverty	8.40	39.91	56.42	71.26	152.42	22.17	7264
On Housing Benefit	1.52	27.91	53.12	72.26	229.30	32.93	7264
Housing market tensions indices							
Housing Affordability index	1.50	5.37	9.53	11.58	153.43	7.08	7264
Mean House Prices	48053.92	172387.27	305637.34	371462.76	4922700.88	227257.88	7264
On Housing Waiting List	0.43	10.09	21.30	26.33	146.31	17.37	7264
Mortality							
total_death_s	0.14	6.59	8.92	10.93	34.85	3.38	7264

TABLE 9: Summary statistics: Distribution of Population Size Across MSOAs by Year - using 2011 Census Boundaries

Year	Min	P10	Q1	Median	Mean	Q3	Max	P90	Sd
2001	2140.00	5742.90	6155.00	7261.50	7275.76	7973.50	15543.00	9077.00	1330.20
2002	113.00	5738.00	6175.00	7271.00	7304.84	8037.00	15463.00	9138.00	1408.04
2003	283.00	5748.00	6210.00	7292.00	7341.10	8104.00	15488.00	9185.00	1415.96
2004	571.00	5774.00	6241.00	7319.00	7381.20	8145.00	15875.00	9233.00	1427.37
2005	1080.00	5791.00	6293.00	7359.00	7439.99	8232.00	17084.00	9334.00	1447.46
2006	1738.00	5798.00	6338.00	7399.00	7492.13	8300.00	16336.00	9423.00	1464.25
2007	2293.00	5826.00	6390.00	7444.00	7552.76	8382.00	15901.00	9521.00	1482.80
2008	2333.00	5850.00	6439.00	7498.00	7615.85	8476.00	15914.00	9625.00	1504.66
2009	2251.00	5864.00	6476.00	7532.00	7670.50	8554.00	16060.00	9719.00	1531.24
2010	2228.00	5887.00	6517.00	7569.00	7733.98	8638.00	16225.00	9827.00	1568.27
2011	2224.00	5910.00	6559.00	7629.00	7800.43	8731.00	16439.00	9944.00	1607.53
2012	2279.00	5923.00	6585.00	7672.00	7855.55	8802.00	16810.00	10075.00	1641.65
2013	2265.00	5944.00	6618.00	7706.00	7908.38	8883.00	16794.00	10178.00	1678.18
2014	2292.00	5968.00	6654.00	7762.00	7972.32	8962.00	17761.00	10267.00	1715.49
2015	2335.00	5996.00	6703.00	7810.00	8038.52	9049.00	18534.00	10369.00	1765.75
2016	2331.00	6018.00	6744.00	7864.00	8107.38	9135.00	20939.00	10470.00	1818.68
2017	2259.00	6028.00	6759.00	7894.00	8157.84	9186.00	23150.00	10569.00	1869.17
2018	2242.00	6046.00	6781.00	7927.00	8209.39	9244.00	24969.00	10675.00	1924.80
2019	2224.00	6046.00	6807.00	7955.00	8254.39	9286.00	26513.00	10777.00	1977.92
2020	2226.00	6050.00	6810.00	7974.00	8293.25	9348.00	27911.00	10887.00	2034.04

Source: Office for National Statistics

Note: Mid-year (30 June) estimates of the usual resident population for Middle layer Super Output Areas (MSOAs) in England and Wales by year, using 2011 Census Boundaries. MSOA boundaries are drawn to ensure an average number of inhabitants of 8,000. Every census, in 2001, 2011 and 2021, redefined the boundaries of these units to ensure population-size consistency over time by adjusting them based on changes in population density and distribution. This explains why the mean population size across MSOAs observed in this table differs relatively more from 8,000 as one moves away from 2011.

TABLE 10: Summary statistics - Deciles of the Treatment Variable

Decile	Min	Q1	Mean	SD	Q3	Max
1	-62761.43	-7073.71	-5650.43	7083.28	-1255.05	-441.40
2	-440.35	-187.51	-101.56	126.92	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	0.78	2.98	0.00	18.48
7	18.75	72.06	143.38	80.66	213.43	298.41
8	298.66	476.08	839.31	447.70	1190.77	1882.37
9	1893.09	2964.20	5057.14	2358.67	6809.72	9854.95
10	9855.69	13394.09	22736.86	11857.61	30040.77	59392.54

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 11: Summary Statistics - Quintiles of the Treatment Variable

Quintile	Min	Q1	Mean	SD	Q3	Max
1	-62761.43	-2870.13	-2877.97	5726.86	-42.36	0.00
2	0.00	0.00	0.00	0.00	0.00	0.00
3	0.00	0.00	0.38	2.09	0.00	18.47
4	18.48	136.42	491.02	473.88	675.00	1882.37
5	1893.09	4636.72	13897.00	12297.55	18071.71	59392.54

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 12: Summary statistics - Treatment Intensity Groups - Welfare Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	9855.69	13394.09	22736.86	11857.61	30040.77	59392.54	726
Decile 9	1893.09	2964.20	5057.14	2358.67	6809.72	9854.95	726
Small Change	0.72	120.67	473.47	473.82	661.71	1882.37	1508
Negative Change	-62761.43	-3870.21	-3617.39	6209.13	-229.19	-0.06	1156
Null Change	0.00	0.00	0.00	0.00	0.00	0.00	3148

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 13: Summary statistics - Treatment Intensity Groups - Housing Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	8542.07	11953.45	21020.24	12330.97	27536.83	58086.79	726
Decile 9	1247.12	2153.54	4028.97	2085.69	5826.01	8520.67	726
Small Change	0.92	95.65	335.72	312.34	505.07	1243.90	950
Negative Change	-62761.43	-3742.06	-3349.09	5811.43	-195.58	-0.06	1268
Null Change	0.00	0.00	0.00	0.00	0.00	0.00	3594

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 15: 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 14: Summary statistics - Treatment Intensity Groups - Civil Providers

Groups	Min	Q1	Mean	SD	Q3	Max	N_obs
Decile 10	4684.54	7898.09	15381.90	10463.27	20671.95	57571.13	726
Decile 9	604.29	1127.61	2061.69	1075.58	2701.52	4681.00	726
Small Change	0.12	72.52	204.06	158.25	302.03	602.65	1151
Negative Change	-61230.45	-2909.32	-3207.23	7017.88	-168.47	-0.95	810
Null	0.00	0.00	0.00	0.00	0.00	0.00	3851

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 16: 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 Difference-in-Differences Estimators

D.1 Static DiD

D.1.1 Event-studies

D.1.2 Binary Definition of Change in Distance - top Quintile vs Areas with No Change in Distance

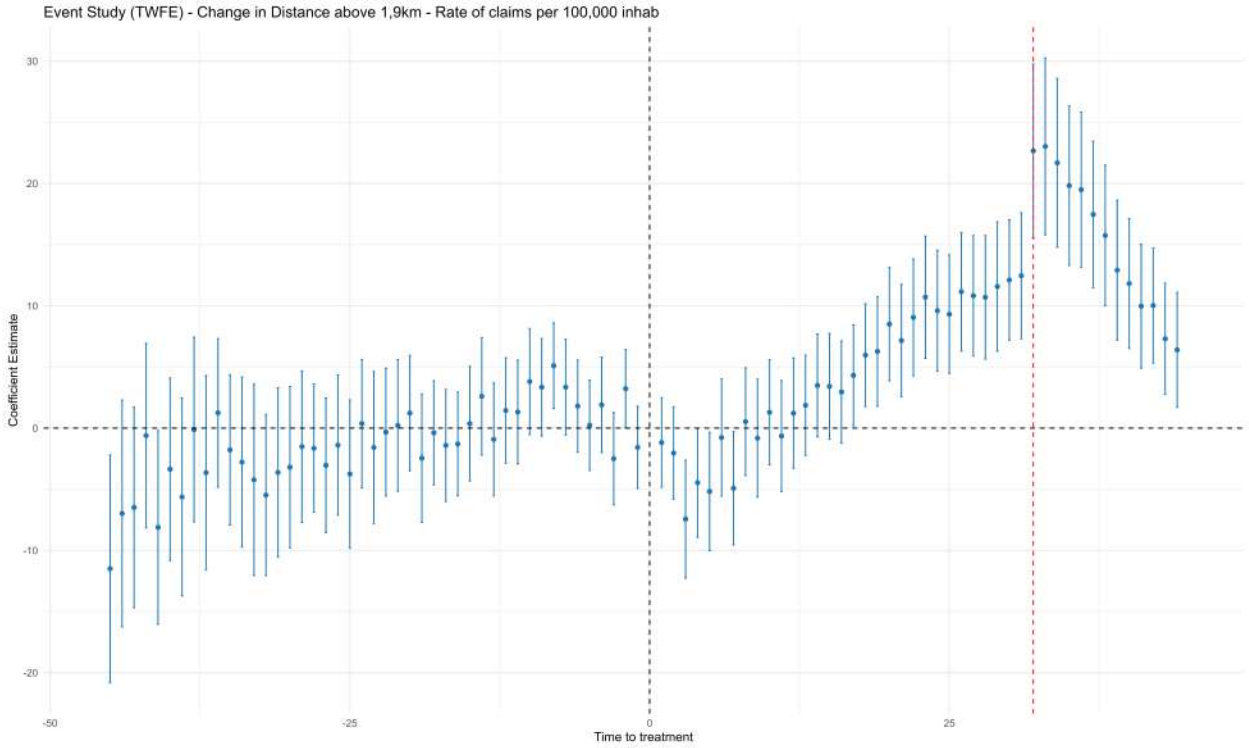


FIGURE 28: Eviction claim rate per 100,000 inhabitants - Static - Change in Distance >1.9km

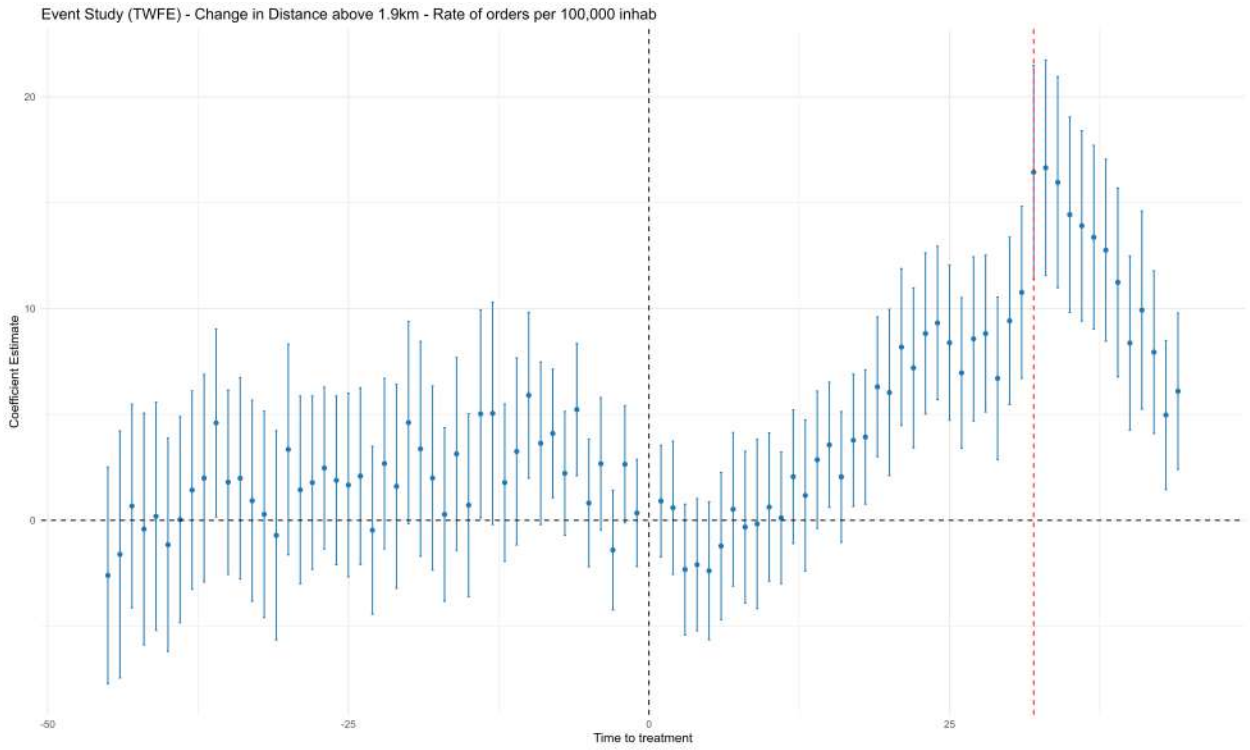


FIGURE 29: Eviction orders per 100,000 inhabitants - Static - Change in Distance >1.9km

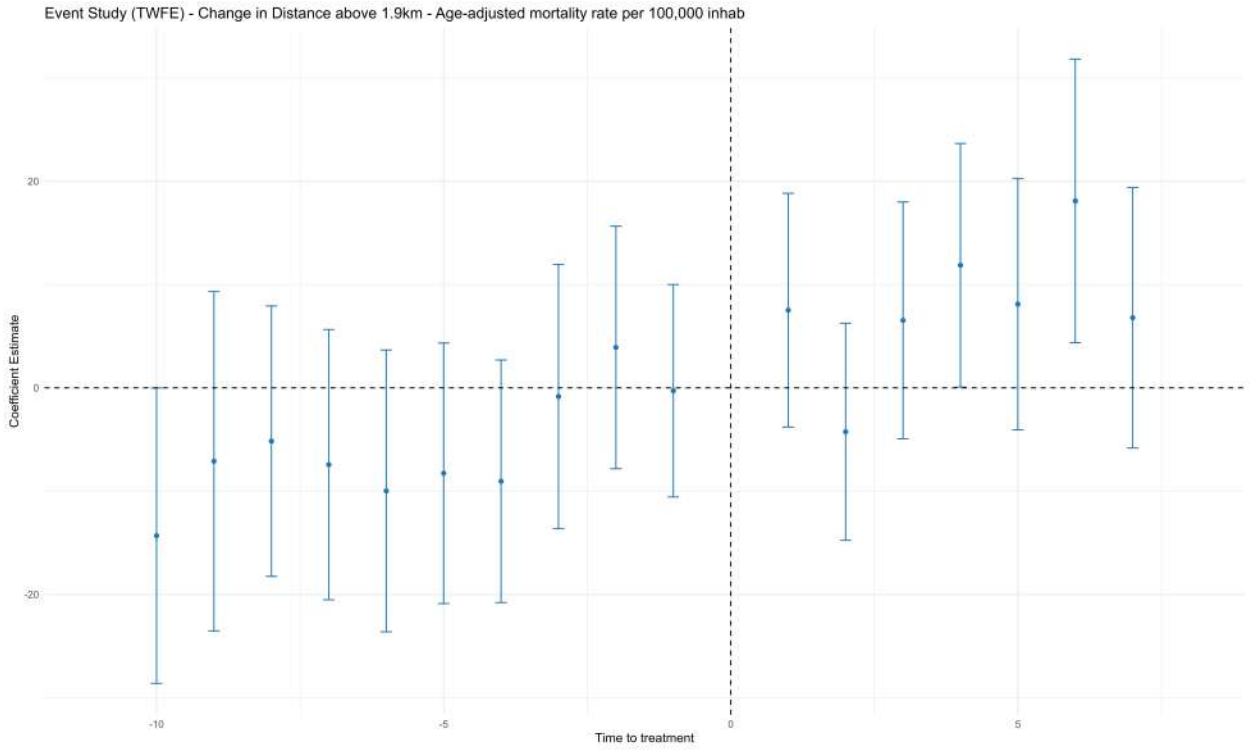


FIGURE 30: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Change in Distance >1.9km

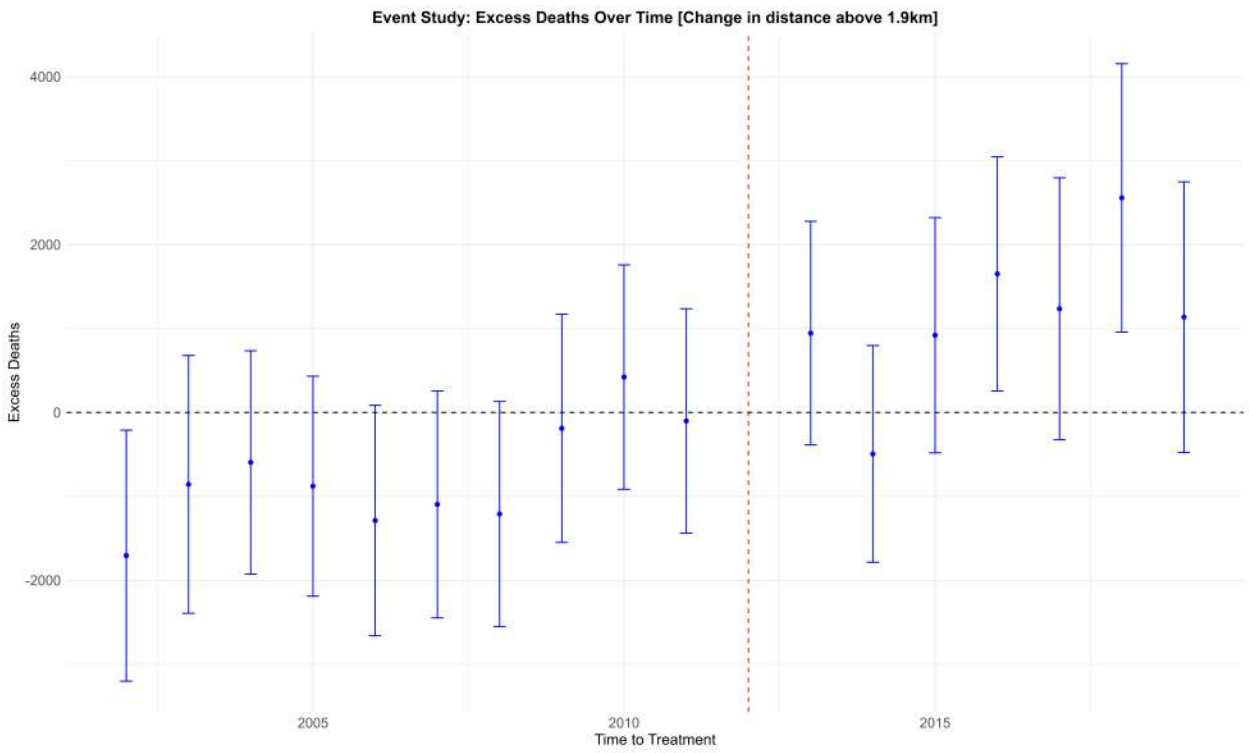


FIGURE 31: Excess deaths - Static - Change in Distance >1.9km

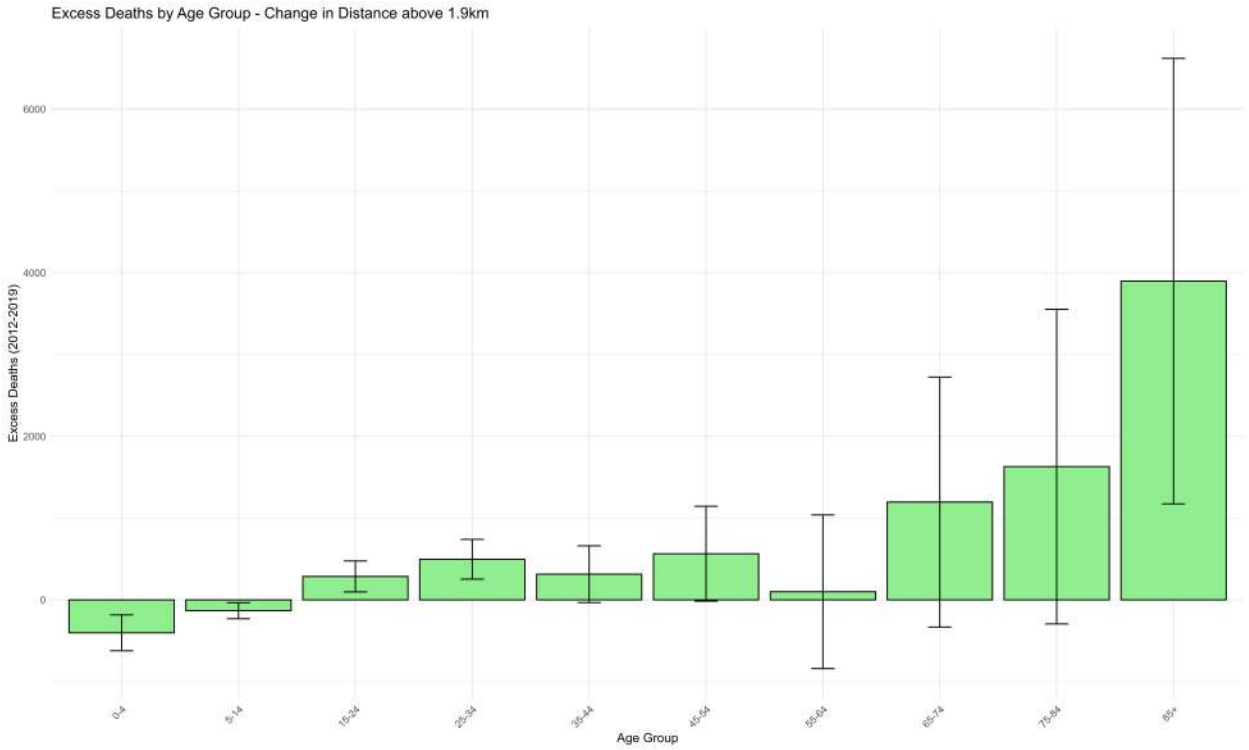


FIGURE 32: Pooled ATT Age-Adjusted mortality by Age Group - Change in Distance >1.9km

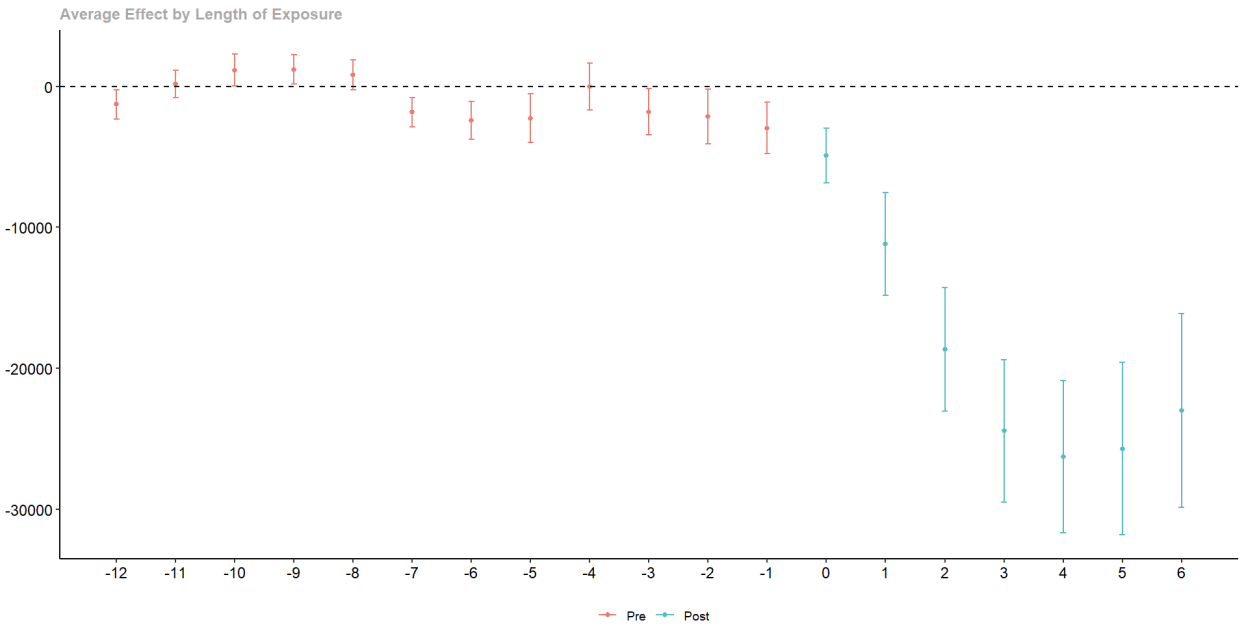


FIGURE 33: Mean House prices - Static

D.1.3 Binary Definition of Change in Distance - by Intensity of Treatment vs Areas with No Change in Distance

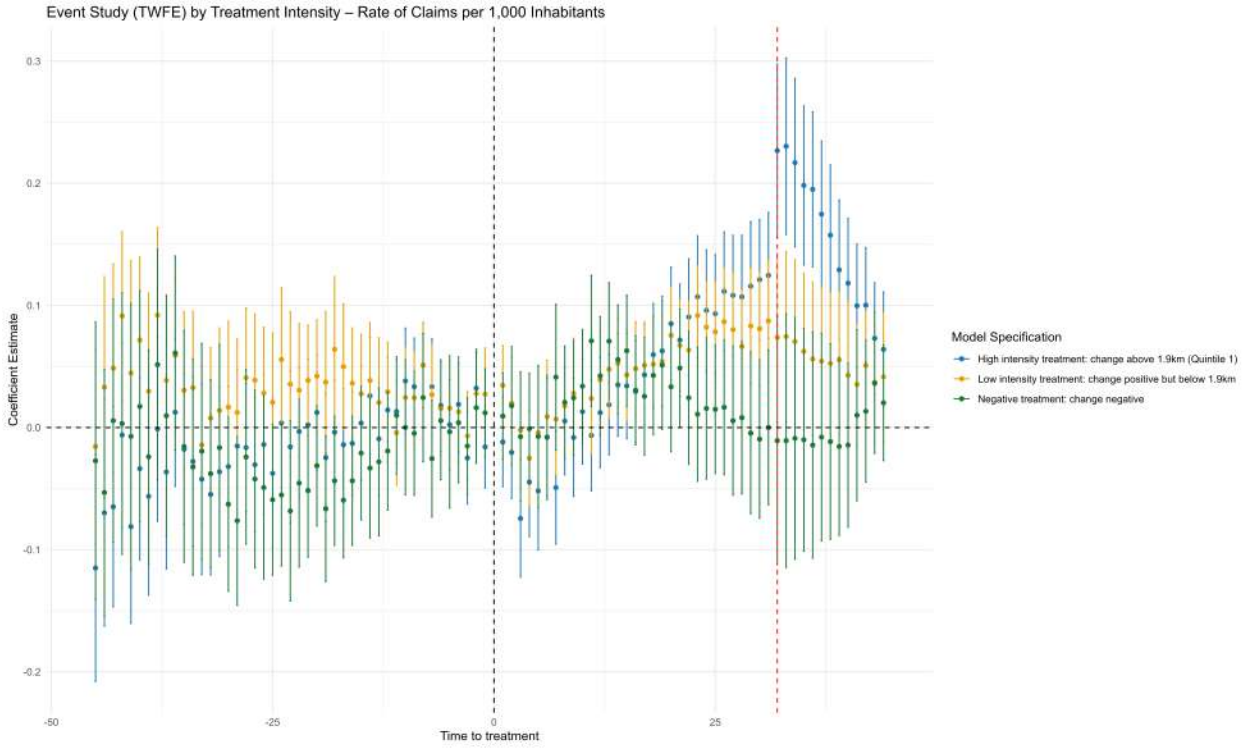


FIGURE 34: Eviction claim rate per 100,000 inhabitants - Static - Intensity of Change in Distance

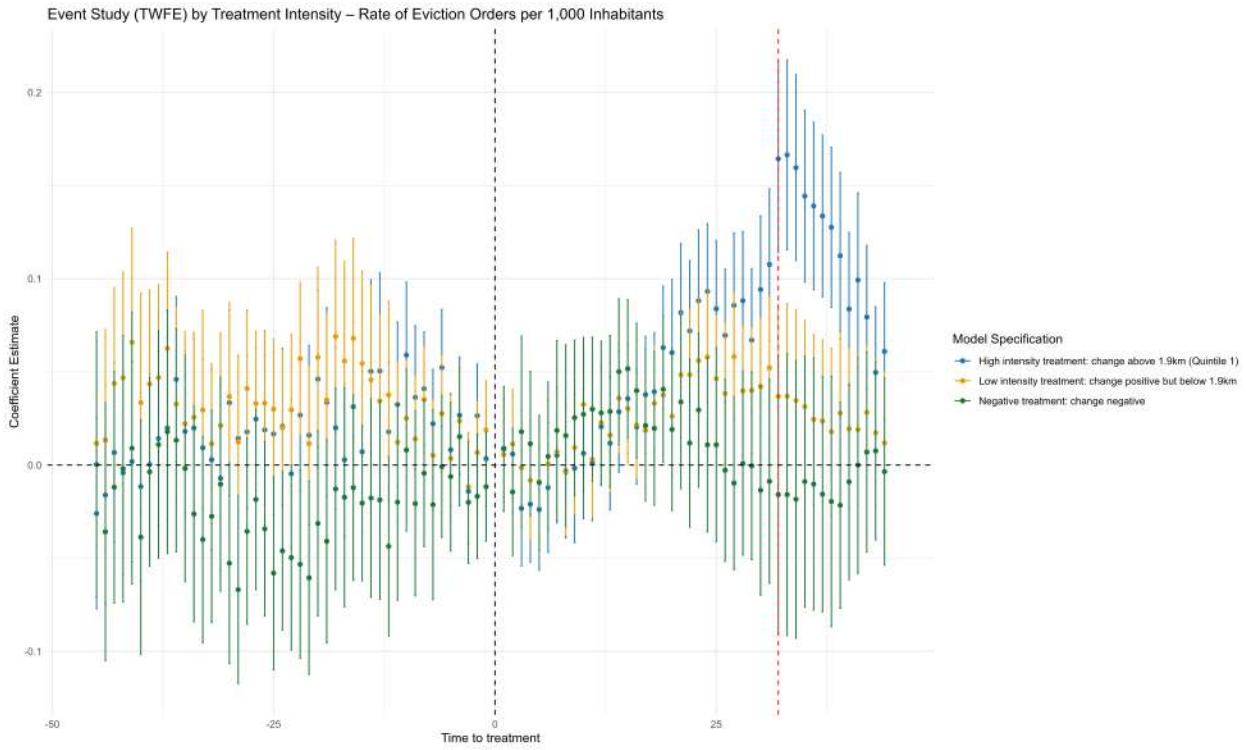


FIGURE 35: Eviction orders per 100,000 inhabitants - Static - Intensity of Change in Distance

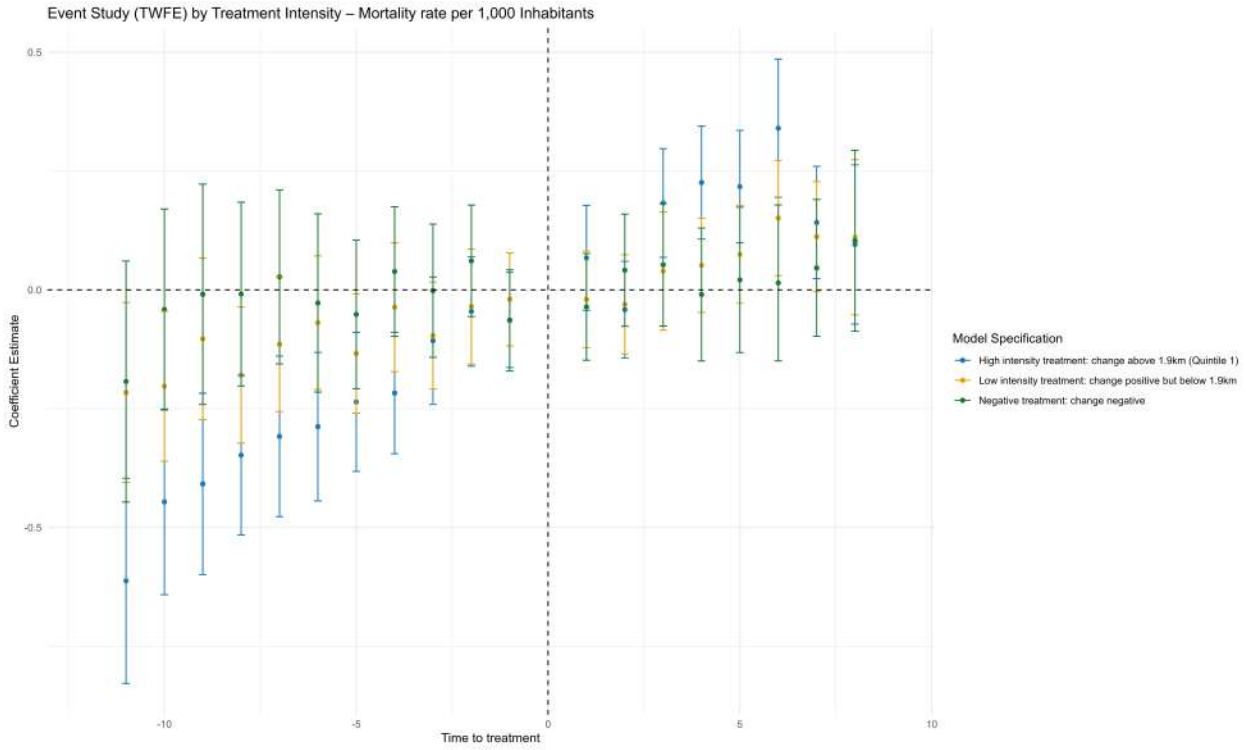


FIGURE 36: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Intensity of Change in Distance

D.1.4 Continuous Definition of Change in Distance

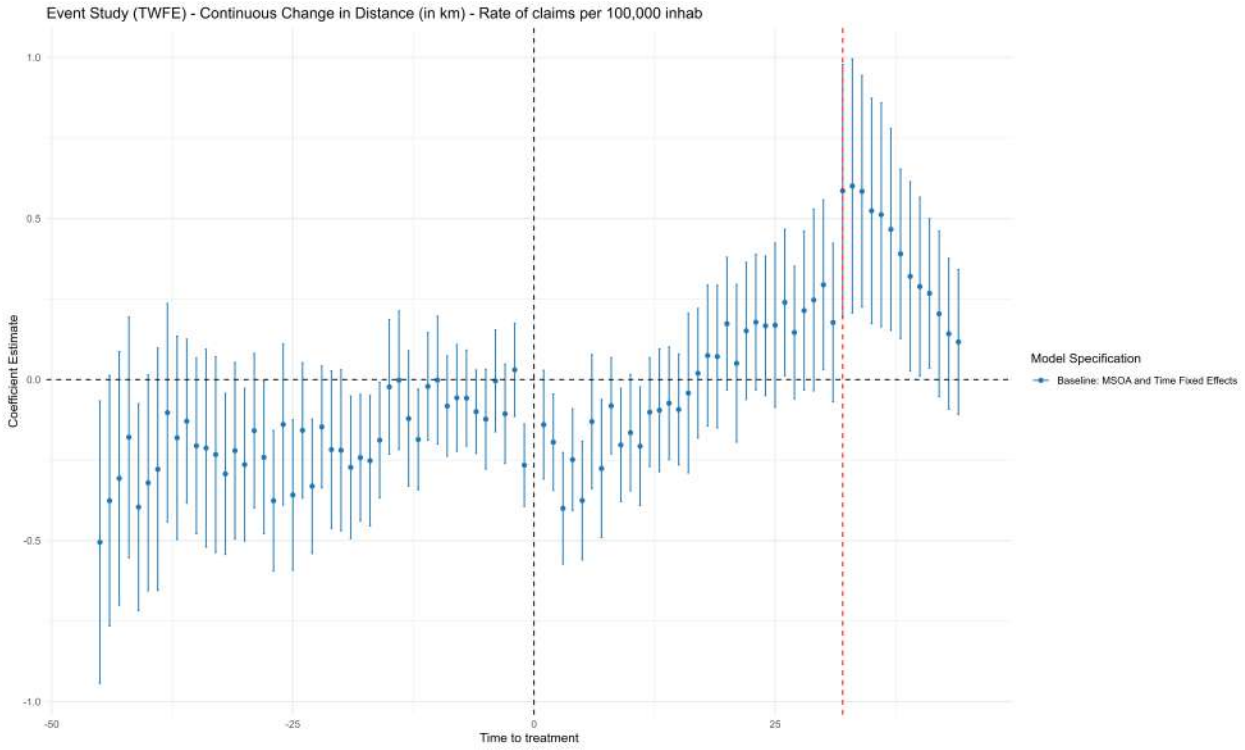


FIGURE 37: Eviction claim rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

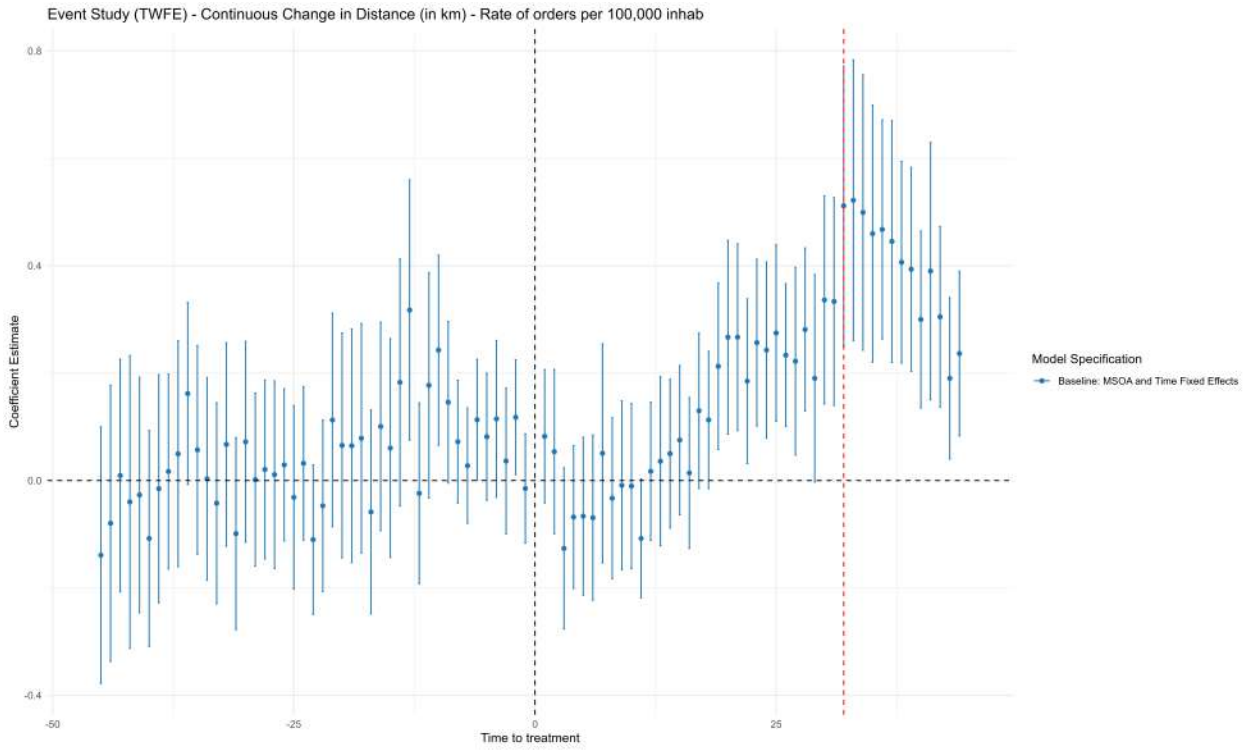


FIGURE 38: Eviction orders per 100,000 inhabitants - Static - Continuous Change in Distance in km

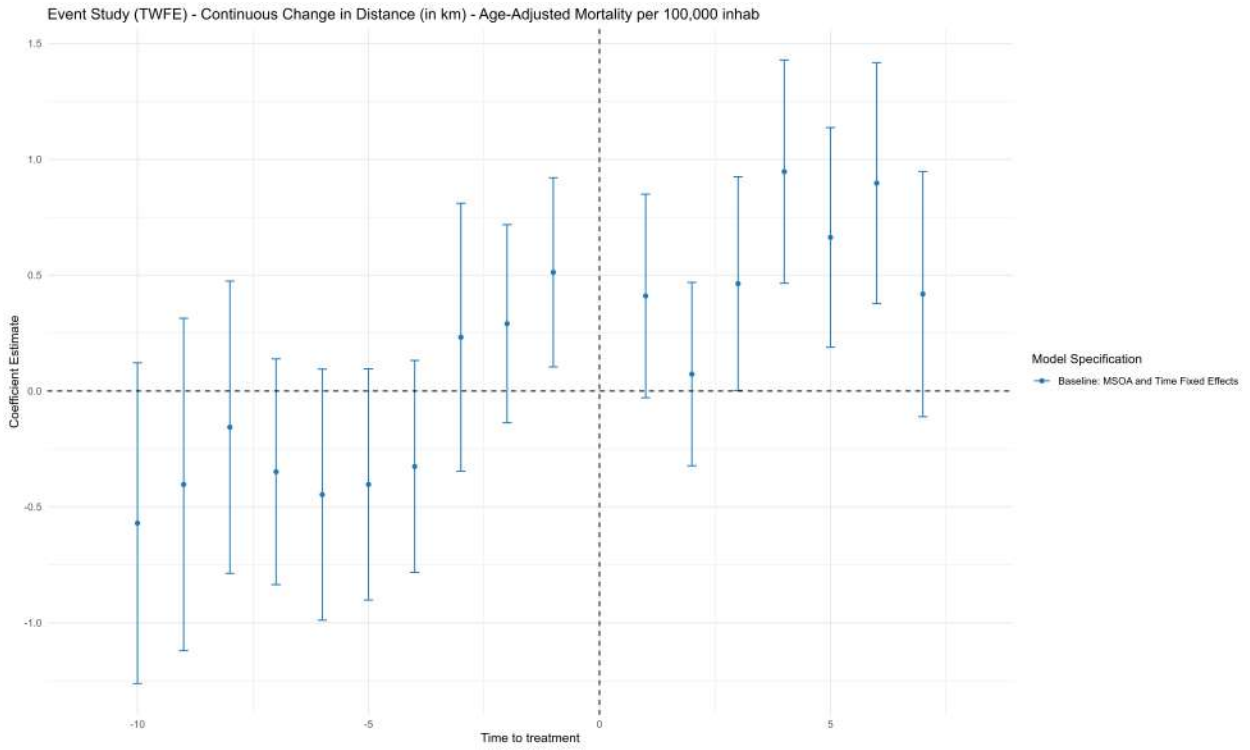


FIGURE 39: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

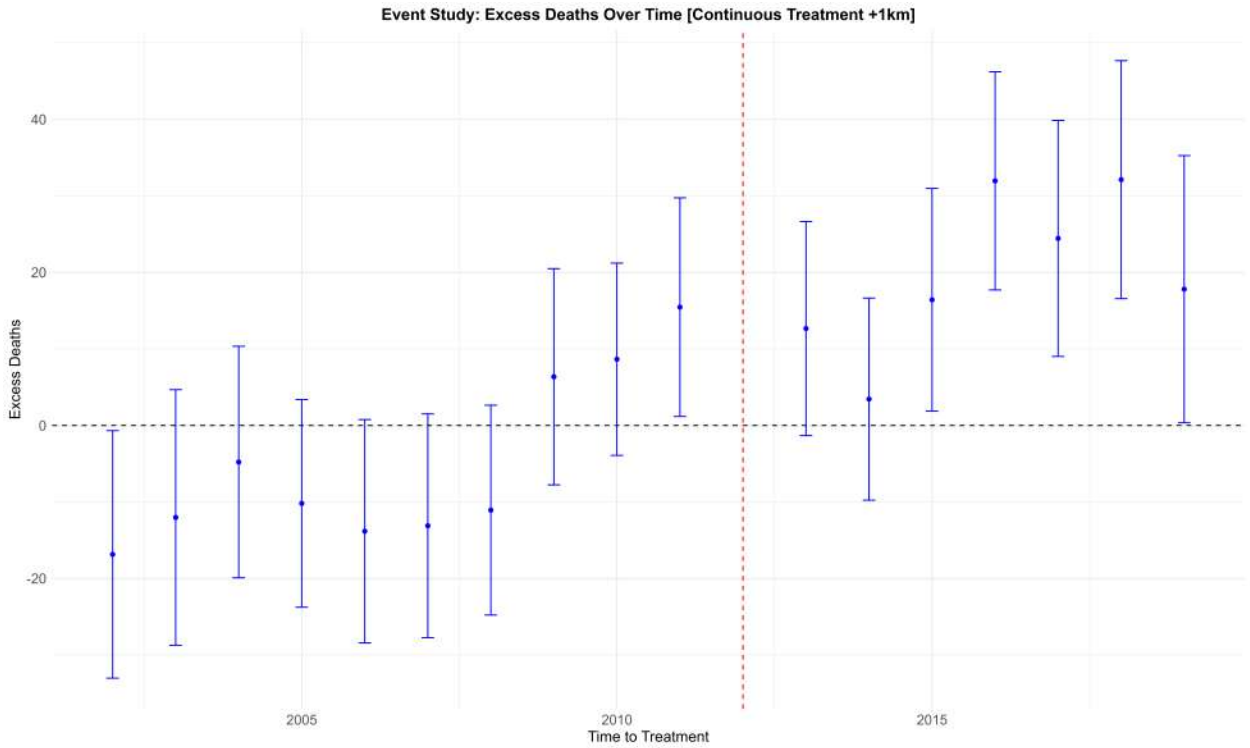


FIGURE 40: Excess deaths - Static - Continuous Change in Distance

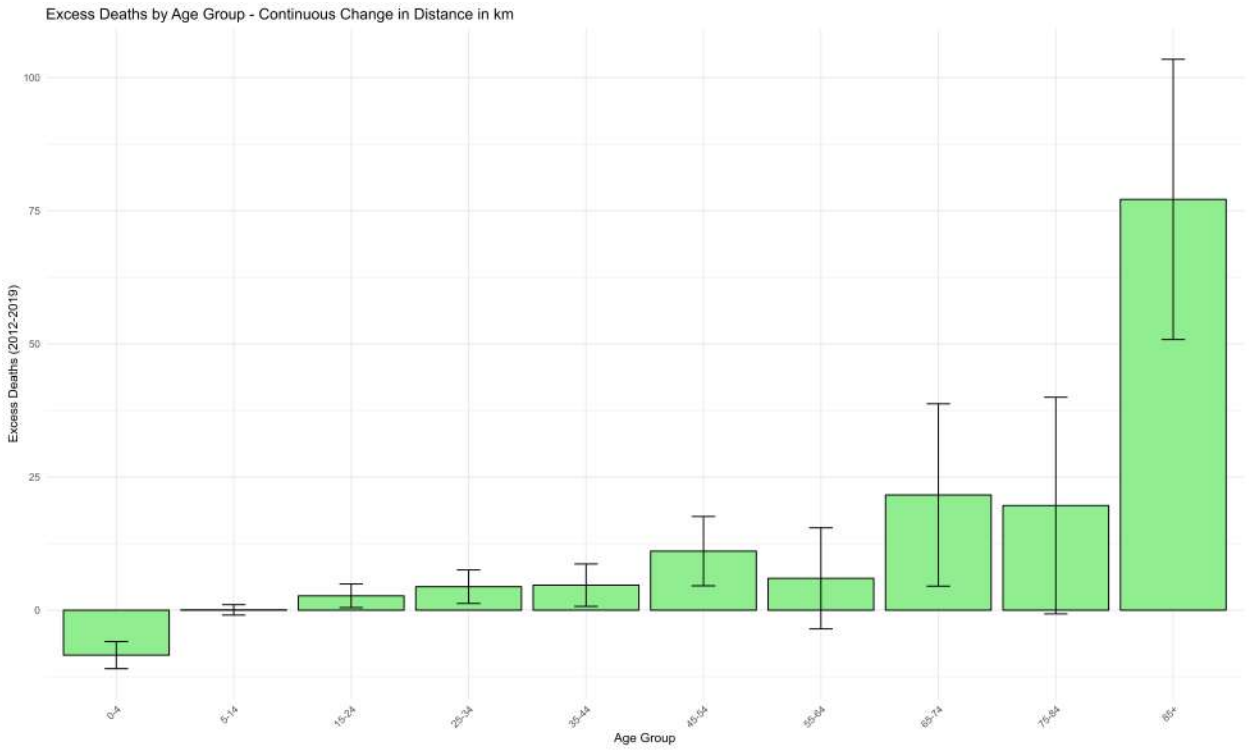


FIGURE 41: Pooled ATT Age-Adjusted mortality by Age Group - Continuous Change in Distance

D.1.5 With Local Authority Fixed Effects

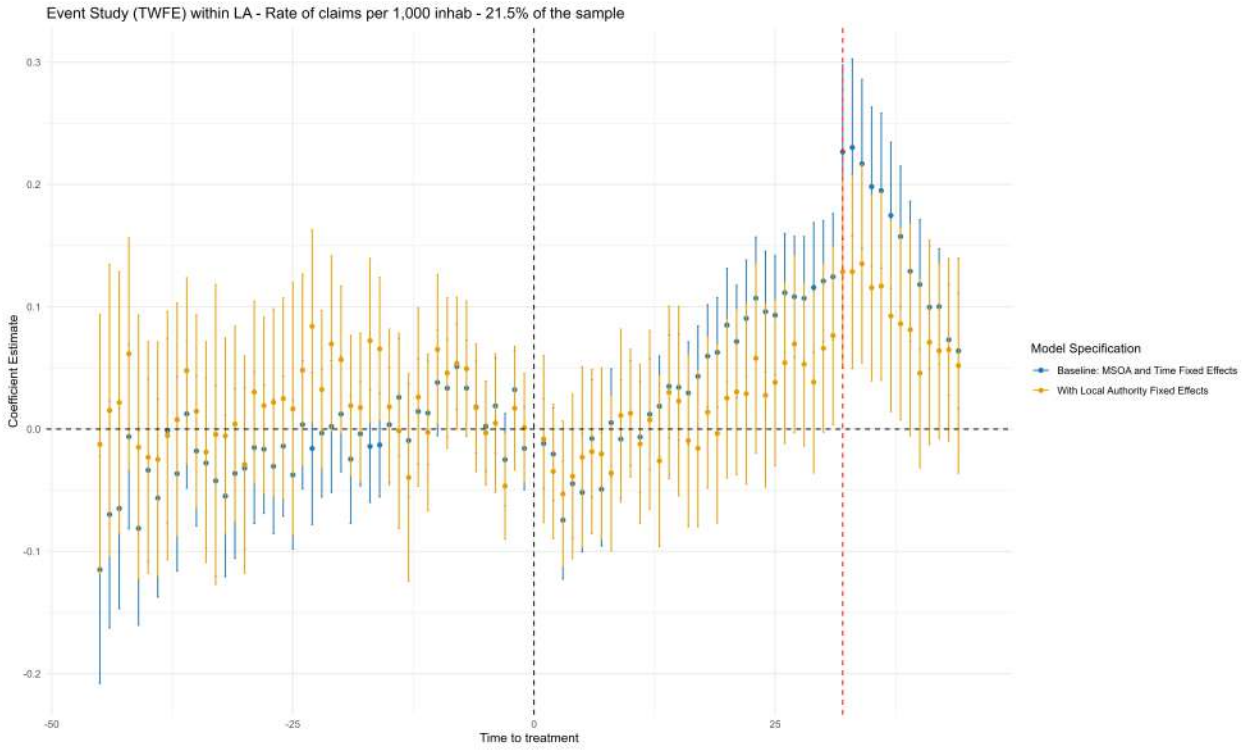


FIGURE 42: Event-study - Eviction Claims - With/without Local Authority Fixed Effects

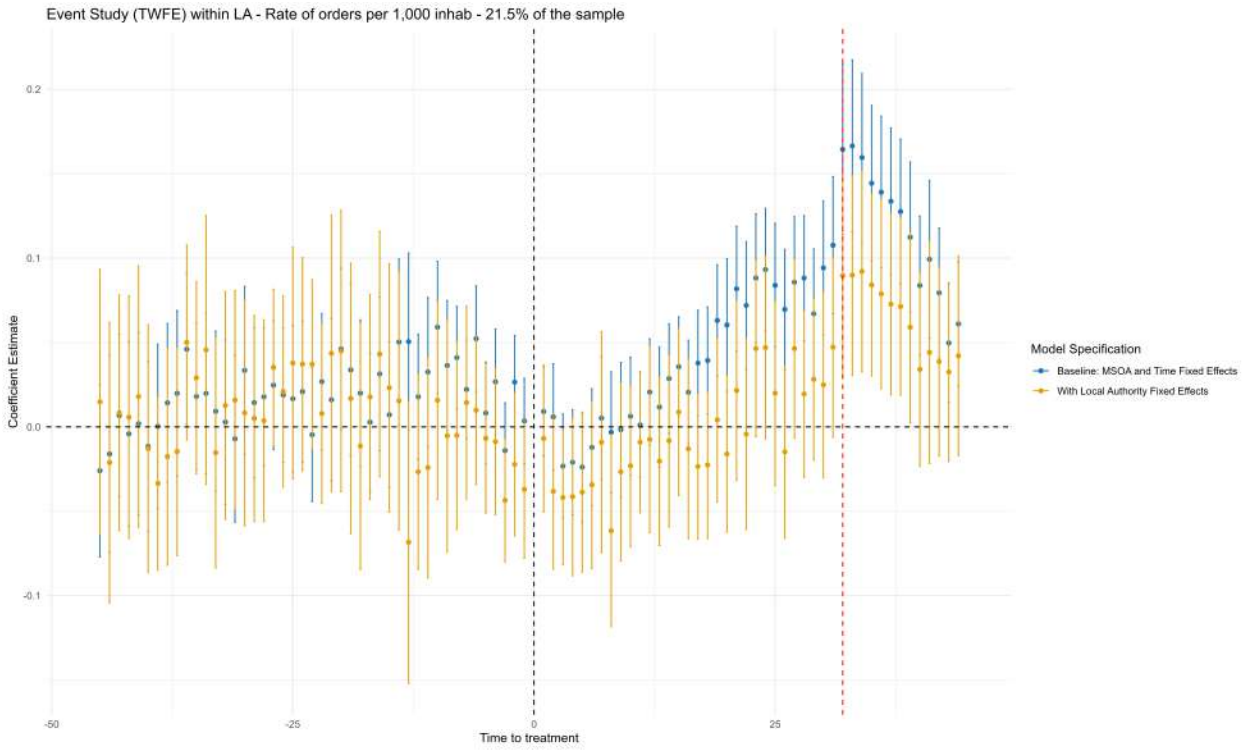


FIGURE 43: Event-study - Eviction Orders - With/without Local Authority Fixed Effects

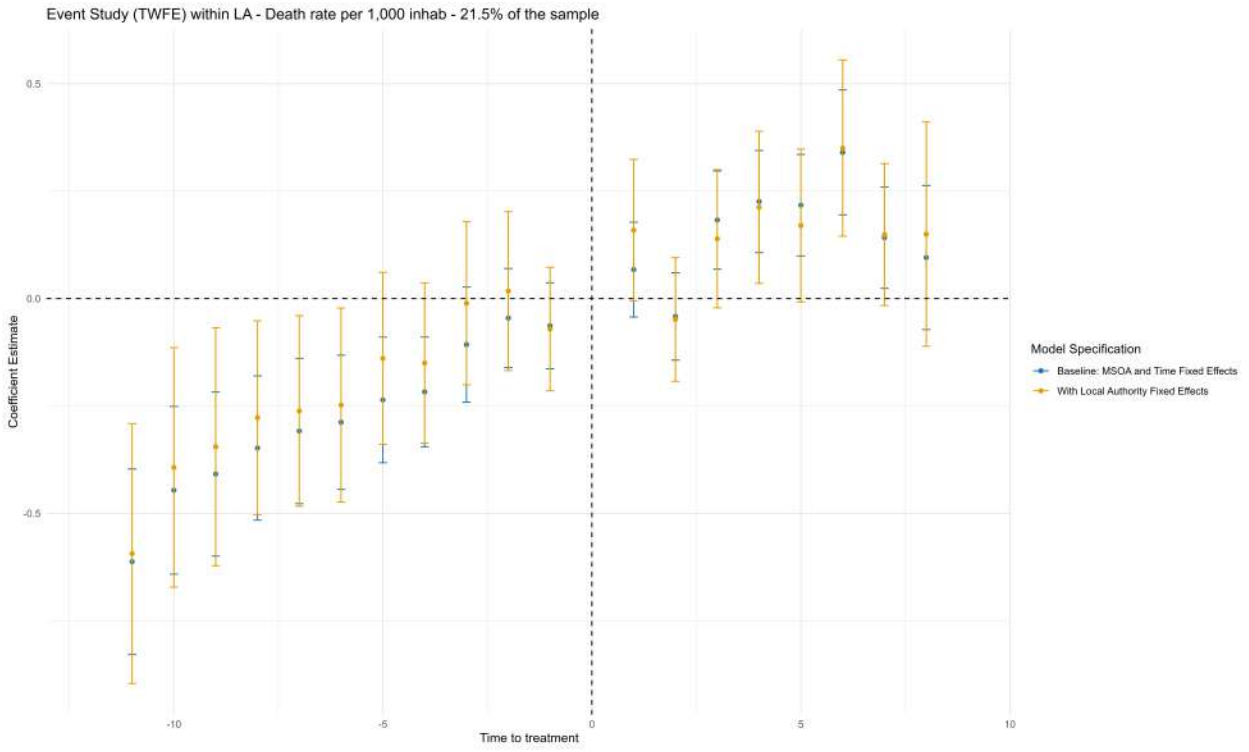


FIGURE 44: Event-study - Mortality - With/without Local Authority Fixed Effects

D.1.6 Overall Average Treatment on the Treated based on event-study aggregation [Binary Definition Change in Distance > 1.9km]

TABLE 17: 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 18: 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 19: 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 20: 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.2 Staggered DiD

D.2.1 Event-studies

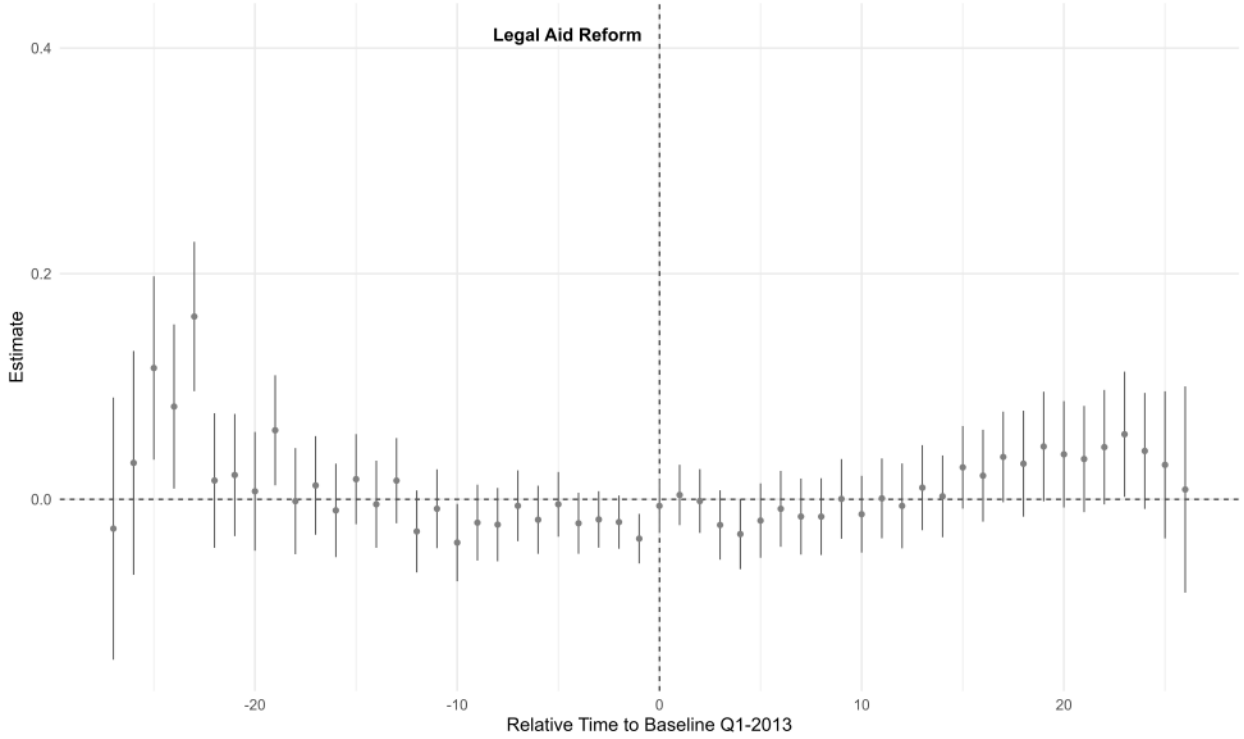


FIGURE 45: Eviction claim rate per 1,000 inhabitants - Staggered

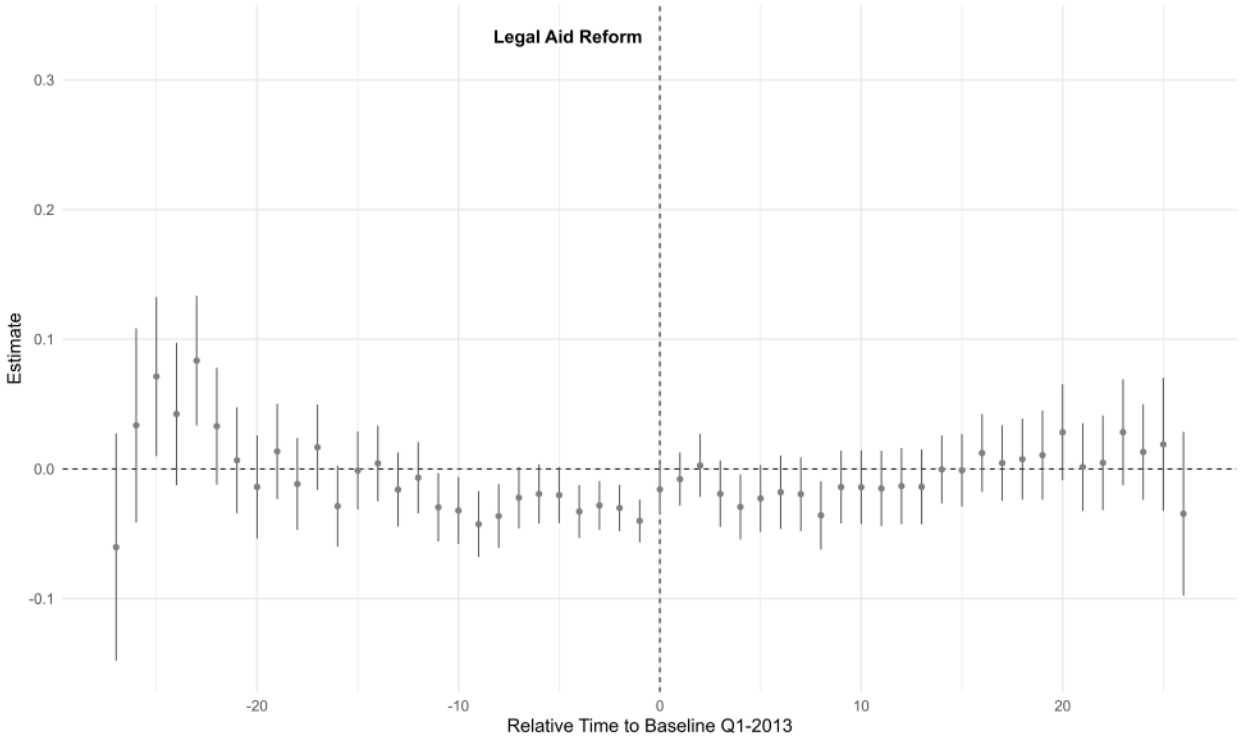


FIGURE 46: Eviction orders per 1,000 inhabitants - Staggered

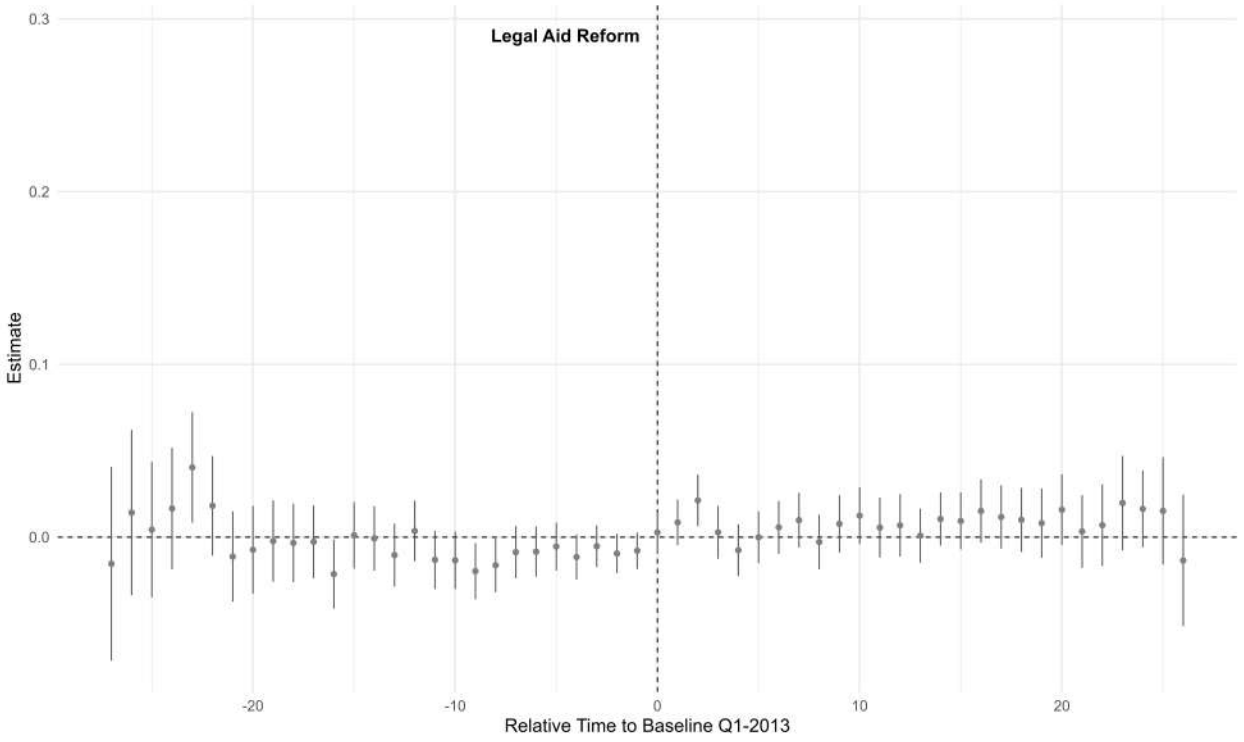


FIGURE 47: Eviction Suspended Possession orders per 1,000 inhabitants - Staggered

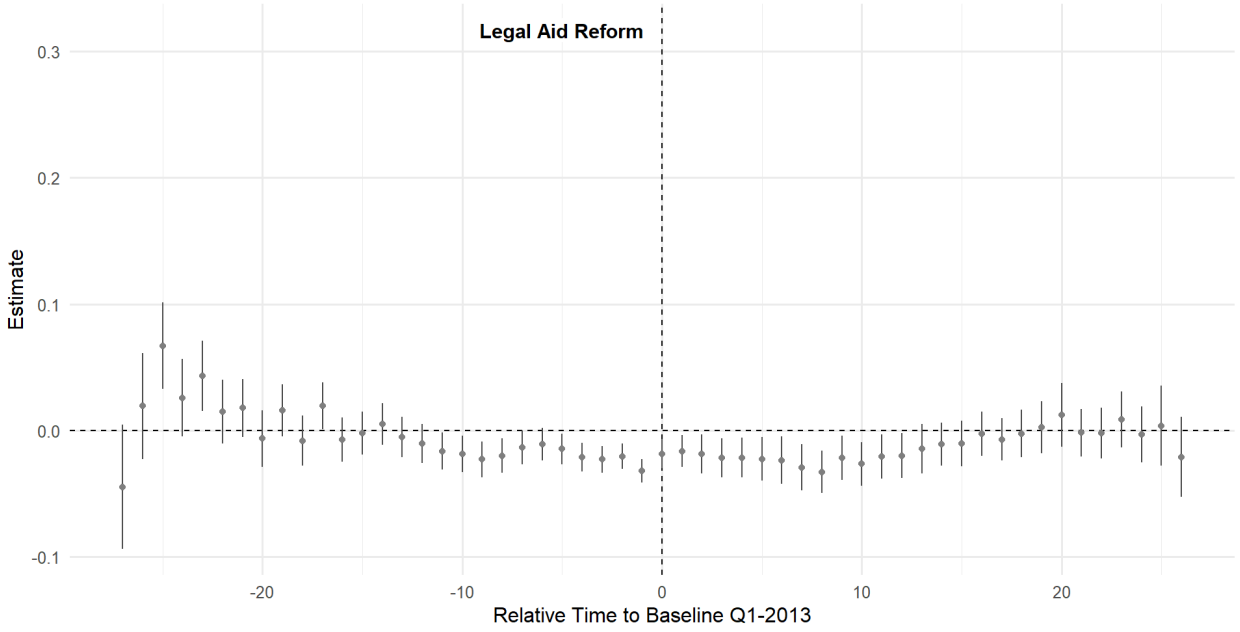


FIGURE 48: Eviction Outright orders per 1,000 inhabitants - Staggered

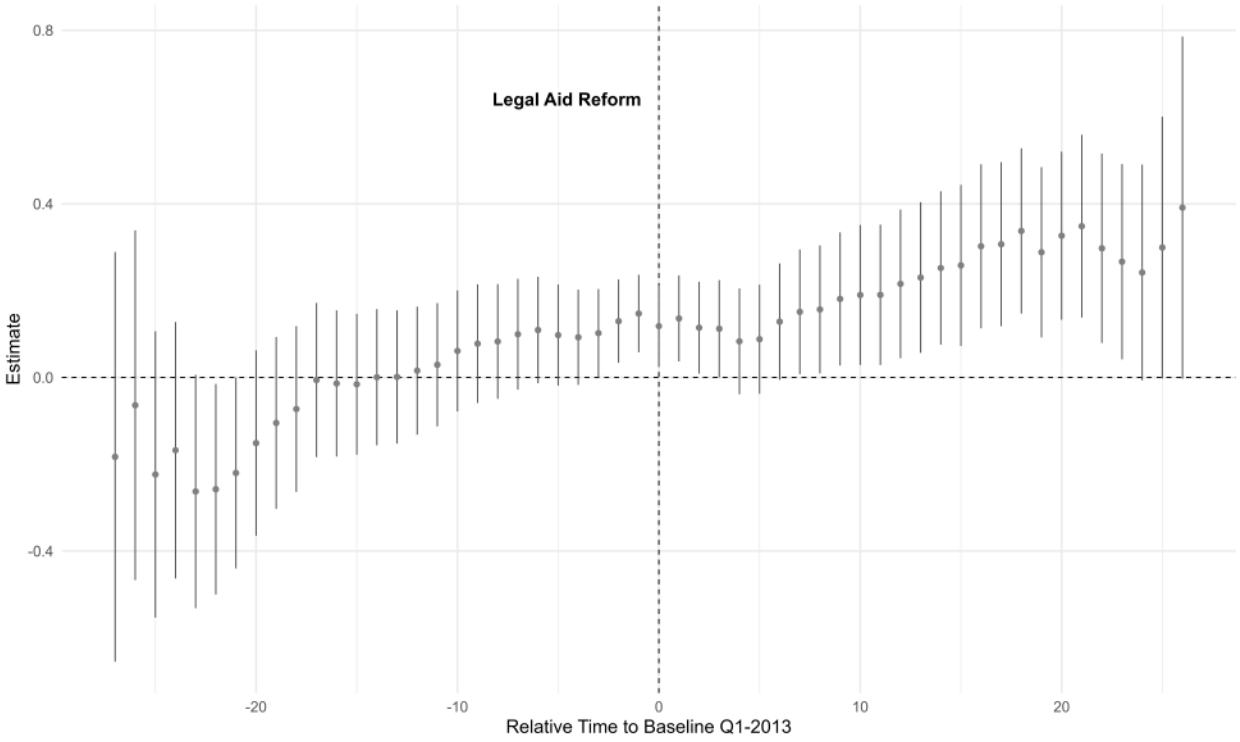


FIGURE 49: Mortality rate per 1,000 inhabitants - Staggered

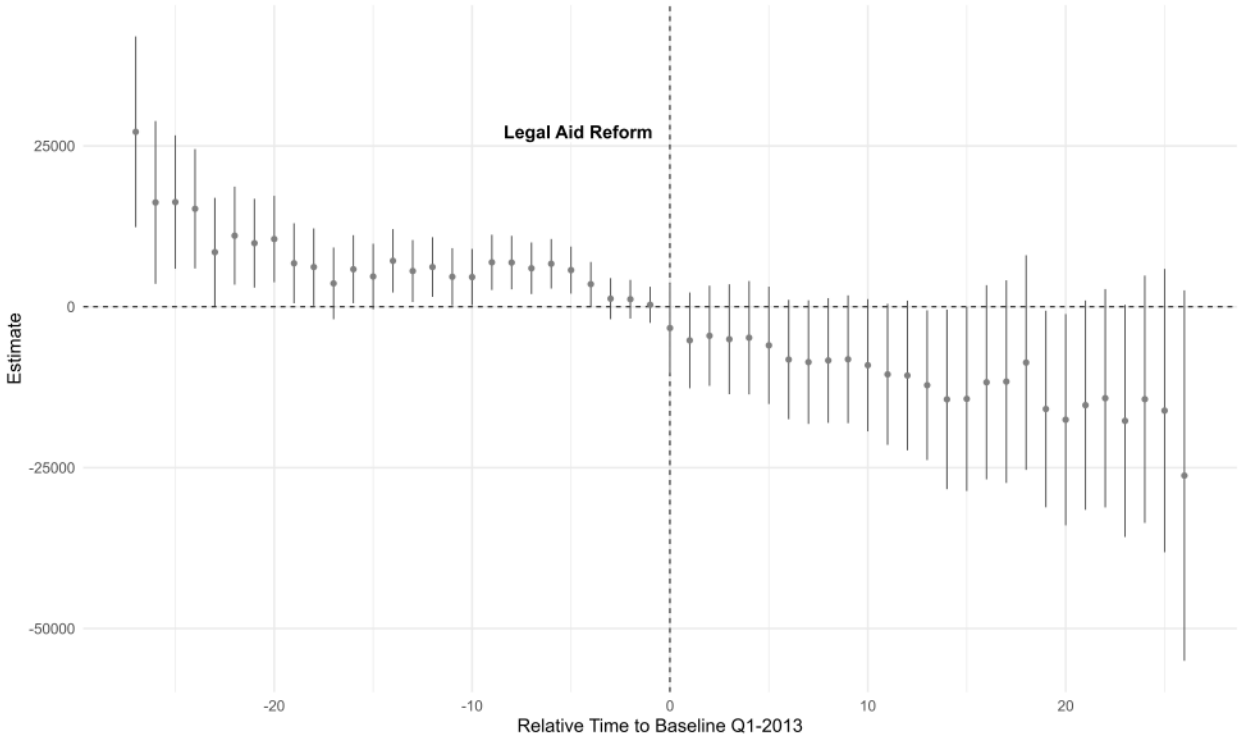


FIGURE 50: Mean House prices - Staggered

D.2.2 Cohort-Time Average Treatment on the Treated

TABLE 21: 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 22: 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 23: 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 24: 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.3 Bartik IV

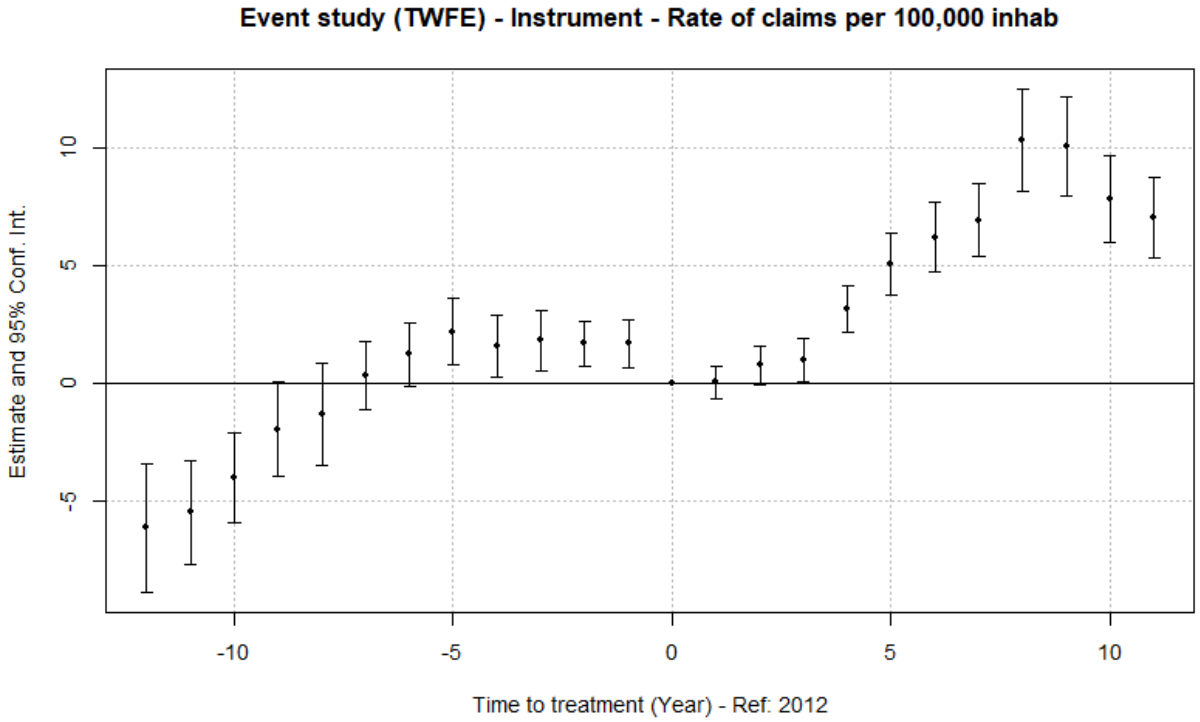


FIGURE 51: EVcition claim rate per 100,000 inhabitants - IV Bartik

E Appendix

E.1 Background Information

E.1.1 Timeline

TABLE 25: 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)

E.1.2 Impact Assessment of the Reform

TABLE 26: 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

Area of Law	Legal Help		Civil Representation	
	Expected	Actual	Expected	Actual
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 proc.	–	–	-53	-79
Public law	-14	26	-1	-39
Welfare benefits	-98	-99	0	-40
Combined family proceedings	–	–	-2	-100

Source: 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 27: 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

Area of Law	Legal Help		Civil Representation	
	Expected	Actual	Expected	Actual
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 proc.	–	–	-70	-79
Public law	-16	26	-1	-38
Welfare benefits	-97	-99	0	-50
Combined family proceedings	–	–	0	-100

Source: 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.1.3 Eviction process in the county courts of England and Wales

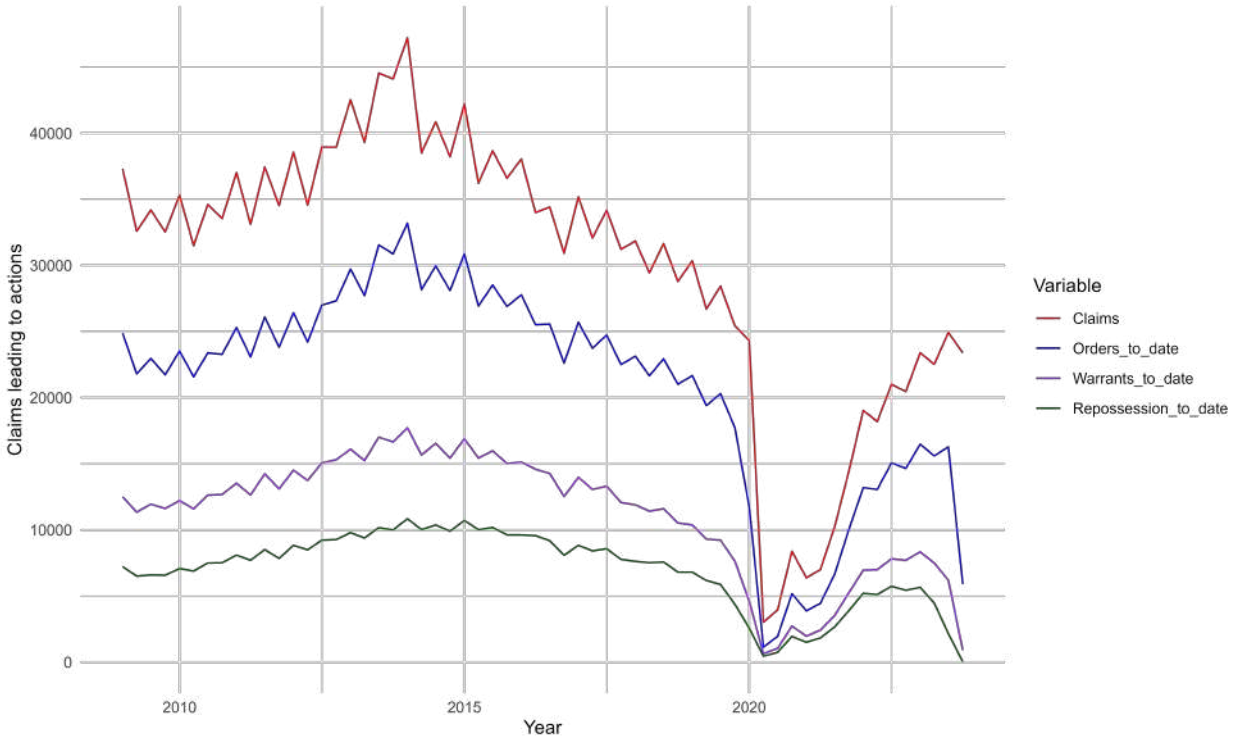


FIGURE 52: Number of possession claims that lead to orders, warrants and repossessions over time

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.

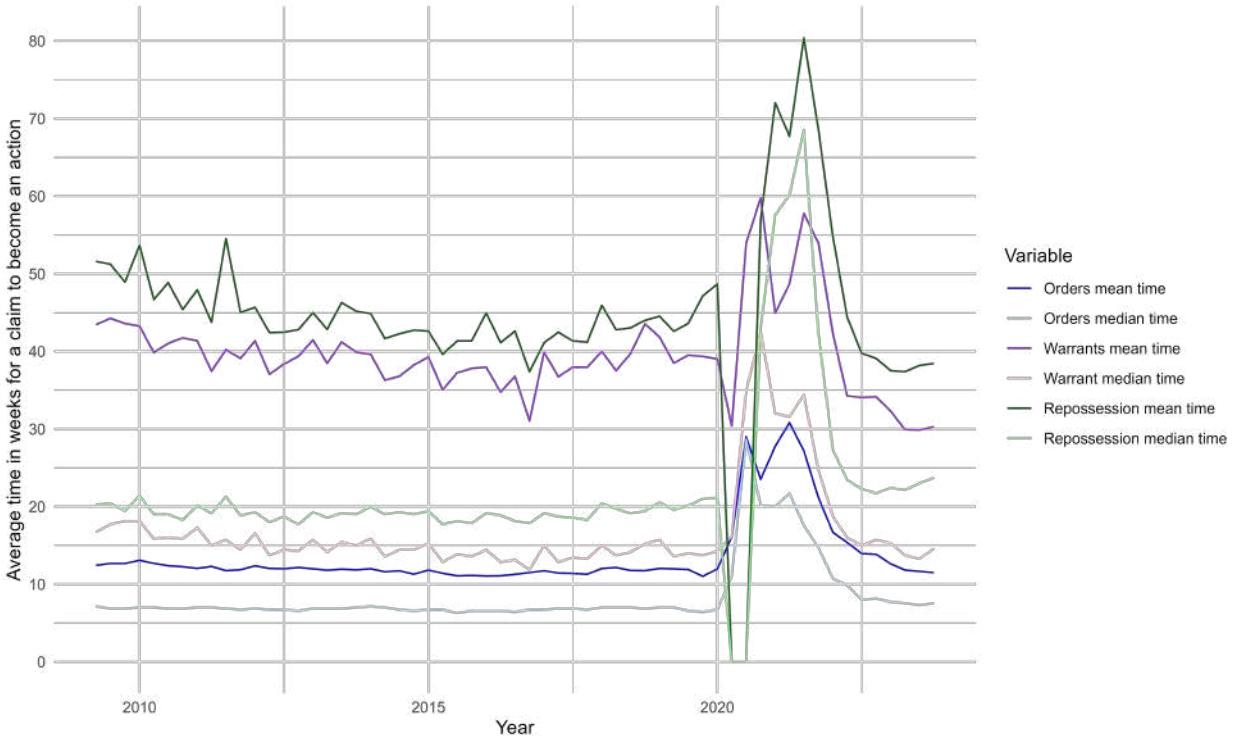


FIGURE 53: Average time for possession claims to become an order, warrant and repossession over time

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.

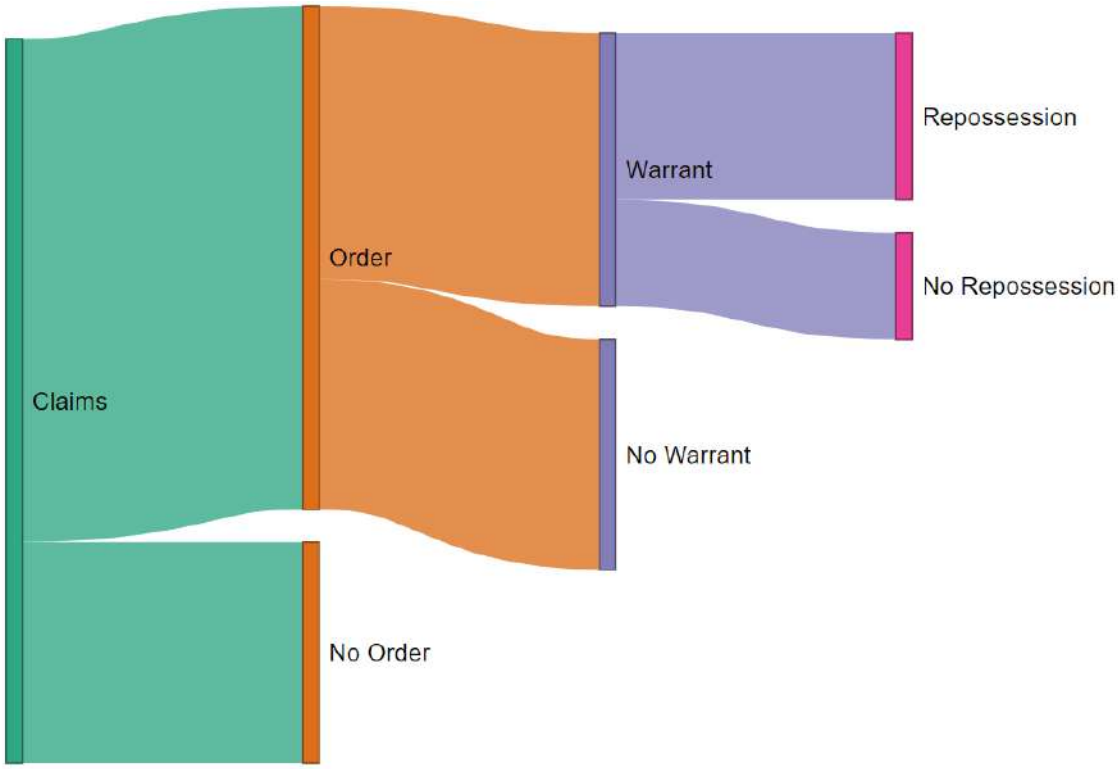


FIGURE 54: Flowchart eviction proceedings outcomes

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

E.2 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.3 Evolution of the number of cases and providers by area of law

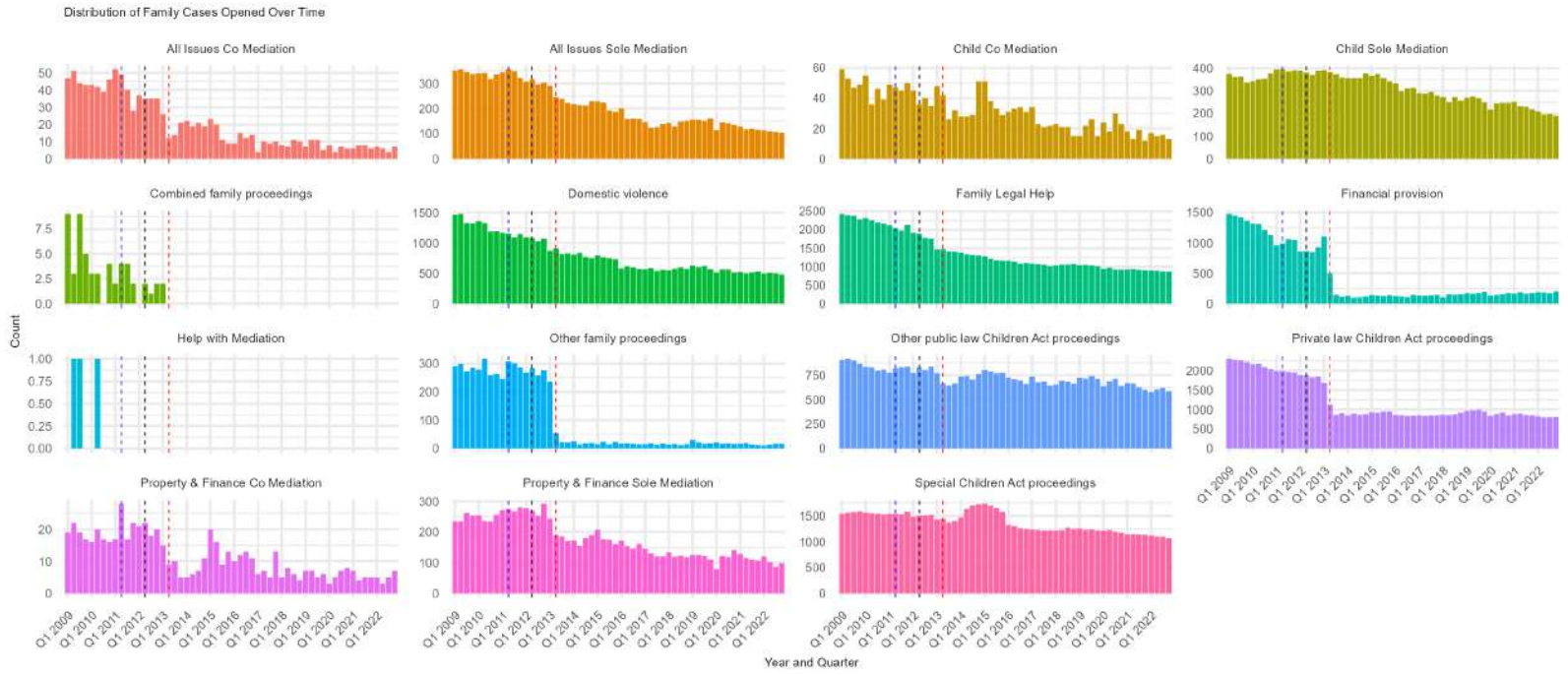


FIGURE 55: Trends in Legal Aid Cases Opened Each Quarter in Family Law, by Area of Law

Source: Ministry of Justice Legal Aid Statistics.
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 classification listed in the **??** pages 48-49.

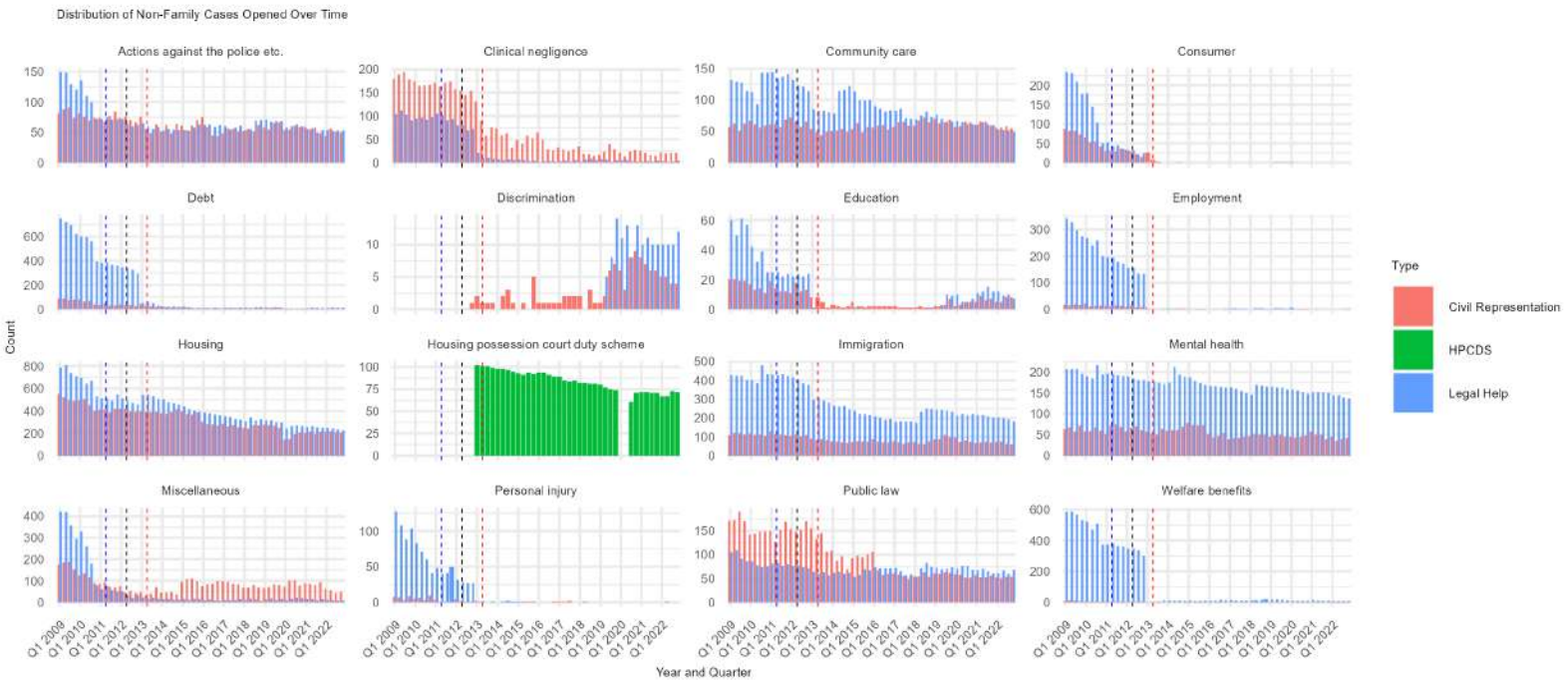


FIGURE 56: Trends in Legal Aid Cases Opened Each Quarter in Non-Family Civil Law, by Area of Law and Type of Assistance

Source: Ministry of Justice Legal Aid Statistics.
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.

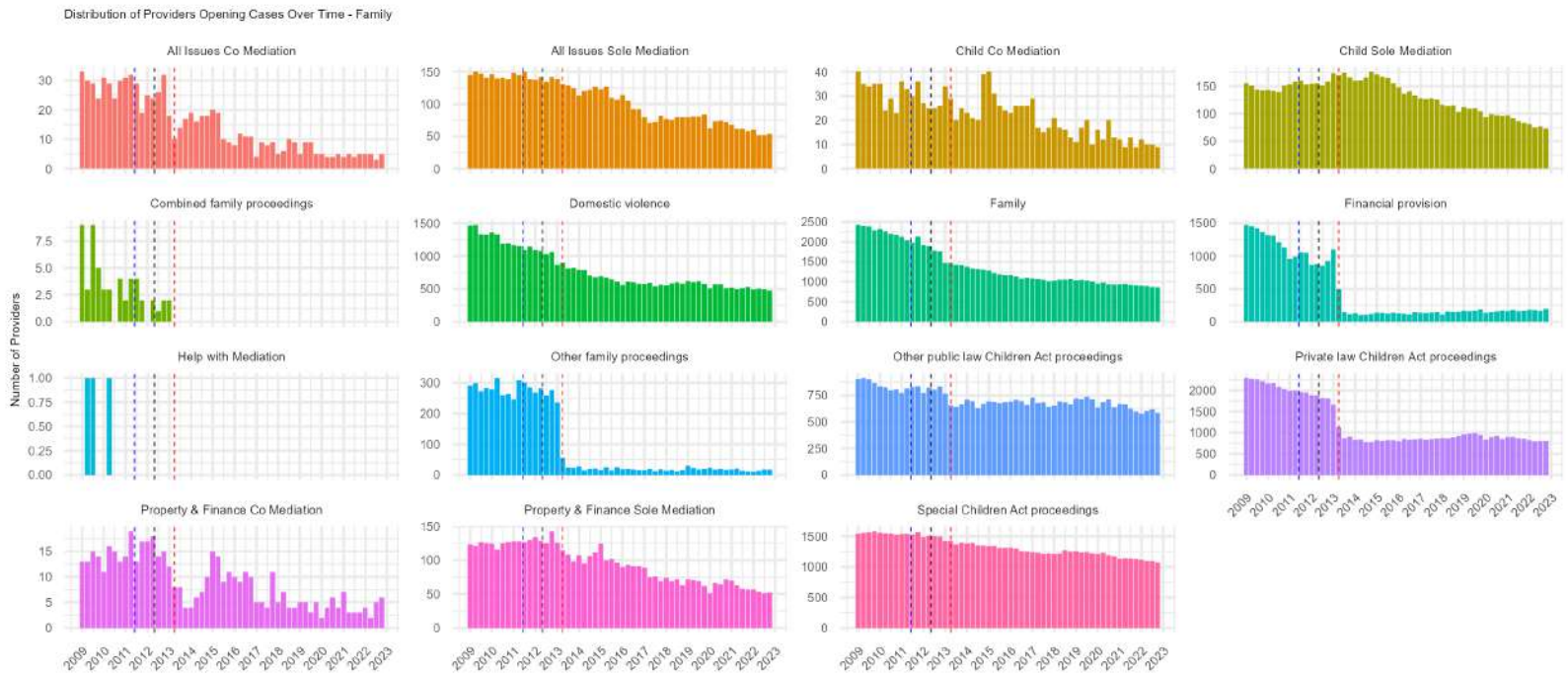


FIGURE 57: Number of Legal Aid Providers Opening Cases Each Quarter in Family Law, by Area of Law

Source: Ministry of Justice Legal Aid Statistics.
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.

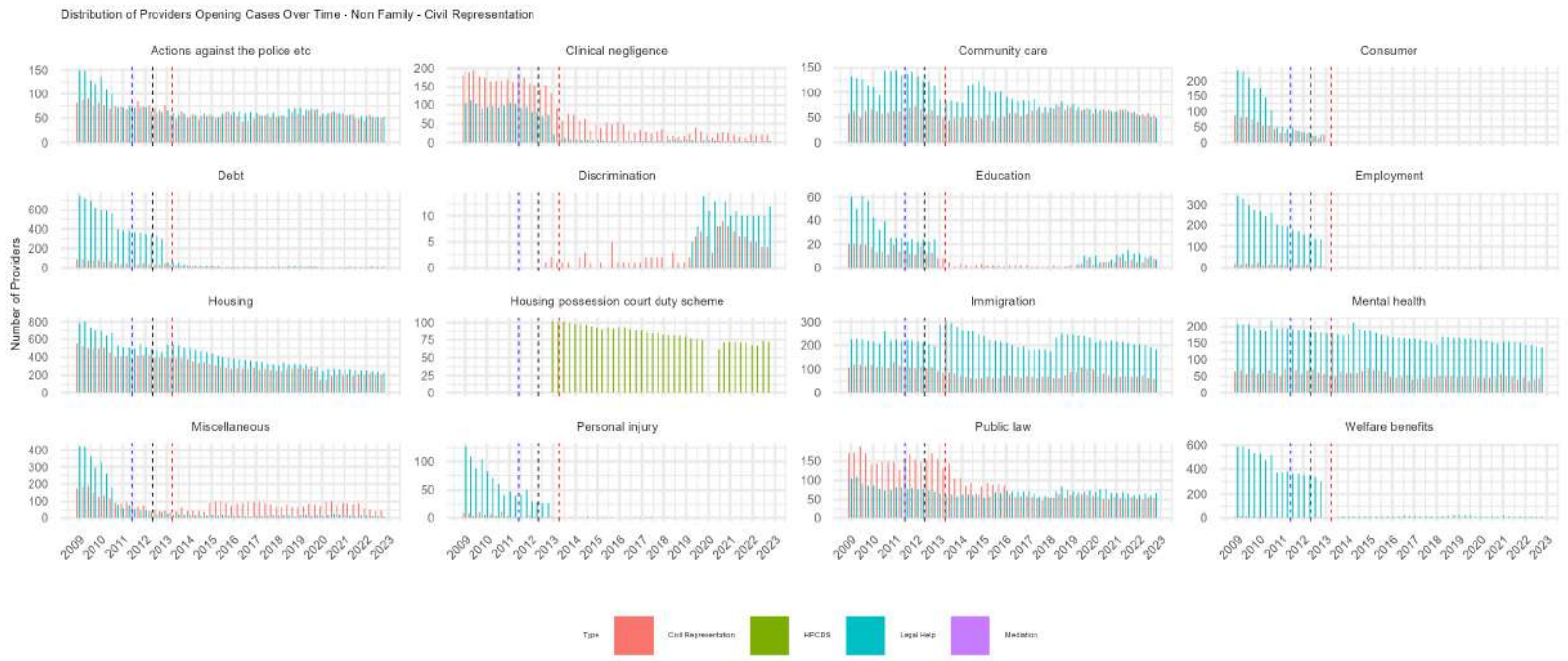


FIGURE 58: Number of Legal Aid Providers Opening Cases Each Quarter in Non-Family Civil Law, by Area of Law and Type of Assistance

Source: Ministry of Justice Legal Aid Statistics.
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.