The Impacts of Reduced Access to Legal Assistance: Evidence from England and Wales

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Abstract

In 2013, England and Wales implemented a sweeping legal aid reform that drastically reduced publicly funded legal assistance for low-income households facing social welfare issues. The 80% funding cut led to uneven provider closures and increased congestion, restricting legal assistance to immediate court actions while eliminating support for early interventions. This paper examines the reform's impact on access to justice and socioeconomic outcomes for vulnerable populations. Using panel data on legal assistance providers between 2011 and 2022, we adopt a difference-in-differences approach to assess its effects on legal aid availability, eviction and debt court cases, housing market tension, healthcare services use, and mortality. Our analysis leverages the exogenous spatial and temporal variation in access to legal assistance providers, considering changes in distance and congestion. Accounting for TWFE limitations, we quantify the cumulative impact of reduced access to free, in-person legal assistance on outcomes with lasting socioeconomic implications. Preliminary findings suggest that the legal aid cuts resulted in an average increase of 3.2 km in distance to the nearest provider. This reduced access led to localized increases in eviction filings and rates, as well as higher mortality over the decade. This study sheds light on an overlooked intervention targeting households at risk of homelessness and over-indebtedness. It provides empirical insights into the unintended socioeconomic and public health consequences of cutting legal aid programs following recessions.

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1 Introduction

Legal assistance programs have traditionally focused on criminal law but have expanded to civil legal issues in many jurisdictions. In the United States, an increasing number of cities have started to subsidize legal representation for tenants facing eviction Roumiantseva (2022). By contrast, the United Kingdom developed a more comprehensive system of publicly funded legal assistance shortly after World War II. Initially designed to support veterans seeking to divorce, the program evolved into a means-tested system encompassing all areas of law. Until a recent reform, it provided low-income households with advice and representation funded by the state to help them navigate the complexities of the everyday justice system.

Despite the prominence of legal aid programs, little is known about their welfare and public health impacts, and even less about the consequences of restricting their scope and eligibility. Recent surveys have revealed widespread unmet needs for legal assistance globally World Justice Project (2019), but robust evaluations of restricted access remain scarce. Emerging empirical work, particularly in the United States, has started exploring the benefits of expanding legal assistance in eviction contexts. This paper shifts the focus to the opposite scenario: what happens when the provision of civil legal assistance is drastically reduced across multiple areas of law. Using the case of England and Wales' 2013 legal aid reform, this study provides new national-level evidence on the consequences of such a policy change. Specifically, it examines how reduced access to legal assistance for disputes in social welfare law influenced court outcomes, housing market tensions, healthcare services use, and mortality rates.

In response to the global financial crisis, the UK implemented austerity measures in the early 2010s, significantly cutting public spending across various sectors, including justice and local government services. The 2013 reform sharply reduced publicly funded legal aid for civil cases, restricting access to cases involving immediate risks to health or housing. It also eliminated funding for early advice interventions which previously helped resolve legal issues before they escalated to court proceedings. Legal aid, which once provided low-income households with state-funded advice and representation through-

out their legal journeys regardless of the legal issue, was drastically scaled back.

The reform caused a 70% drop in the volume of legal aid cases, resulting in an 80% funding shock to legal aid providers. Applied uniformly, the reform ignored regional variations in local providers' capacity to absorb the shock. This caused unintended closures, halving the number of providers nationwide and creating congestion among the remaining ones. It led to spatial and temporal disparities in access to subsidized, in-person legal assistance for cases still covered by the scheme. As a result, access to legal assistance for in-scope cases, such as evictions, became increasingly uneven across the country.

In this paper, we adopt a generalised difference-in-difference approach to document the dynamic causal effects of this reduced access to legal assistance. 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 2022, 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 adopt two empirical strategies. First, we exploit the spatial source of variation. We measure changes in distance to nearest legal aid providers in the two years following the reform. We estimate the reform's effects by comparing the most affected areas to areas that were little to no affected in terms of interruption of their local services. We follow Callaway and Sant'Anna (2021) to estimate treatment effects robust to heterogeneity across groups. Next, we exploit the overtime source of variation. We account for the staggered interruption of legal services the reform generated across the country, which resulted in differential timing of increases in distance to the nearest provider. We follow Borusyak et al. (2021) to retrieve imputation-based estimators robust to treatment heterogeneity over time. We combine the two sources of variation by computing the estimators of de Chaisemartin and D'Haultfœuille (2020) which are robust to both types of hetero-

geneity.

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 and house prices. 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 and decreased house prices, compared to areas that did not experience any change. However, the effects previously observed on 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 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 (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), Uraz (2024)) and can impact welfare participation over the long-run **?**. 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 behaviors (Fetzer (2019)) and mortality (Berman and Hovland (2024)).

This paper is structured as follows. Section 2 provides the institutional context behind the provision of legal aid services in England and Wales and the changes introduced by the 2013 austerity 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 two 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), 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

To measure the change in access to legal assistance, we measure distances to the nearest legal aid provider over time. We use the Ministry of Justice's legal aid statistics, which provide data on the number of cases open and completed every quarter by legal aid providers. This data, released annually as open source, covers providers that contracted with the Ministry of Justice to undertake legal aid work at some point since 2009. However, it does not capture the universe of organisations providing legal support. Specifically, it excludes not-for-profit organisations that have not taken any legal aid work since 2009, despite potentially providing other types of legal services to low-income households. In addition, the data does not report the activity of organizations that previously undertook legal aid work, ceased to do so after LASPO was implemented, yet remained otherwise active.

At the time of writing, there are no comprehensive time series available on the activity of advice providers that operate outside of the legal aid scheme. Existing literature (Society (2017); Wilding (2021)) relies exclusively on the aforementioned source and is restricted to legal aid providers exclusively. To address this limitation, we are currently constructing a database to account for the locations of all organisations involved in providing legal

support to low-income households. To do so, we use data from the Charity Commissioner and the Company House, as well as data provided by civil society actors, to identify the dates of operations of advice providers that hold quality standards. As a first approach, we discard this limitation and consider that providers who do not take legal aid work are imperfect substitutes for the type of assistance that was previously available under the legal aid scheme. We also cluster the analysis at the local authority level to mitigate this concern (see Section 4).

The Ministry of Justice statistics provide information on the postcode of legal aid providers and the area(s) of law for which they undertake legal aid. We map all legal aid providers in England and Wales based on their geographical location and assign them to their respective Middle Super Layer Output (MSOA) areas, using the 2021 Census boundaries. We use the 2021 boundaries because that is the only scale at which court outcomes are available. Whenever other data is based on the 2001 or 2011 Census boundaries, we use the Office for National Statistics official lookup files to assign the respective boundaries. Where several boundaries overlap, for instance, several MSOA based on the 2021 Census correspond to only one MSOA based on the 2011 census, and respectively, we take the average of values for these units. In the 2021 Census, there are 7,264 MSOAs in England and Wales. In 2011, there are 7,201 MSOAs.

For every legal aid provider, we compute the date at which they stop being active. A provider is considered to cease being active at time t if they do not take on new legal aid cases for all subsequent quarters. We restrict our sample to providers that take at least one case in one of the following areas of *social welfare laws*: housing, debt or welfare benefits. In the Appendix, we explore other definitions of the sample, considering providers that do at least one case in one of the **non-family law** and providers that all do **housing law** cases. So-defined housing providers may also cover other areas of laws, but they have at least one case in housing law at some point during the period for which they are considered active. Non-family law providers cover cases in: housing, debt, welfare benefits, clinical negligence, education, consumer, community care, personal injury, public law, mental health, employment and discrimination laws. Table 2 illustrates that the treatment variables we define in section 4 are especially sensitive to the inclusion of legal aid

providers beyond so-defined social welfare laws. We further explore this characteristic of our data in a Bartik shift-share instrument setting.

To measure distance, we use the *st_distance* function from the *sf* R package which calculates the Euclidian straight-line distance between two points in Cartesian coordinates. We take the distance between each MSOA's population-weighted centroid and the nearest legal services provider. We allow the nearest provider to be outside of the MSOA but impose that it is located within the same local authority to account for residency tests and local-authority-specific dynamics of assistance provision. When there is no active provider within the same local authority, we take the nearest provider regardless of administrative boundaries. As a robustness check, we will use the *georoute* module developed by Weber and Peclat (2016) which estimates the distance and travel time between two addresses. We also plan to test the sensitivity of our estimates with alternative measures of distance specifically designed to account for spatial (in)accessibility as in Page et al. (2018). This will allow us to account for providers clustering within metropolitan areas and disparities between rural and urban settings.

3.2 Court Outcomes

We consider the occurences 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 judgements. 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 instalment 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 47 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 instalments 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] Forthcoming
[4] Forthcoming
[5] Forthcoming

3.5 Housing Market Tensions Indices

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

Housing affordability: Housing affordability is proxied by houses prices related to the share of low-income households in the area. Average yearly house prices by MSOA are sourced from the House Price Statistics for Small Areas. [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* variation in access and consider areas that have experienced relatively little to no change in access after the reform as control groups and areas 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

[6] Forthcoming

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

(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 my estimates with heterogeneity-robust estimators following Callaway and Sant'Anna (2021).

In this strategy, our treatment definition is static: areas are treated or untreated based on the relative change in the distance they experienced over a given time frame. To classify areas between most affected and least affected, we first use the quintile distribution of the treatment when measuring the change in distance between Q2 2012 and Q1 2014. Figures 6 and 7 show that solely the first quintile is 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 a few areas experienced a decrease in distance, we exclude them and, instead, define untreated areas as areas that did not experience any change in distance between Q2 2012 and Q1 2014. Table 12 shows the distribution of the change in distance variable among my 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

[8] Forthcoming

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 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.

4.2 Estimation procedure

First, we implement my 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 operationalize 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 out-

comes 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 in time q. It can be decomposed between 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.

θ_t are time-fixed effects and δi are MSOA fixed effects. To rule out concerns of within-local authority serial correlation and overdispersion, I cluster my error terms $\epsilon_{i,g,t}$ at the local authority g level (Woolridge, 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.

4.3 Overtime Source of Variation: Staggered Treatment

4.3.1 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 my 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.[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 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

[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 $t-1$.

[11] We are currently investigating how to account for units that experience an additional "dose" in the

Figure 11 show the final composition of my sample.

4.3.2 Estimation procedure

The estimation is similar to Equation 4.2 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 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

treatment. As a first approach, we focus on the first increase in distance only.

enabling to breakdown cohort-average treatment effects along covariates (*upcoming*).

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 presented to the UK Parliament almost two years before being enacted and adopted eleven months before it came into force. As a consequence, providers may have anticipated it and started diversifying their funding sources to increase their resilience as soon as the reform was announced. This seems especially relevant in light of the slight pre-trends that one can detect in the 12 to 10 quarters ahead of Q2 2013 in the event-studies generated with a staggered definition of the treatment in section 5.2

Next steps To address concerns of anticipation, we are in the process backing up the analysis to the time at which the reform was announced instead of when it was effectively implemented. To guide the decision on which date is the most accurate, we are testing the sensitivity of the results according to different definitions of the pre/post-reform period. The event-study graphs generated with the staggered definition of the treatment suggest that the date of reform announcement may be more accurate than the date of adoption.

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 state the parallel trend assumption is the absence of confounding contemporaneous shocks, such as a policy change, that would affect treated and control areas differently, and affect the outcomes. we identified one of such concurring policy changes: the reform of the welfare benefits system and the replacement of six legacy benefits and tax credits by the introduction of Universal Credit (UC). The UC policy was first launched in 2013 in a few pilot areas and gradually rolled out starting in February 2015.

When UC was introduced in one area, the local demand for legal assistance to deal with benefits issues may have increased. At the same time, the local provision of legal services in these areas may have been affected in response (eg., some local authorities may have increased their funding to the local advice sector anticipating the surge in demand). We are currently testing whether migration to UC predicts changes in legal assistance provision in areas where it was first introduced. If this turns out to be a major caveat, we will test the sensitivity of the estimation to the exclusion of the 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

[12]Forthcoming

that the impact on outcomes for highly affected areas should be equal to the impact that would have been observed in little affected areas if they had been as much affected. Otherwise, TWFE estimates are biased. The use of distance to measure changes in access is reassuring to make such an assumption. Following Cunningham (2021), we argue that the timing and intensity of changes in access across areas appear arbitrary and unrelated to the impact they have had on residents.

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

[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.

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 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 indica-

5 Results

tors. 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.

5.1 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 20, 21, 22, 23, and 42 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 26, 27, and 28 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 29, 30, 31, and 32 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 37, 38, 41 and 42 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.

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. Firstly, the rise in the number of eviction claims could suggest a decline in the number of eviction cases settled outside of courts, contrary to policymakers' expectations. They had implemented the reform 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 most affected areas after the reform suggests that access to assistance lawyers facilitated compromises or assisted in moving out without resorting to formal eviction processes, where landlords file claims with the court.

Secondly, the increased number of eviction orders issued could be attributed to fewer cases where tenants had legal representation by a lawyer, or because tenants accessed legal representation too late in the process to avoid eviction orders. This notion 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 deemed unfavourable from a tenant's perspective, with Suspended Possession Orders being a commonly perceived 'least bad' outcome. These orders allow tenants to stay in the property as long as they meet certain conditions, but they still face the risk of facilitated eviction if they fail to comply. The event study 39 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 representative 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 most affected areas through several potential mechanisms. Firstly, the higher number of evicted households suggests a prevalence of increased housing insecurity. Formal eviction proceedings represent only a conservative estimate of the total number of people facing eviction prospects; many move out before court proceedings. Reduced access to legal assistance also implies a higher number of households navigating the legal journey alone, generating stress.

There is a body of evidence linking the experience of unresolved legal problems with poor physical and mental health outcomes. Studies indicate a bidirectional relationship between legal problems and health outcomes, with legal issues exacerbating health problems and vice versa. The World Justice Project estimated in 2018 that one in three people suffered from physical or mental health issues as a consequence of their legal problems. Legal needs surveys report a high level of stress, anxiety, depression, and physical health difficulties among households with social welfare legal problems (Organ and Sigafoos, 2018; YouGov, 2019). Moreover, legal problems have been shown to play a key role in exacerbating health inequalities Genn (2019).

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 lack of legal assistance means that there are more households without holistic support. Fieldwork suggests that when assisting a tenant with an eviction case, lawyers often extend beyond providing

strictly housing legal advice (Caspi and Rafkin (2024), Uraz (2024)). They may signpost households to healthcare providers, particularly when identifying disabilities or mental health issues. Additionally, they may check eligibility for public benefits or local welfare schemes, all of which have spillover effects on prospects for better health.
Preliminary findings suggest that mortality effects are concentrated among the elderly but this is currently under investigation 24, 32.

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) and Berman and Hovland (2024) to measure the impacts of austerity on voting behavior 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 19). Finally, the results hold when local authority fixed ef-

fects 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.

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.

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 public sector as externalities resulting from reduced legal aid, including courts costs from increased hearings, local authority costs for additional households in temporary accommodation, and NHS costs 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 (eg. 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.

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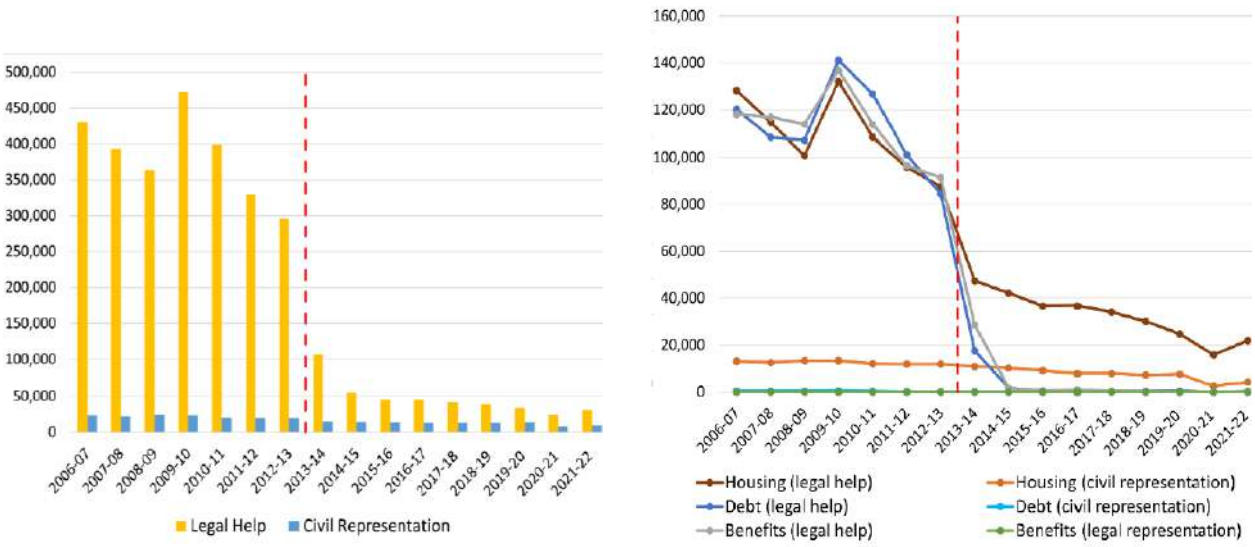
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A Figures

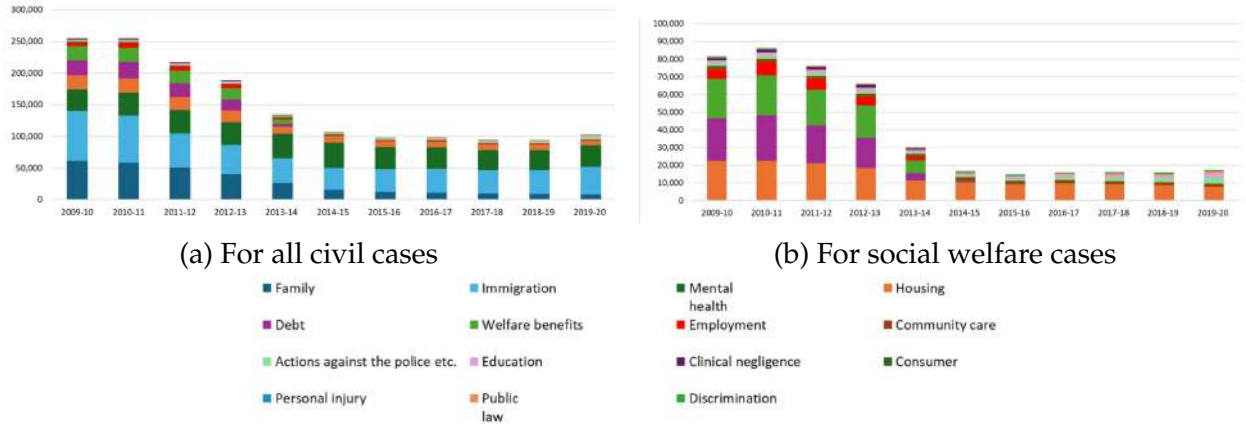


(a) By type of legal aid work

(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.

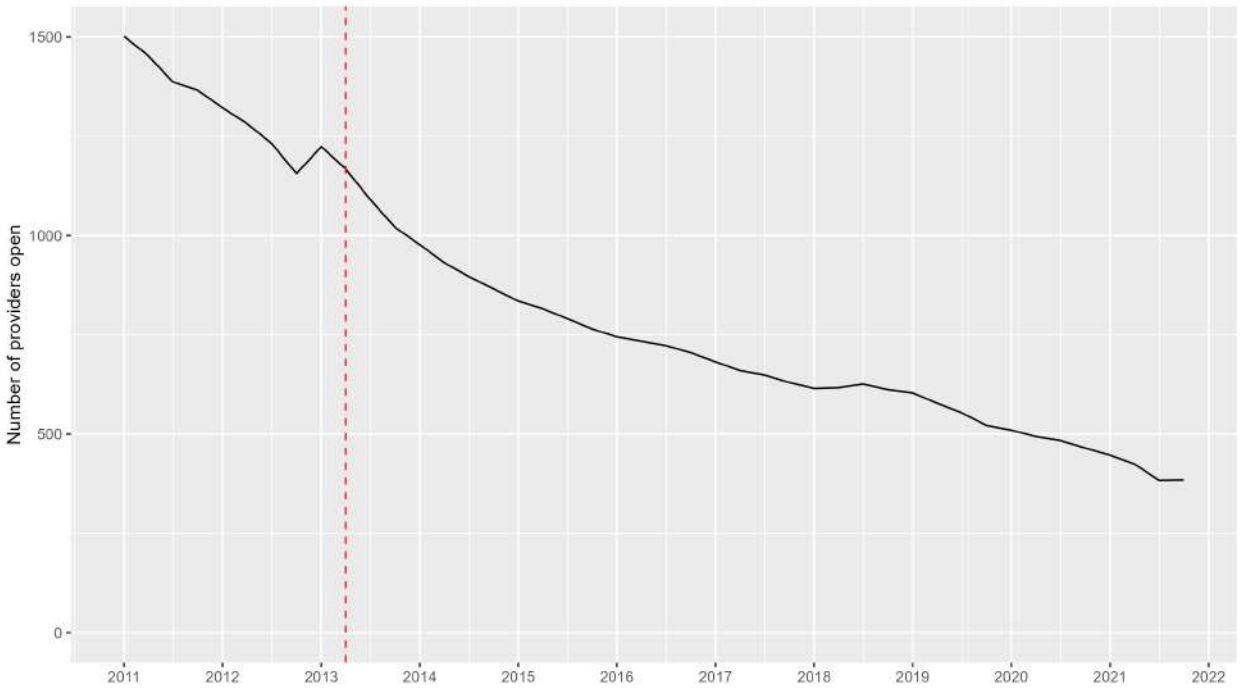


FIGURE 3: Number of active legal aid providers over time

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.

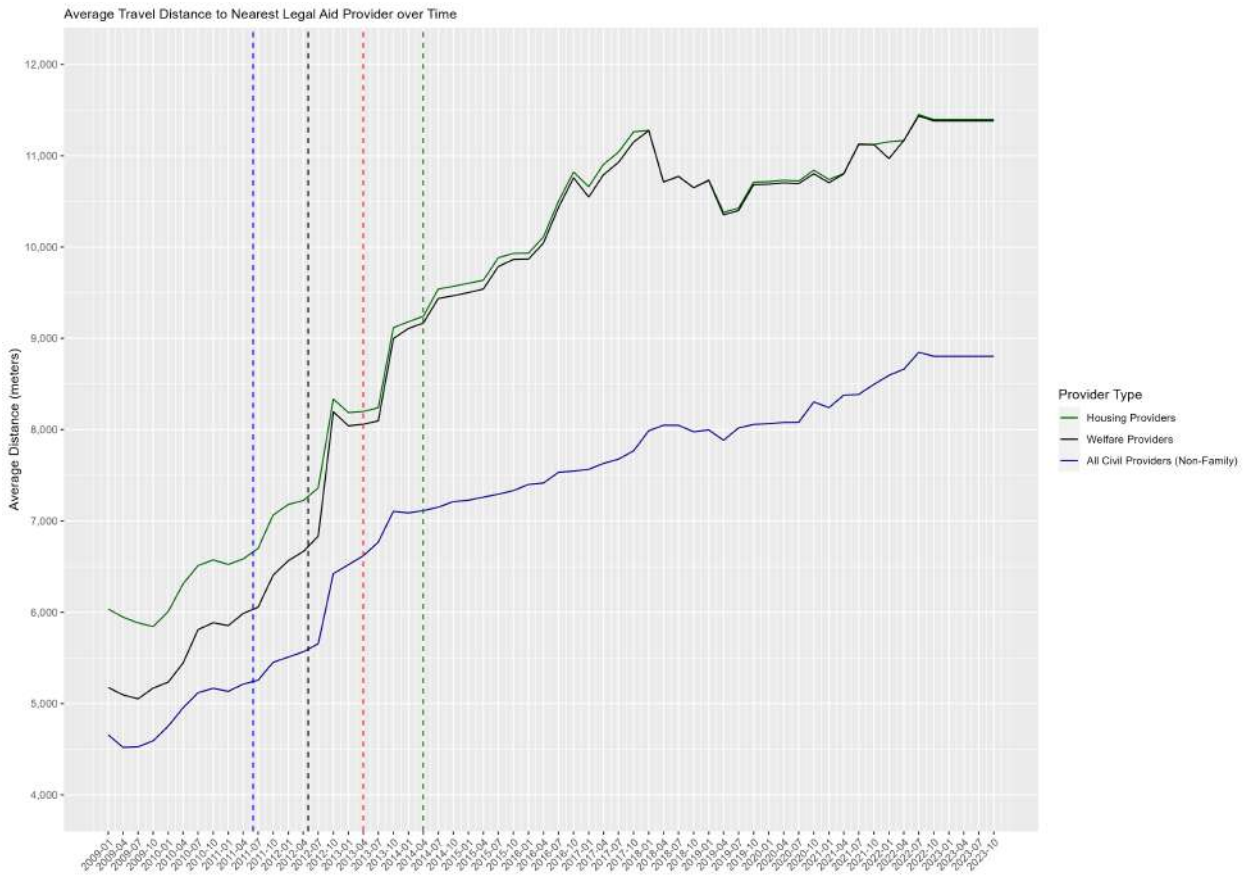


FIGURE 4: Average Distance to Nearest Legal Aid Provider over Time

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.

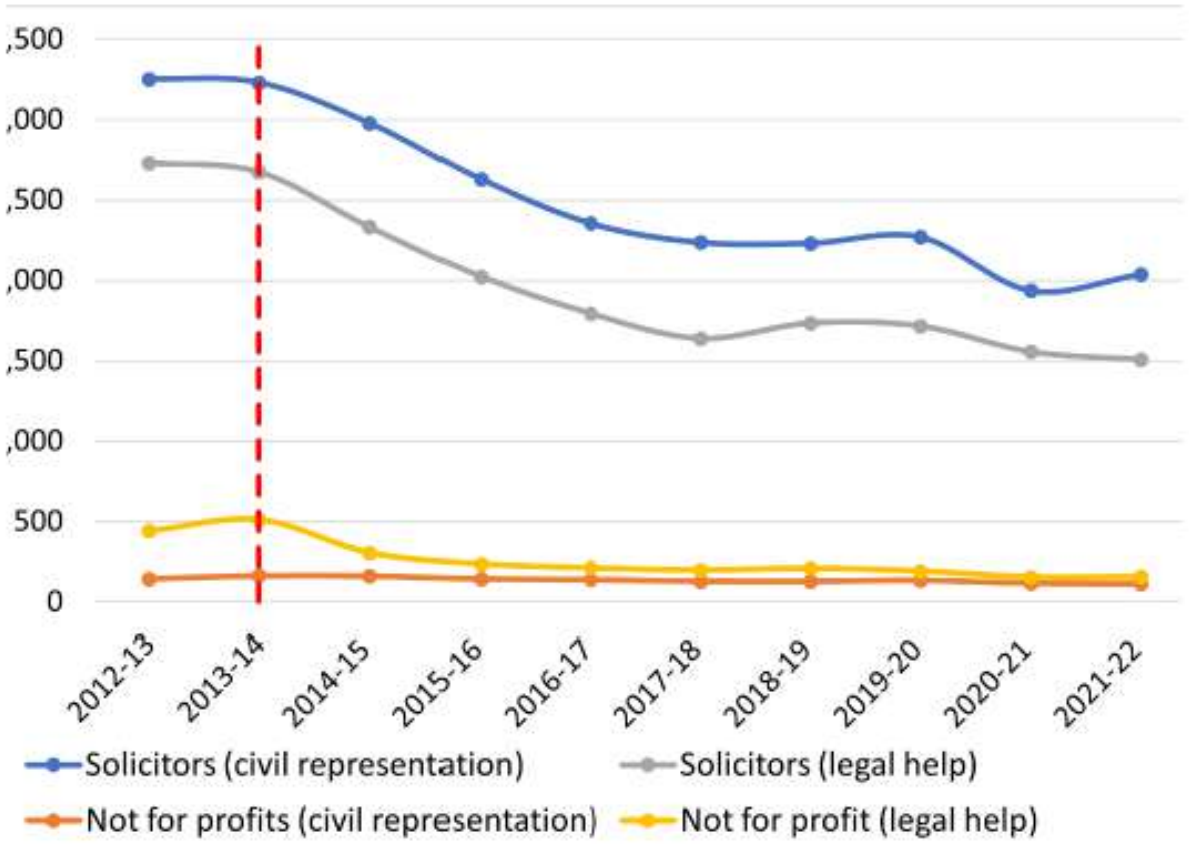


FIGURE 5: Number of active legal aid providers over time, by type of providers

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.

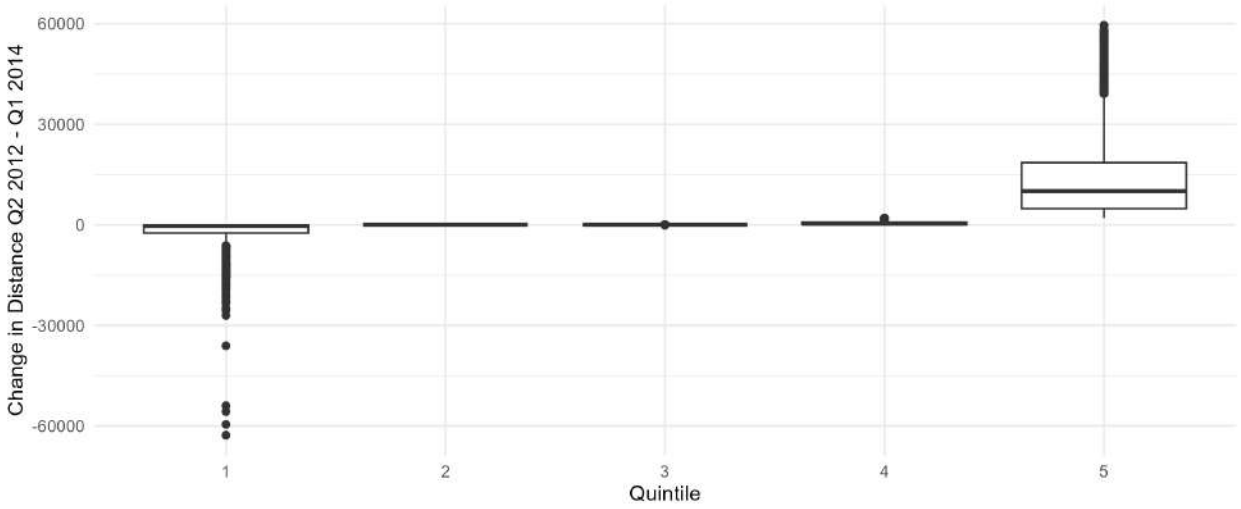


FIGURE 6: Distribution of the Treatment Variable across Quintiles

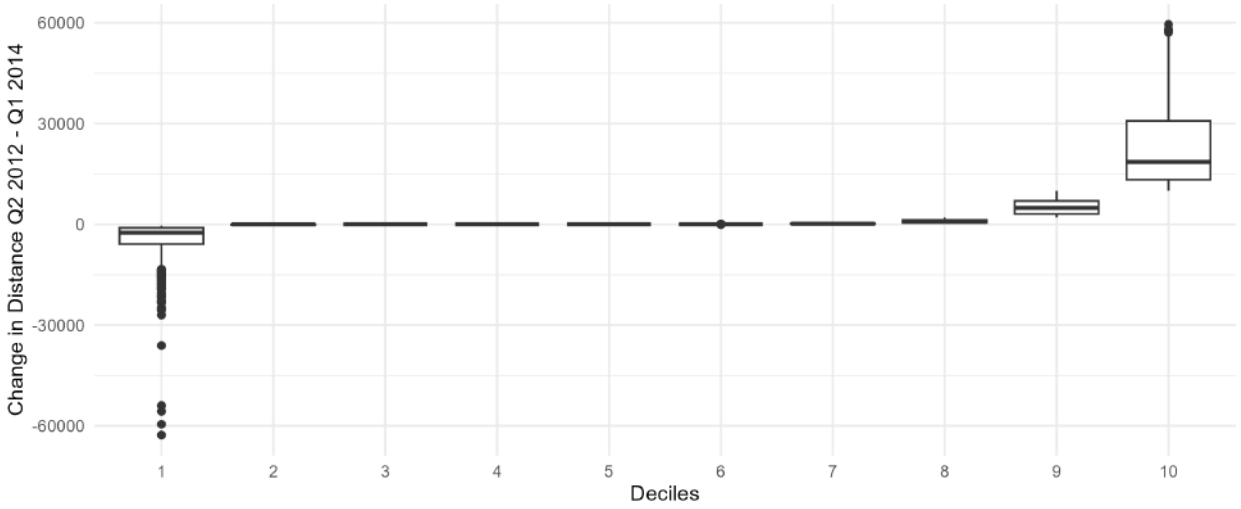


FIGURE 7: Distribution of the Treatment Variable across Deciles

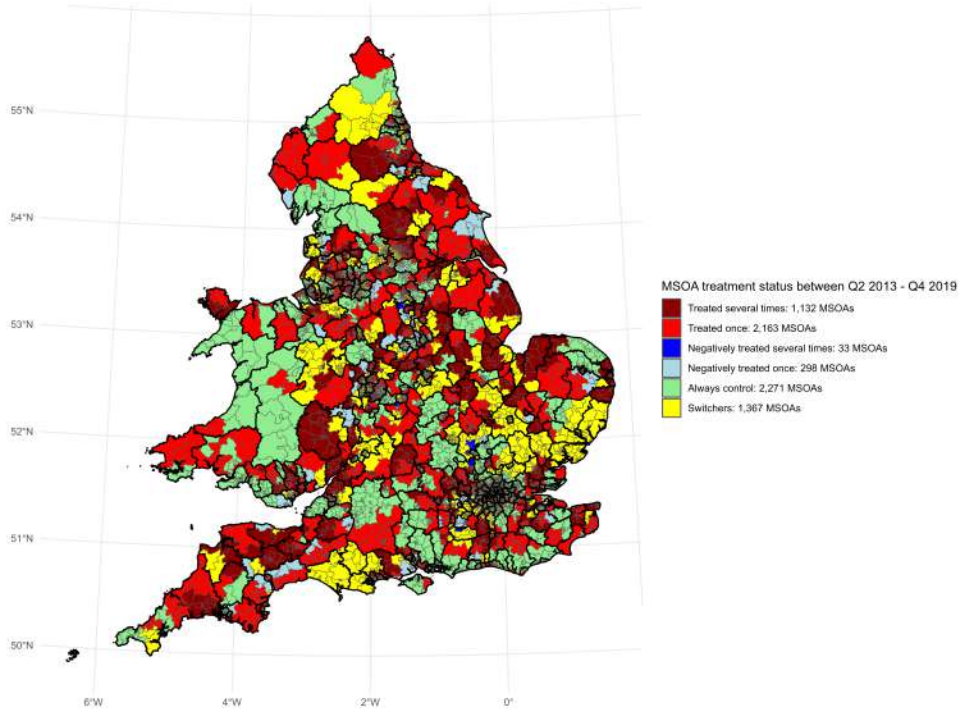


FIGURE 8: Dynamic Treatment Distribution Across England and Wales

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.

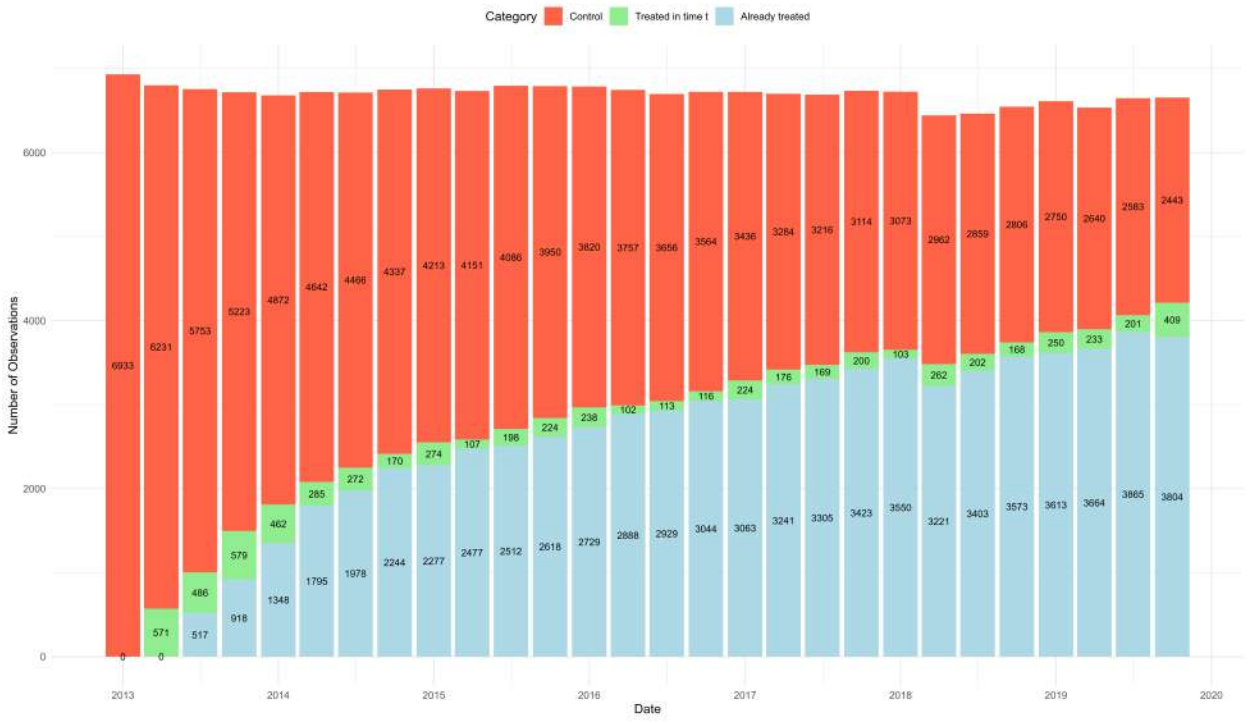


FIGURE 9: Number of Observations over Time

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.

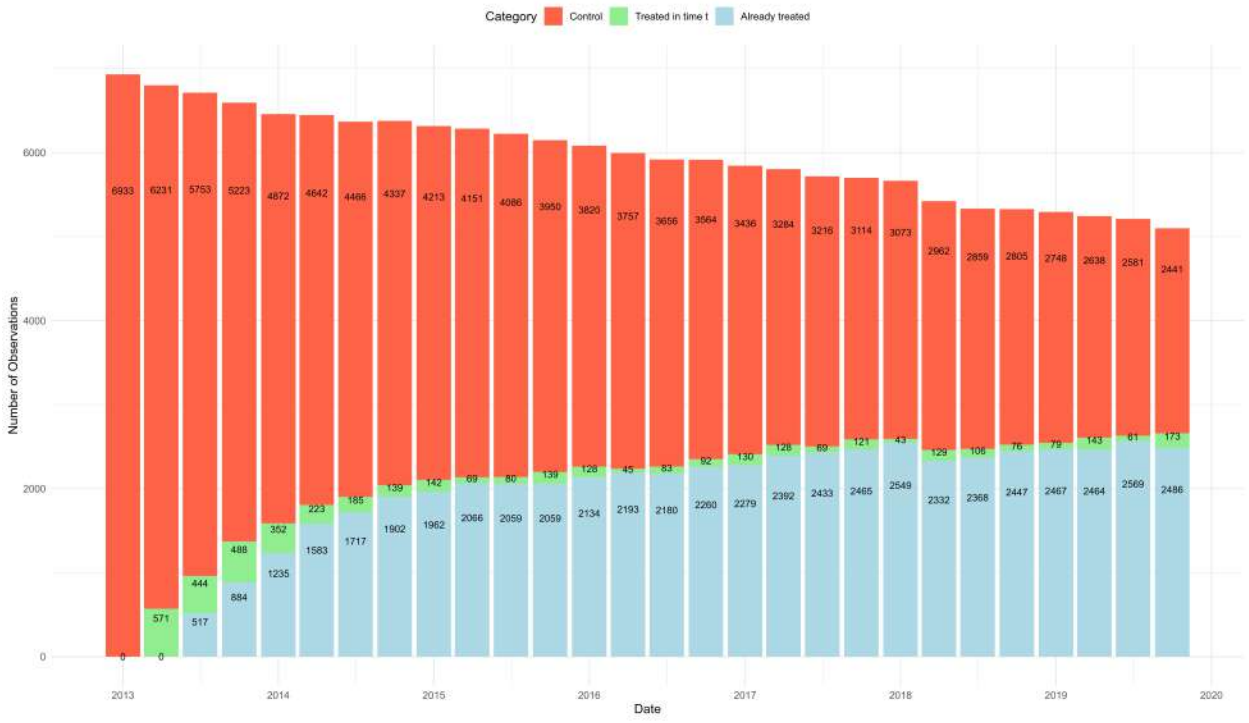


FIGURE 10: Number of Observations over Time - Excluding Higher than One Treatment Dose

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.

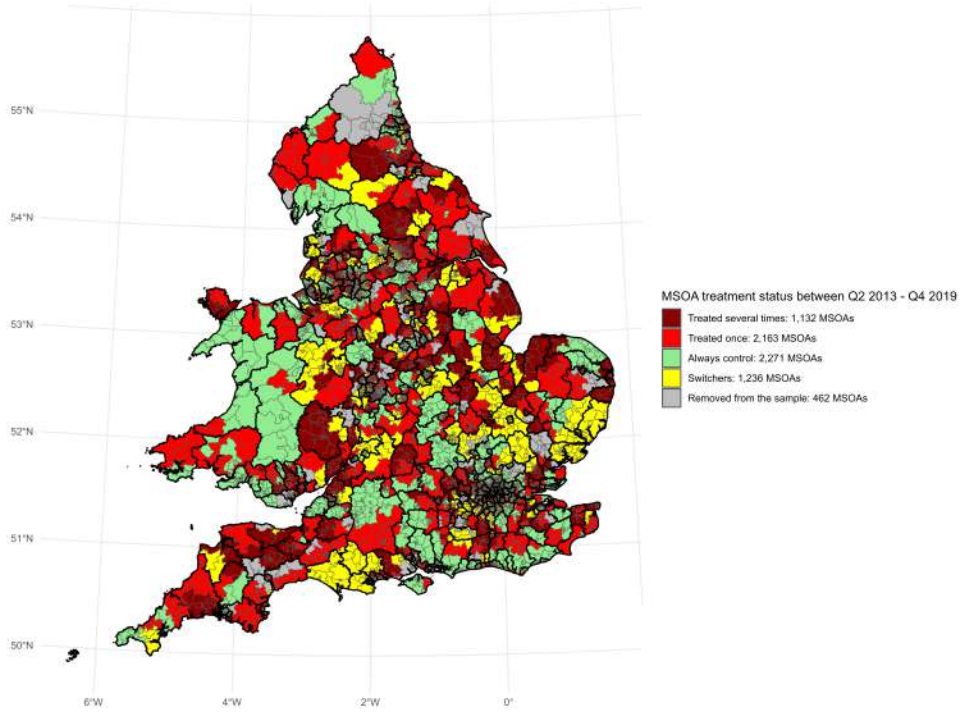


FIGURE 11: Dynamic Treatment Distribution Across England and Wales - without Negative Treatment

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.

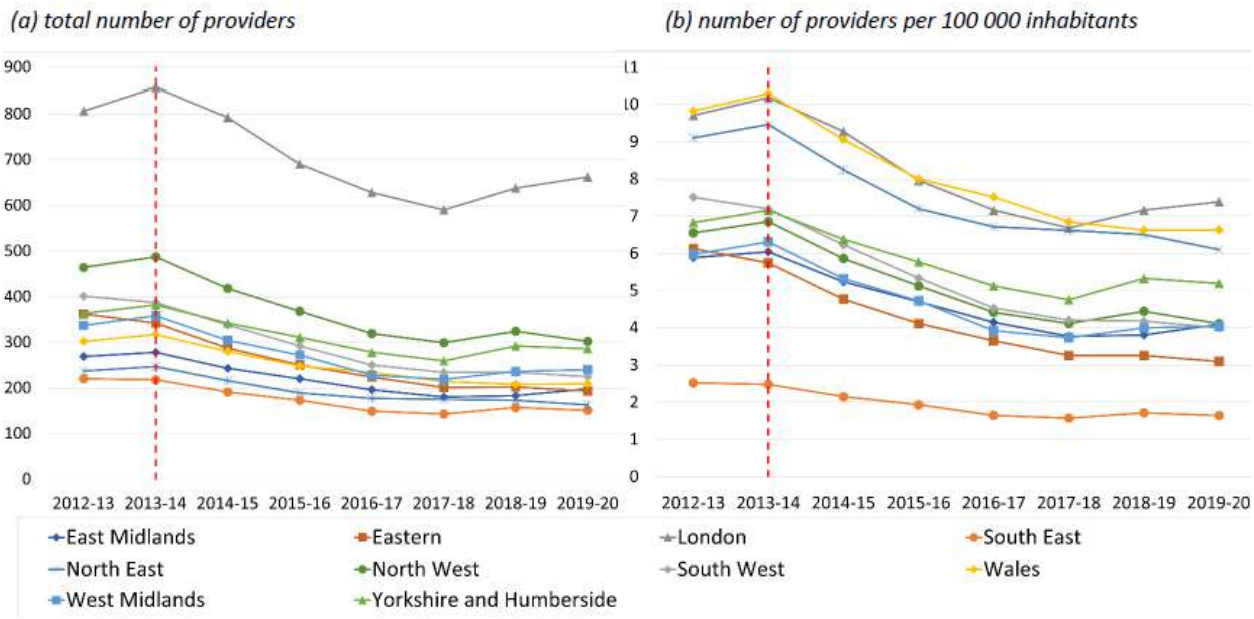
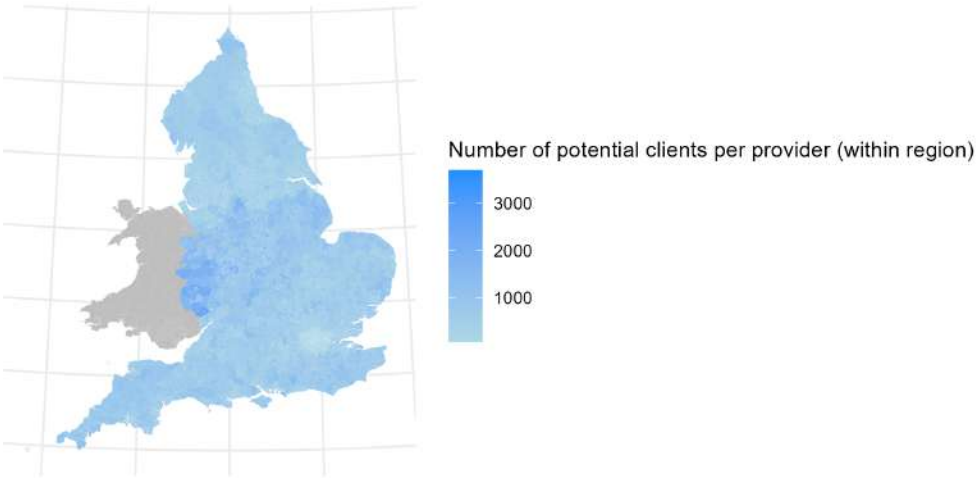
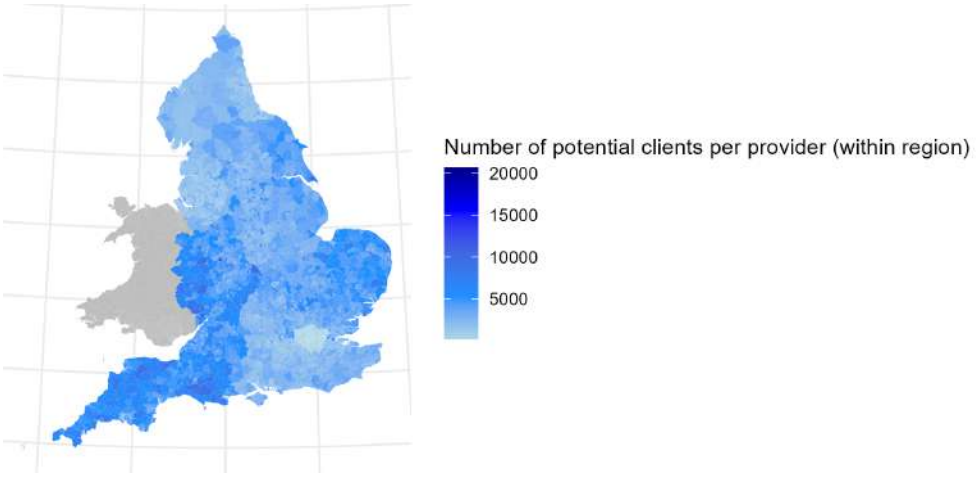


FIGURE 12: Number of legal aid providers across regions in England and Wales

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.

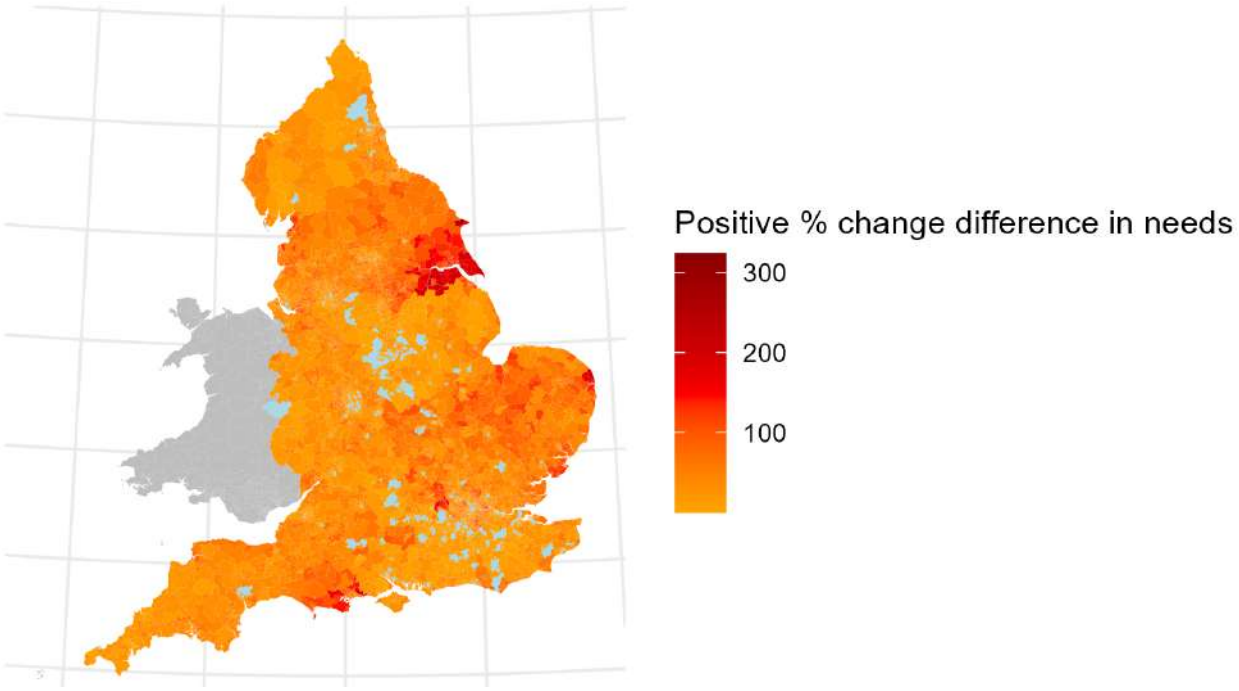


FIGURE 14: Evolution of housing legal needs between 2011 and 2021

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.

B Additional Figures

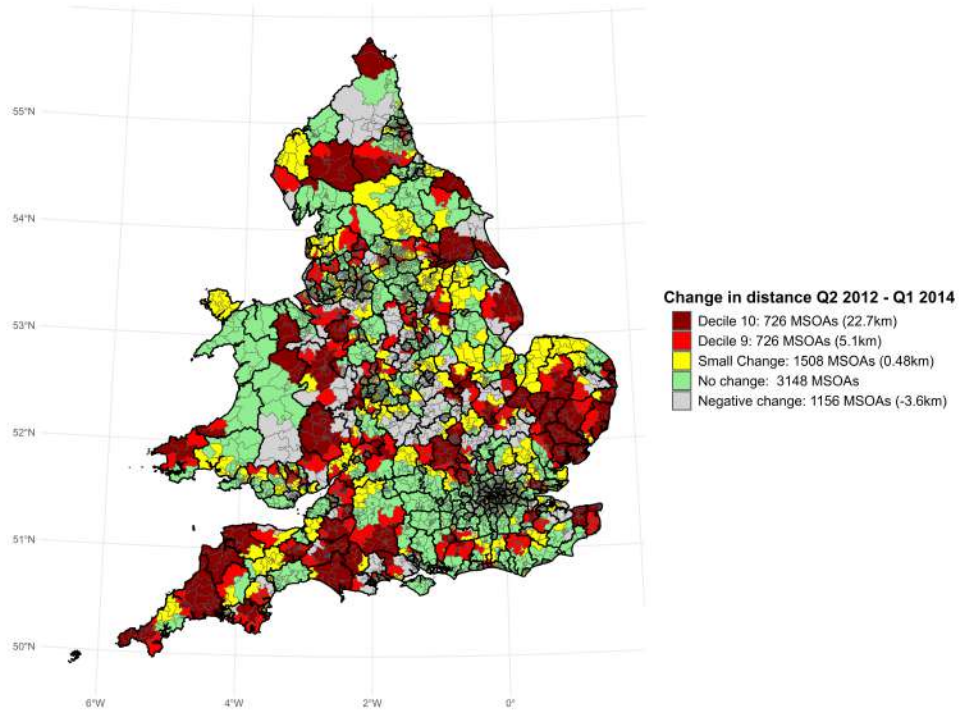


FIGURE 15: Distribution of the Treatment Group across MSOAs - Welfare Providers - Static

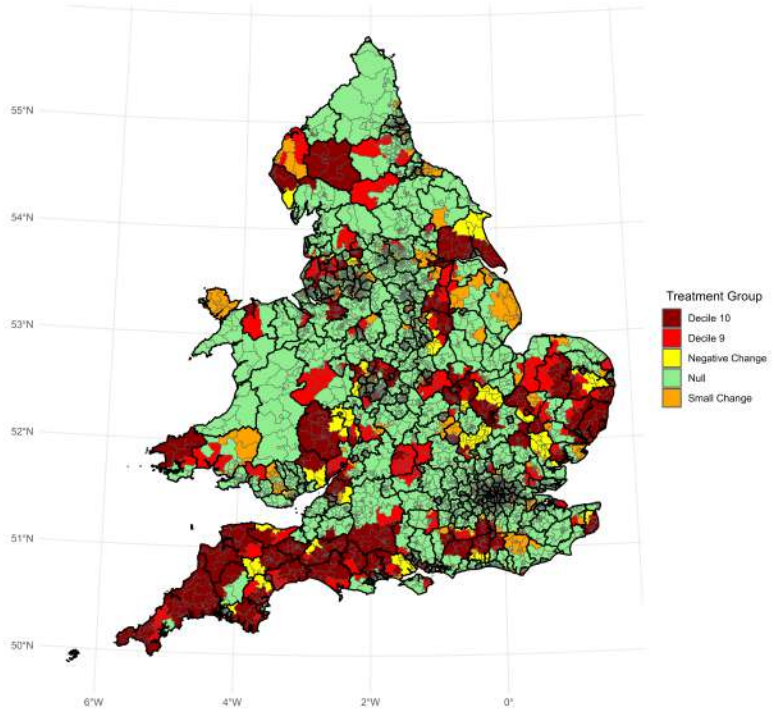


FIGURE 16: Distribution of the Treatment Group across MSOAs - Housing Providers - Static

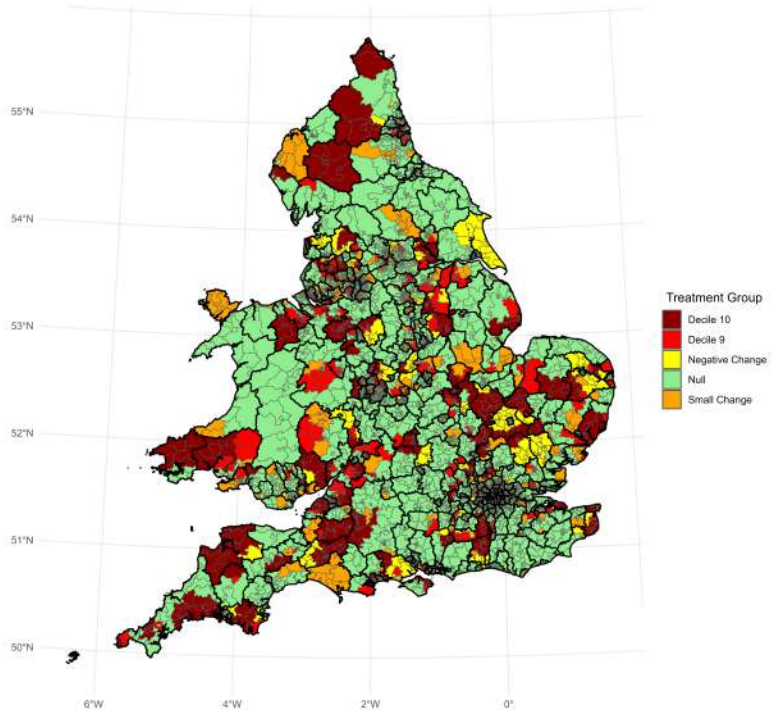


FIGURE 17: Distribution of the Treatment Group across MSOAs - Civil Providers - Static

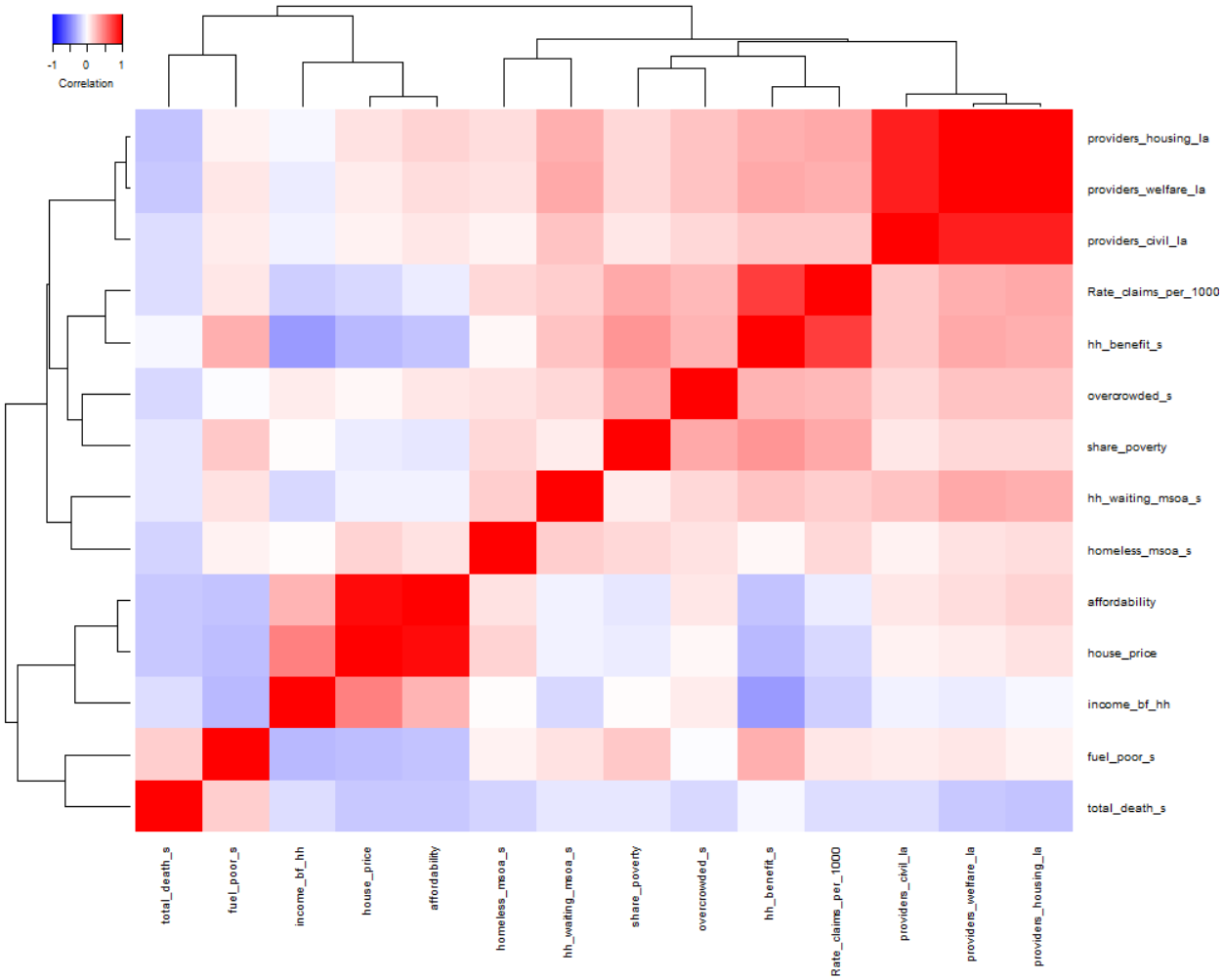


FIGURE 18: Correlation of variables

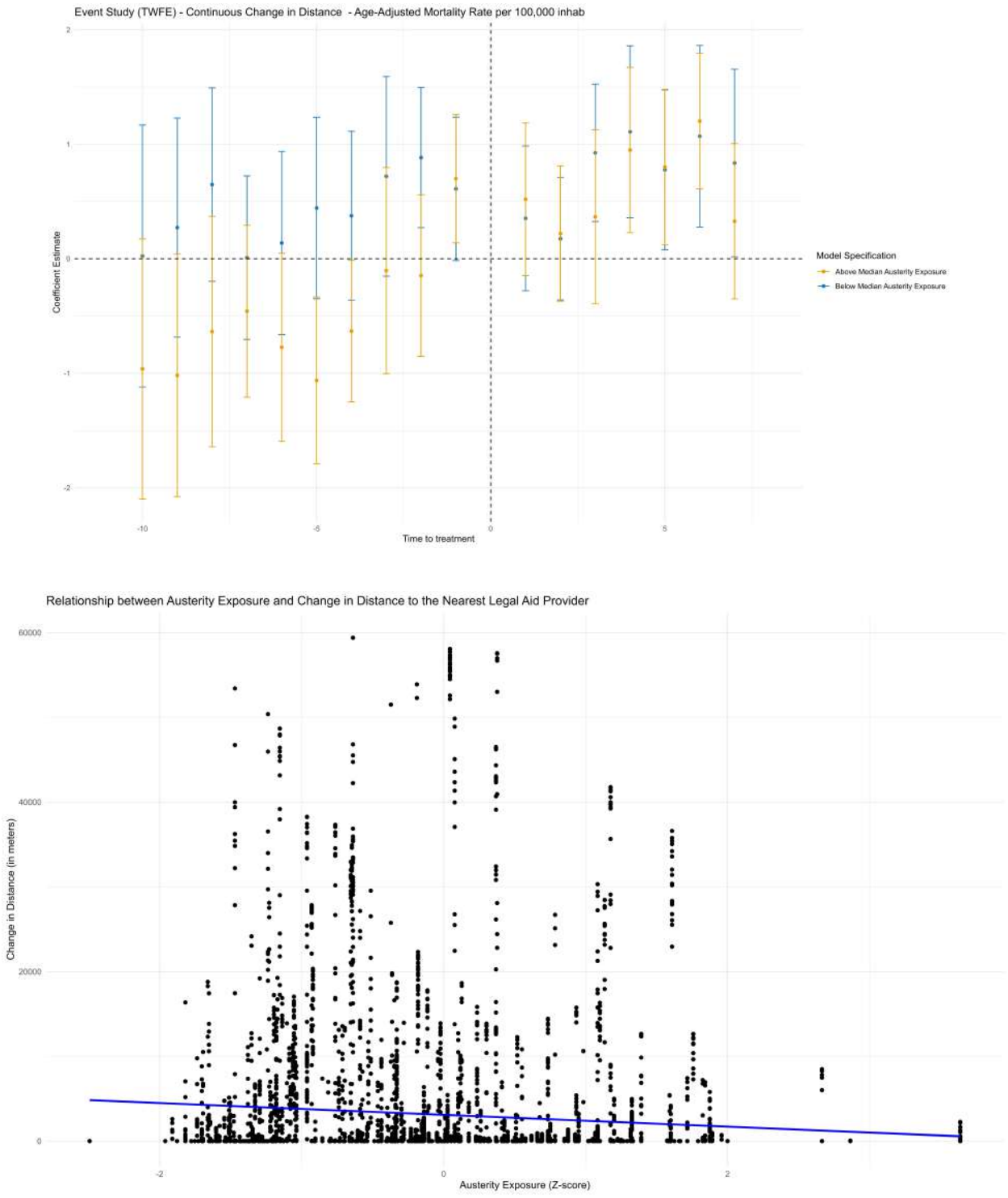


FIGURE 19: Relationship between austerity exposure and change in distance to the nearest legal aid provider

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

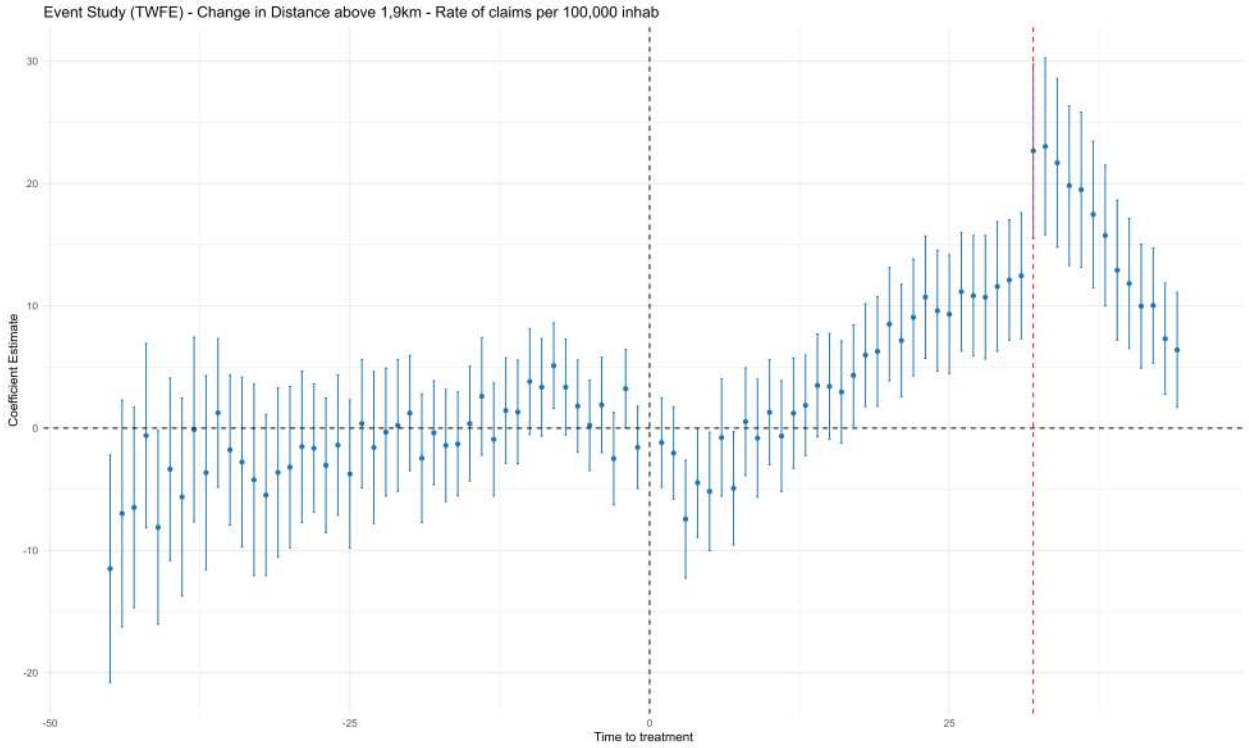


FIGURE 20: Eviction claim rate per 100,000 inhabitants - Static - Change in Distance >1.9km

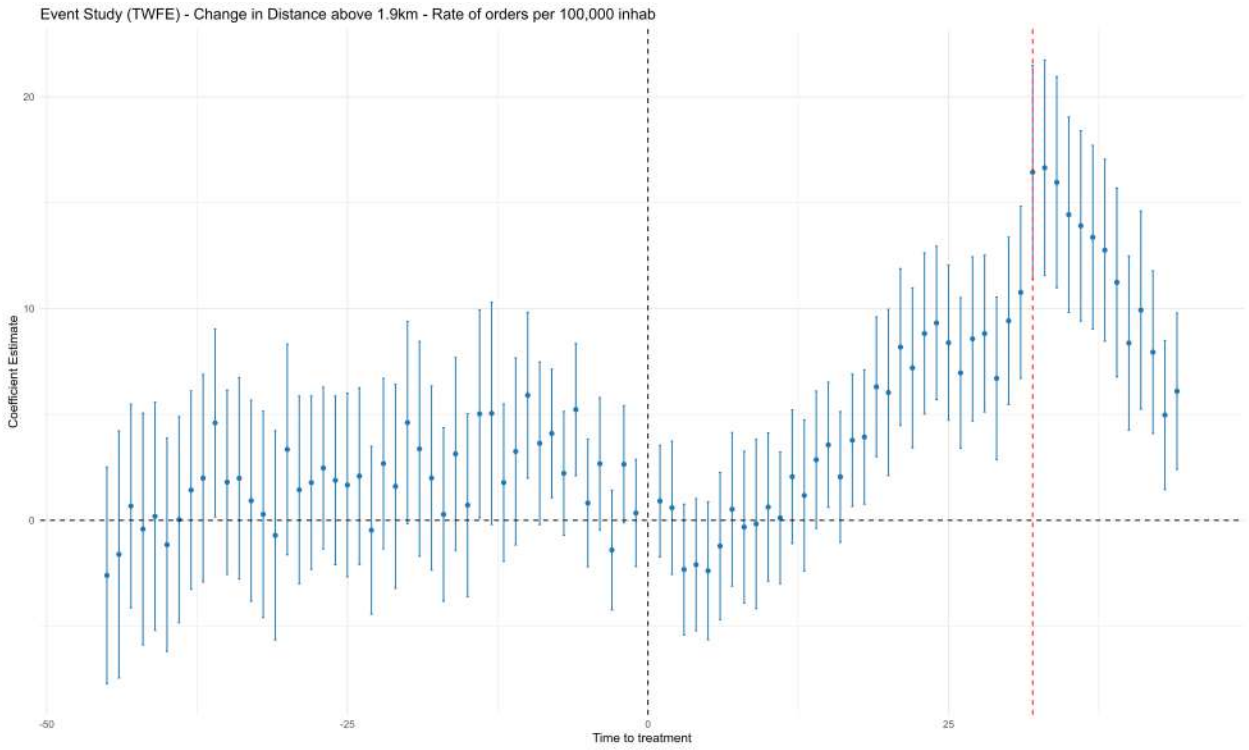


FIGURE 21: Eviction orders per 100,000 inhabitants - Static - Change in Distance >1.9km

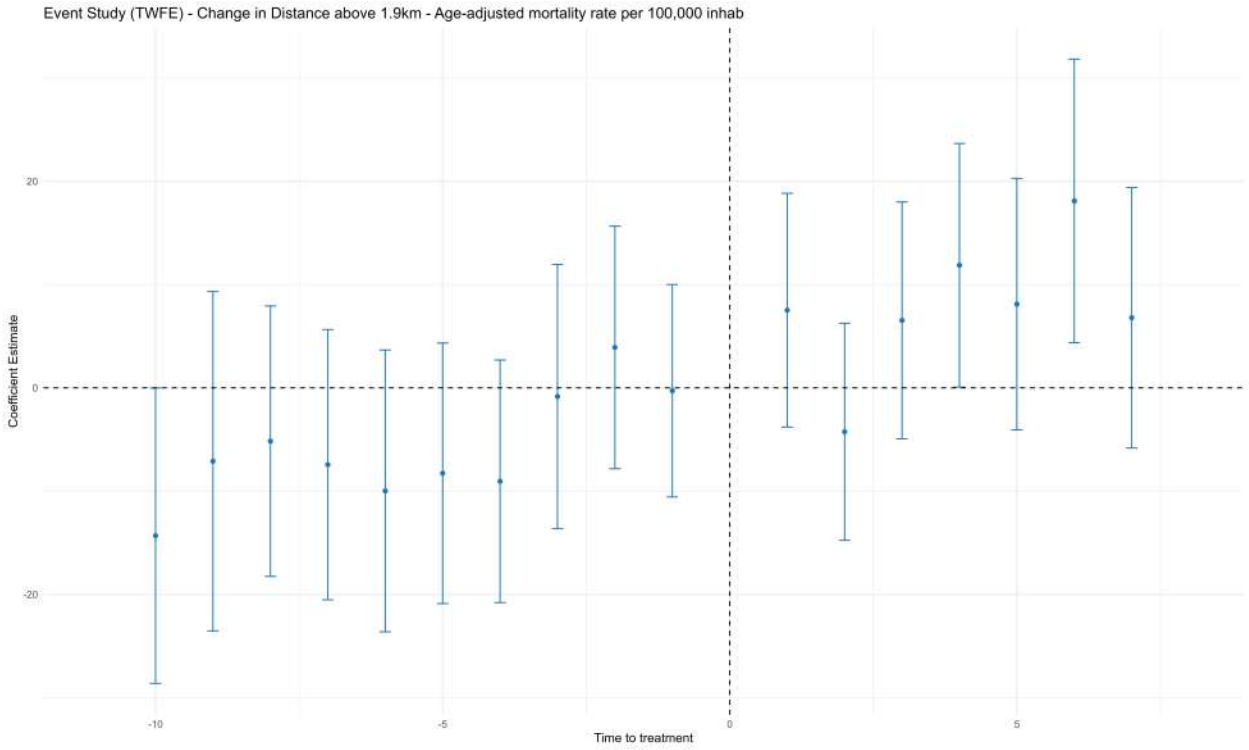


FIGURE 22: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Change in Distance >1.9km

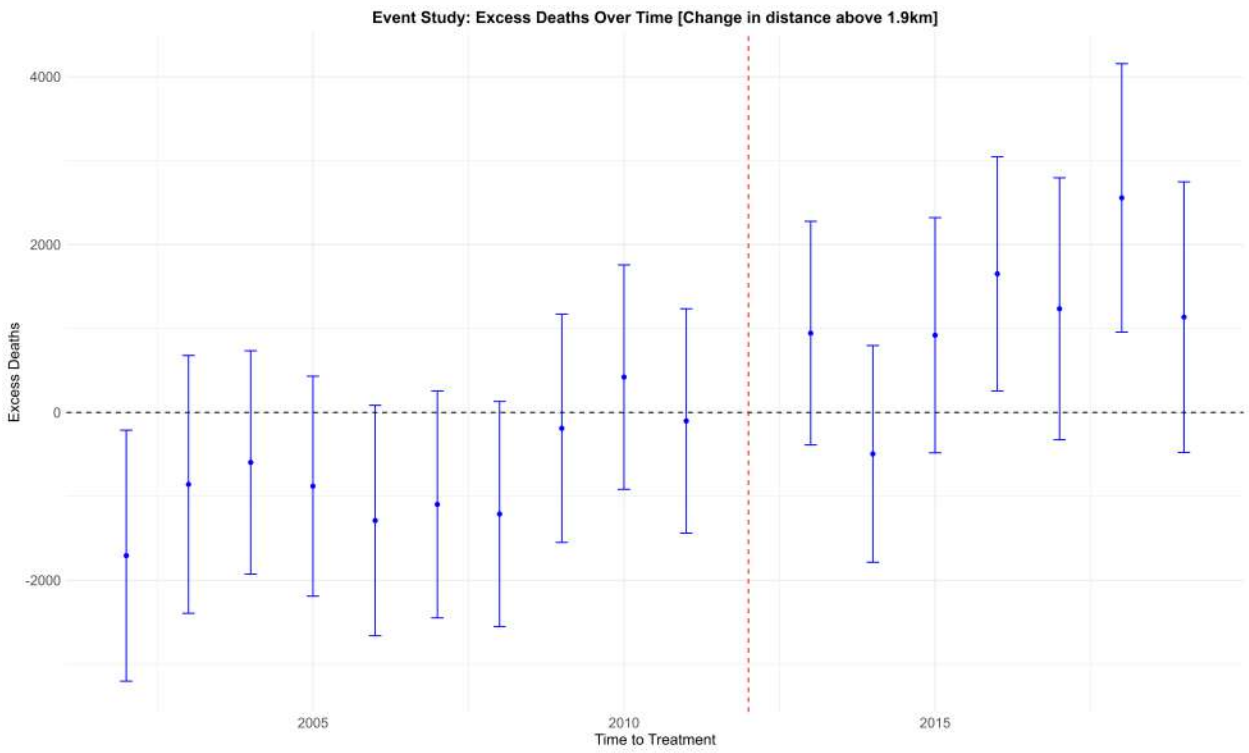


FIGURE 23: Excess deaths - Static - Change in Distance >1.9km

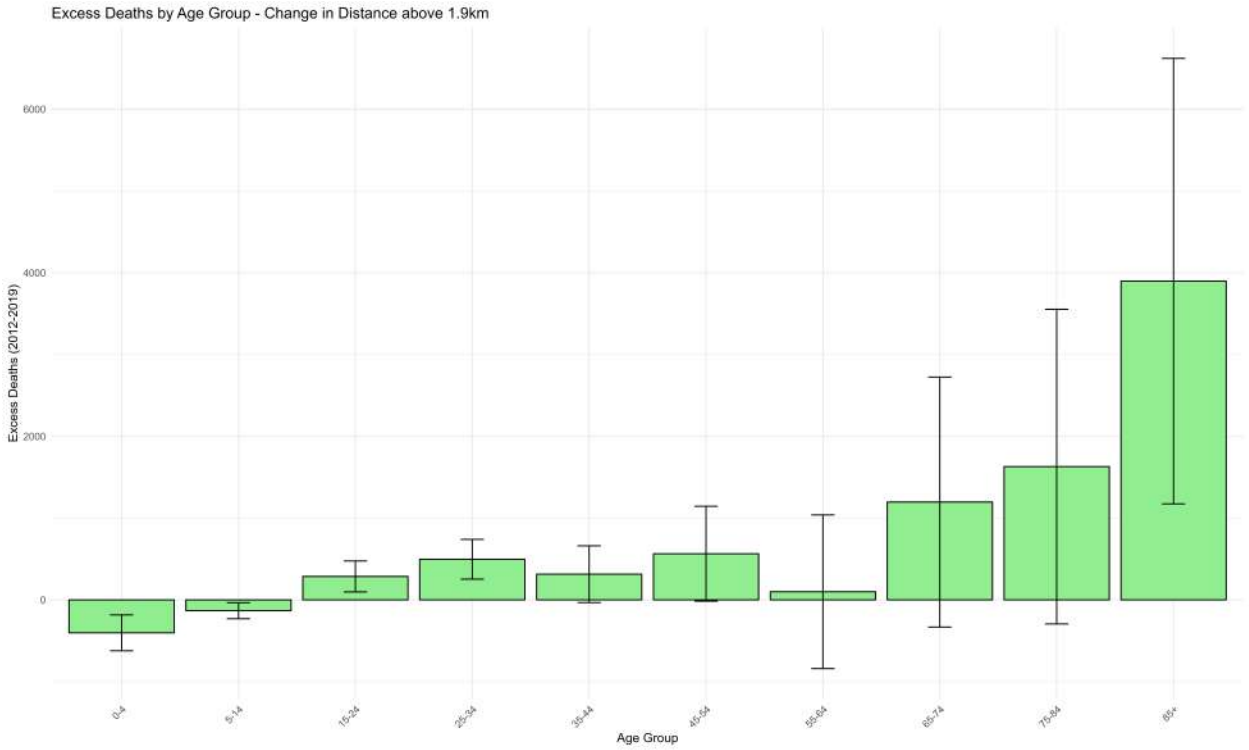


FIGURE 24: Pooled ATT Age-Adjusted mortality by Age Group - Change in Distance >1.9km

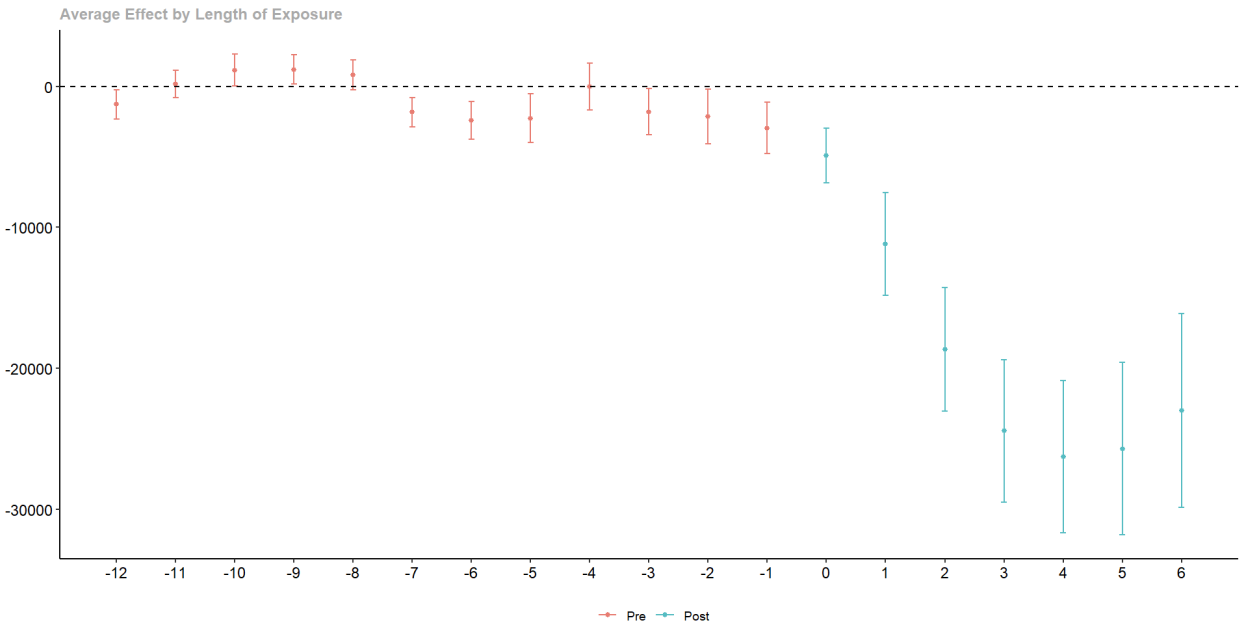


FIGURE 25: Mean House prices - Static

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

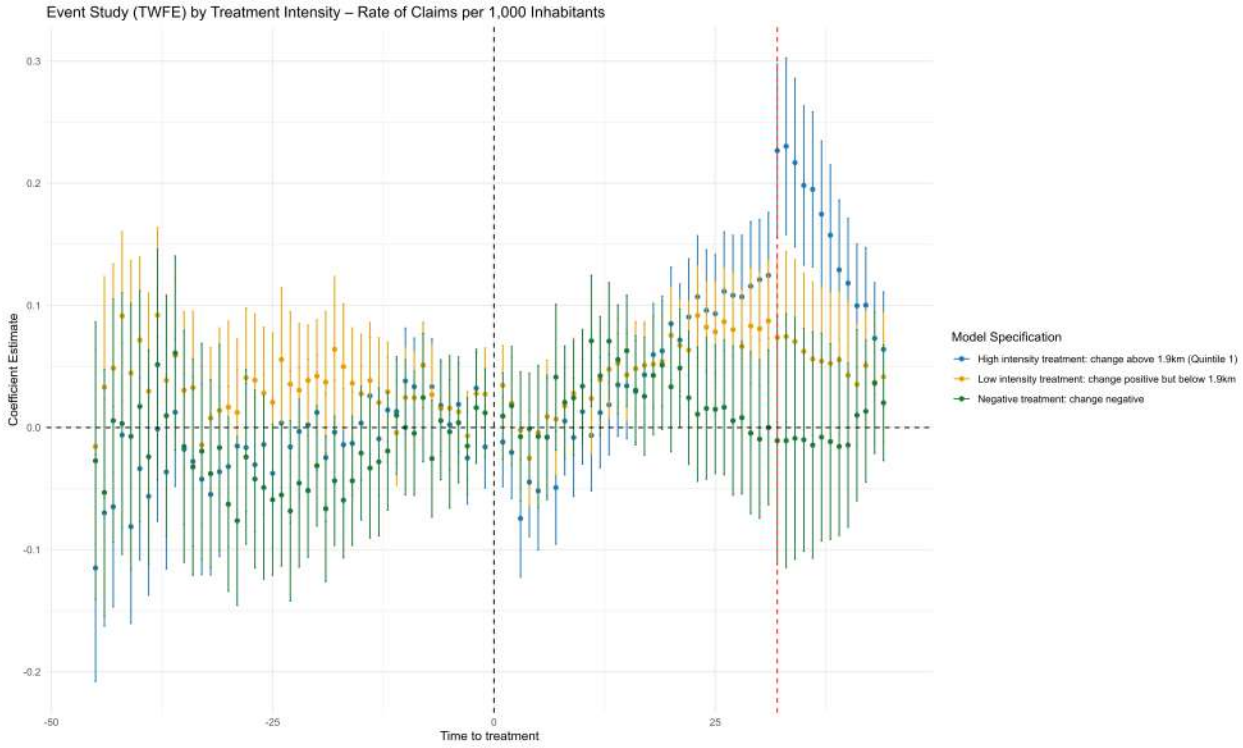


FIGURE 26: Eviction claim rate per 100,000 inhabitants - Static - Intensity of Change in Distance

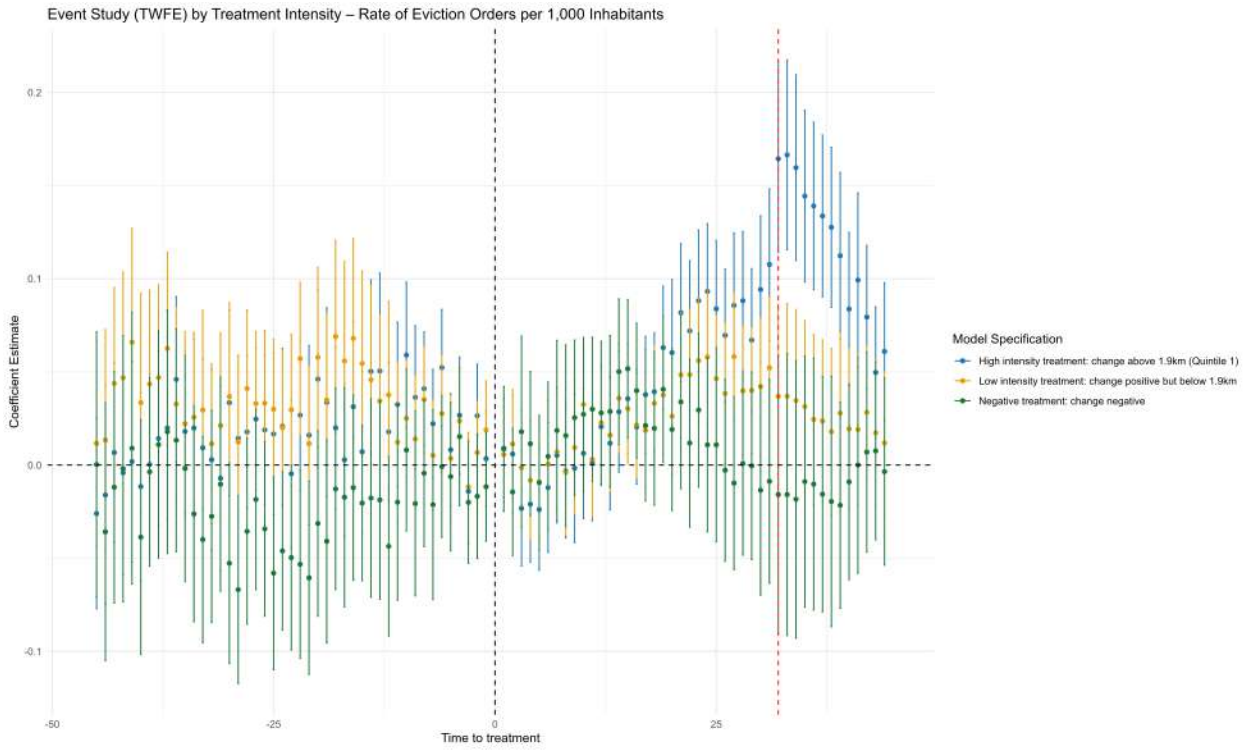


FIGURE 27: Eviction orders per 100,000 inhabitants - Static - Intensity of Change in Distance

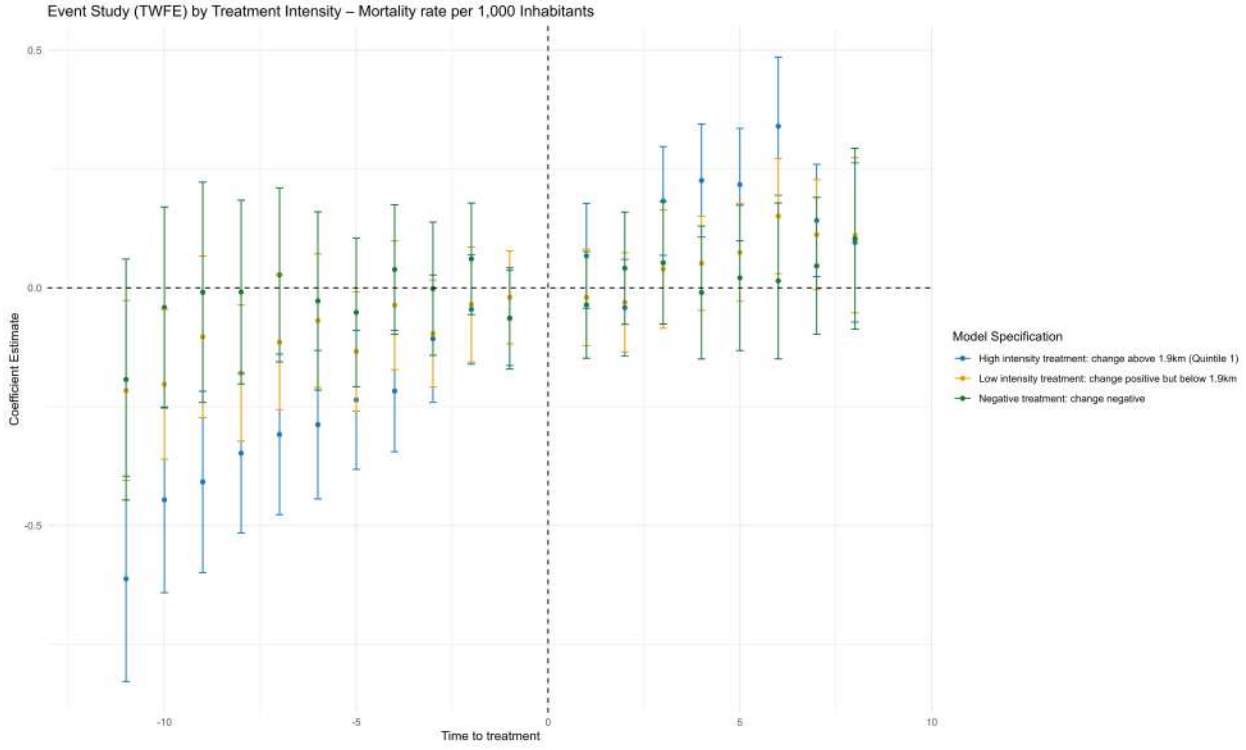


FIGURE 28: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Intensity of Change in Distance

D.1.4 Continuous Definition of Change in Distance

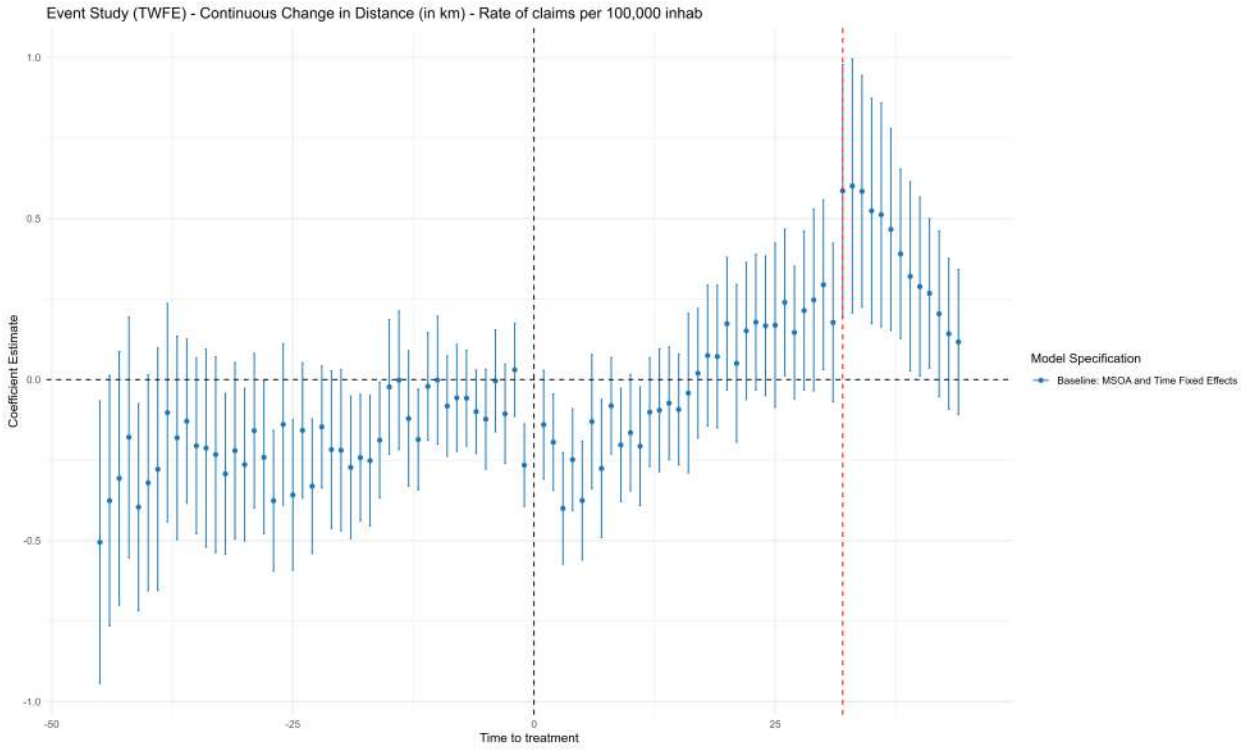


FIGURE 29: Eviction claim rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

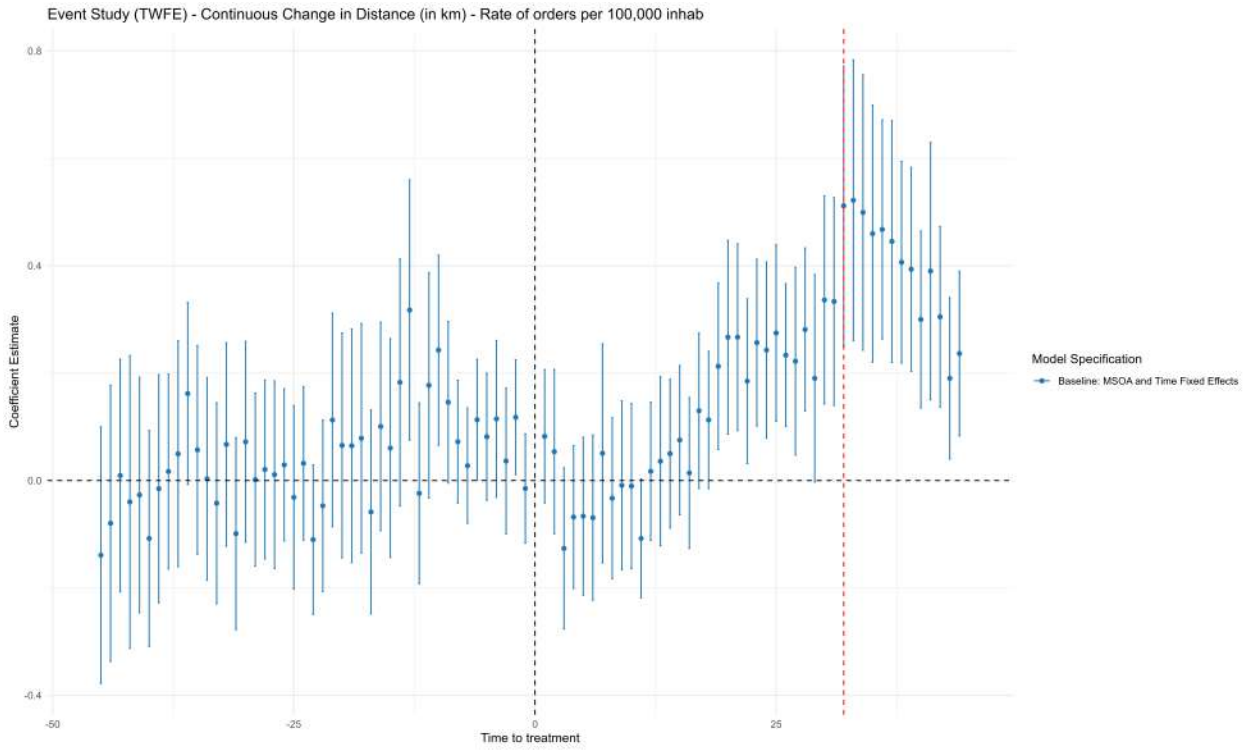


FIGURE 30: Eviction orders per 100,000 inhabitants - Static - Continuous Change in Distance in km

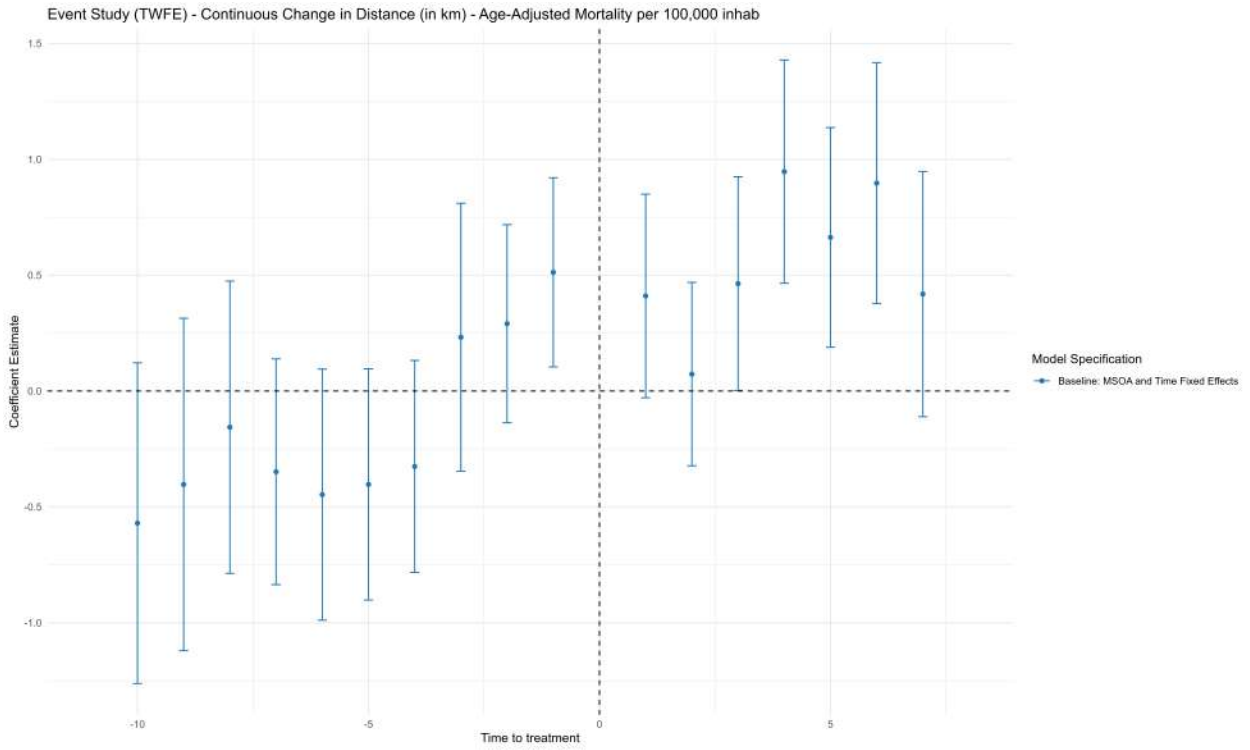


FIGURE 31: Age-adjusted Mortality rate per 100,000 inhabitants - Static - Continuous Change in Distance in km

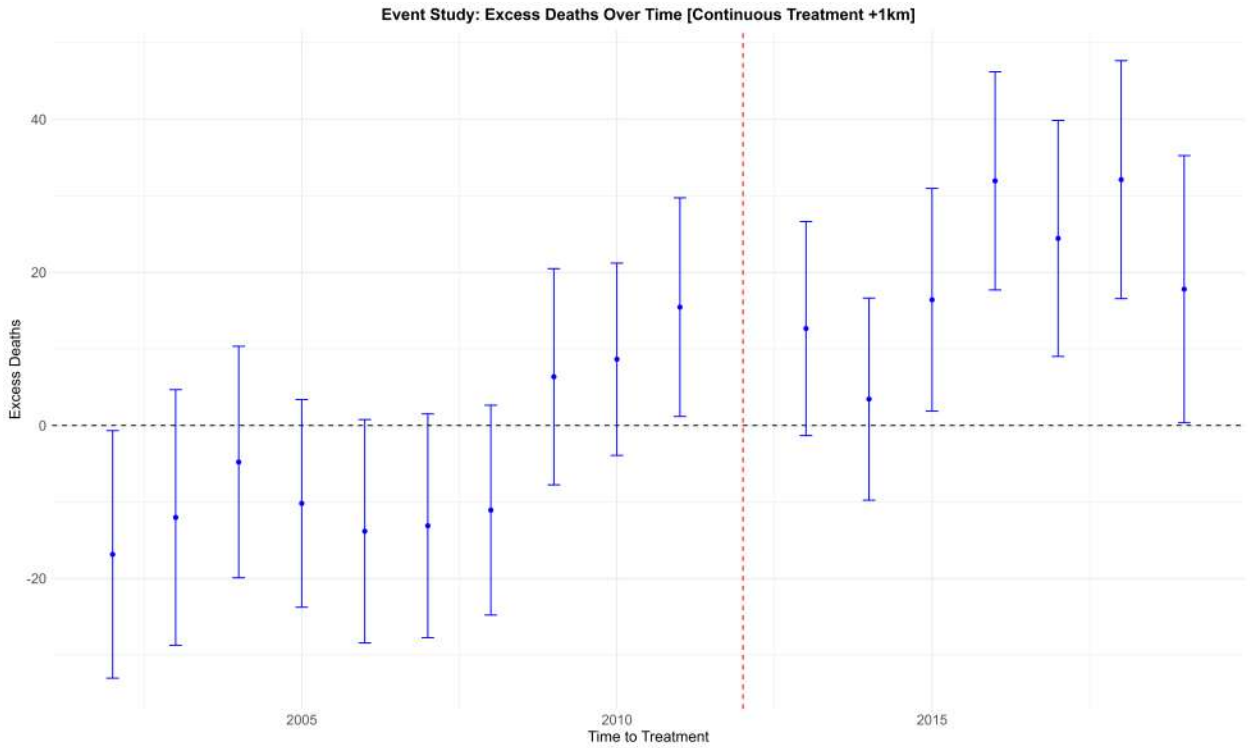


FIGURE 32: Excess deaths - Static - Continuous Change in Distance

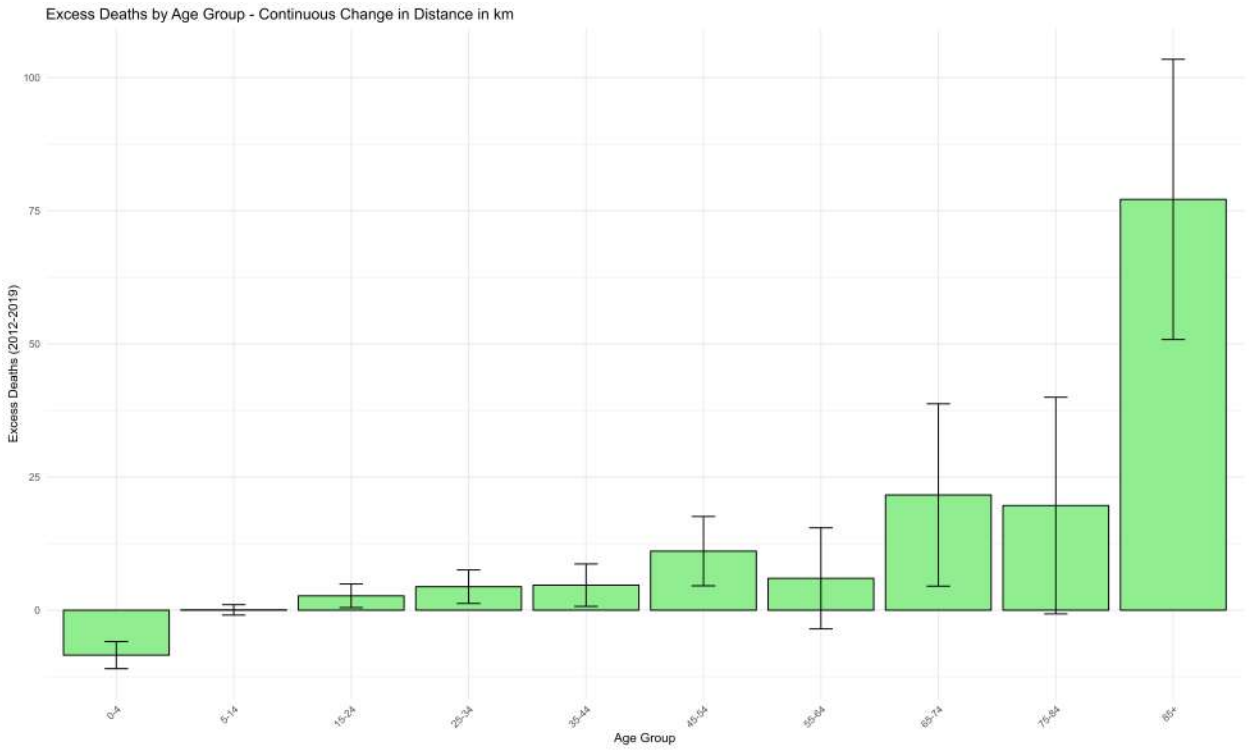


FIGURE 33: Pooled ATT Age-Adjusted mortality by Age Group - Continuous Change in Distance

D.1.5 With Local Authority Fixed Effects

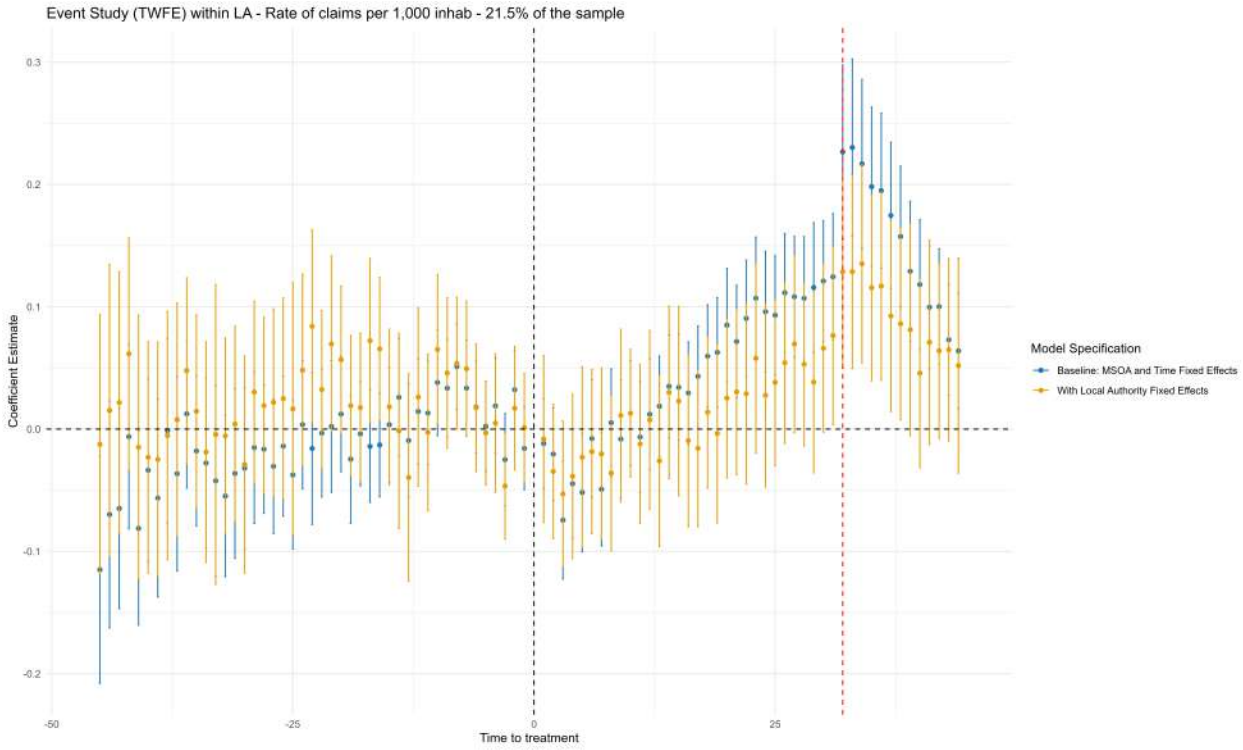


FIGURE 34: Event-study - Eviction Claims - With/without Local Authority Fixed Effects

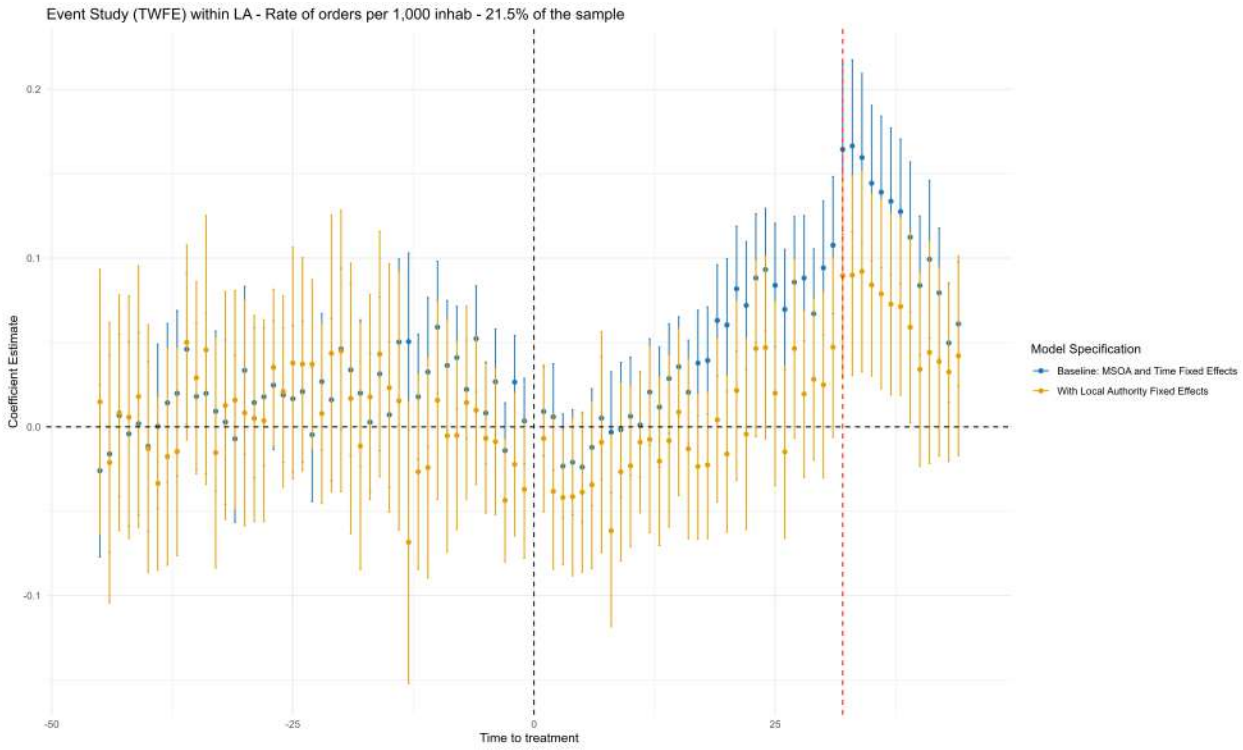


FIGURE 35: Event-study - Eviction Orders - With/without Local Authority Fixed Effects

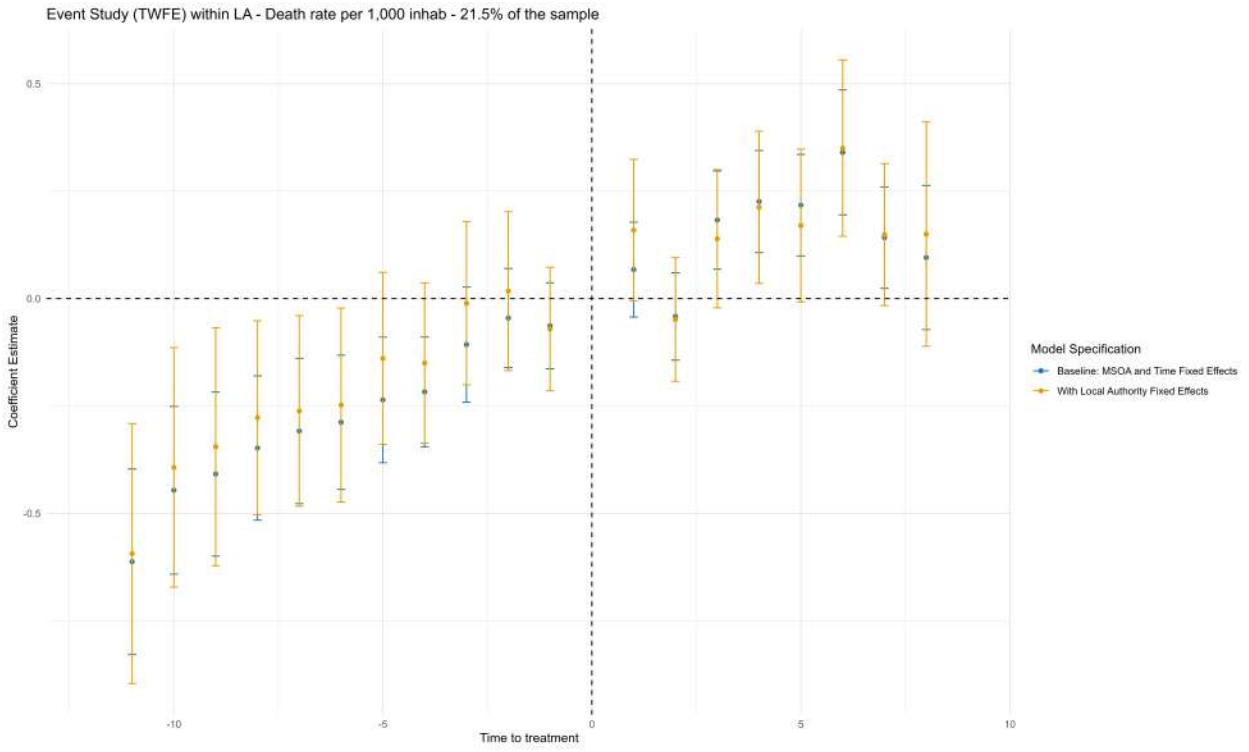


FIGURE 36: Event-study - Mortality - With/without Local Authority Fixed Effects

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

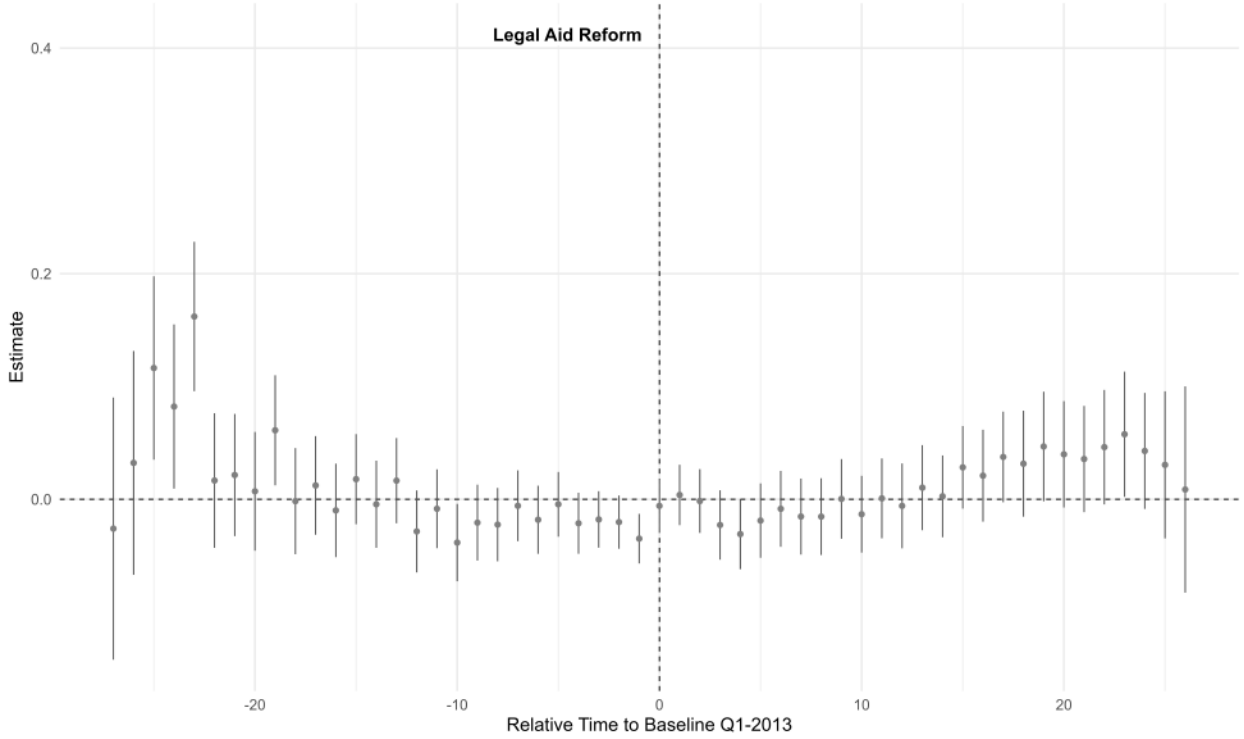


FIGURE 37: Eviction claim rate per 1,000 inhabitants - Staggered

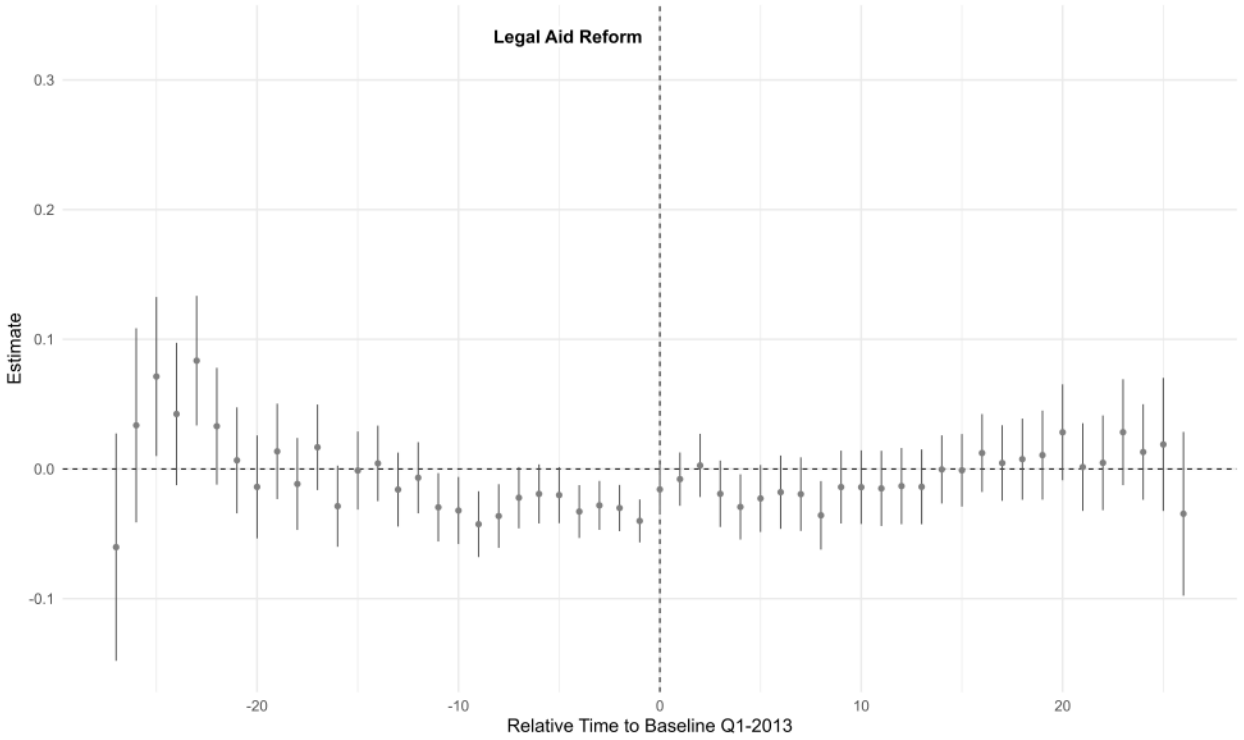


FIGURE 38: Eviction orders per 1,000 inhabitants - Staggered

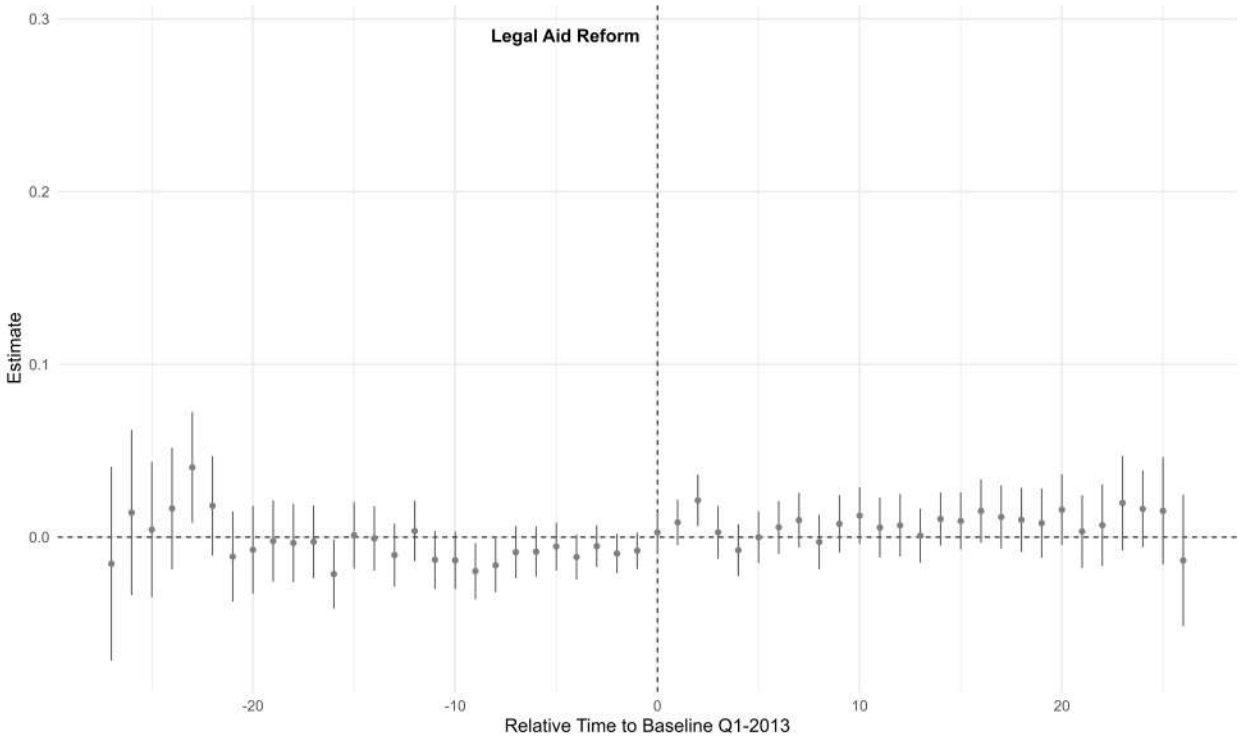


FIGURE 39: Eviction Suspended Possession orders per 1,000 inhabitants - Staggered

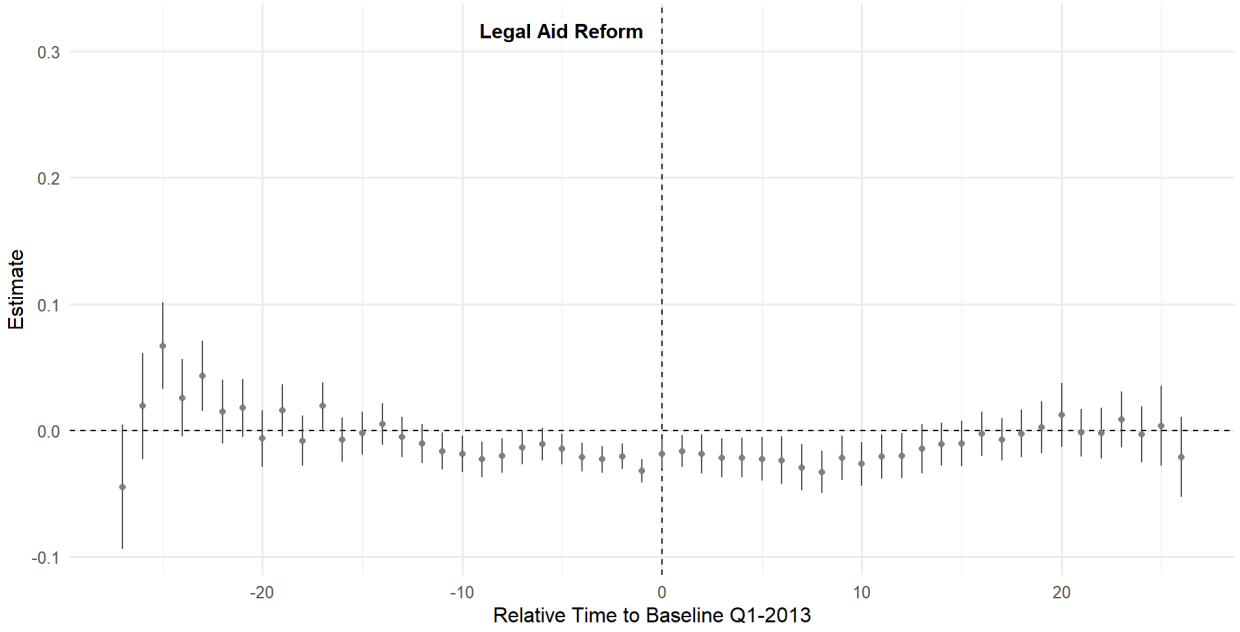


FIGURE 40: Eviction Outright orders per 1,000 inhabitants - Staggered

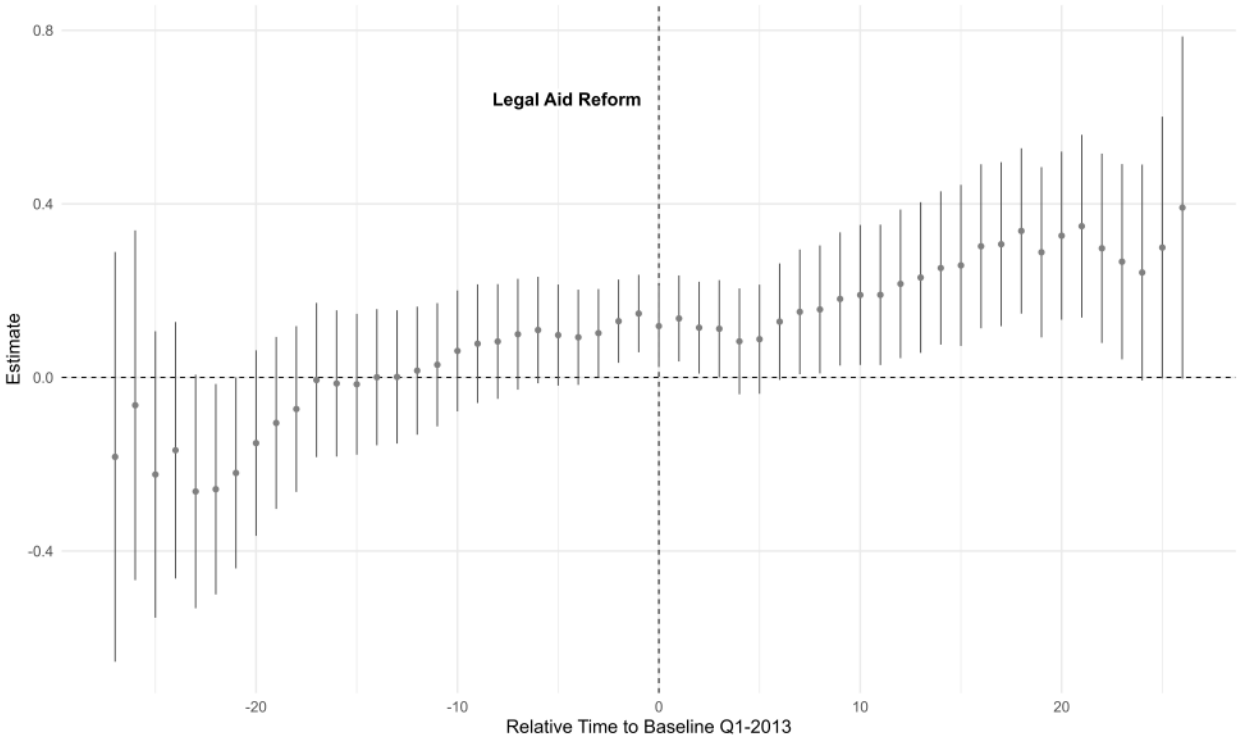


FIGURE 41: Mortality rate per 1,000 inhabitants - Staggered

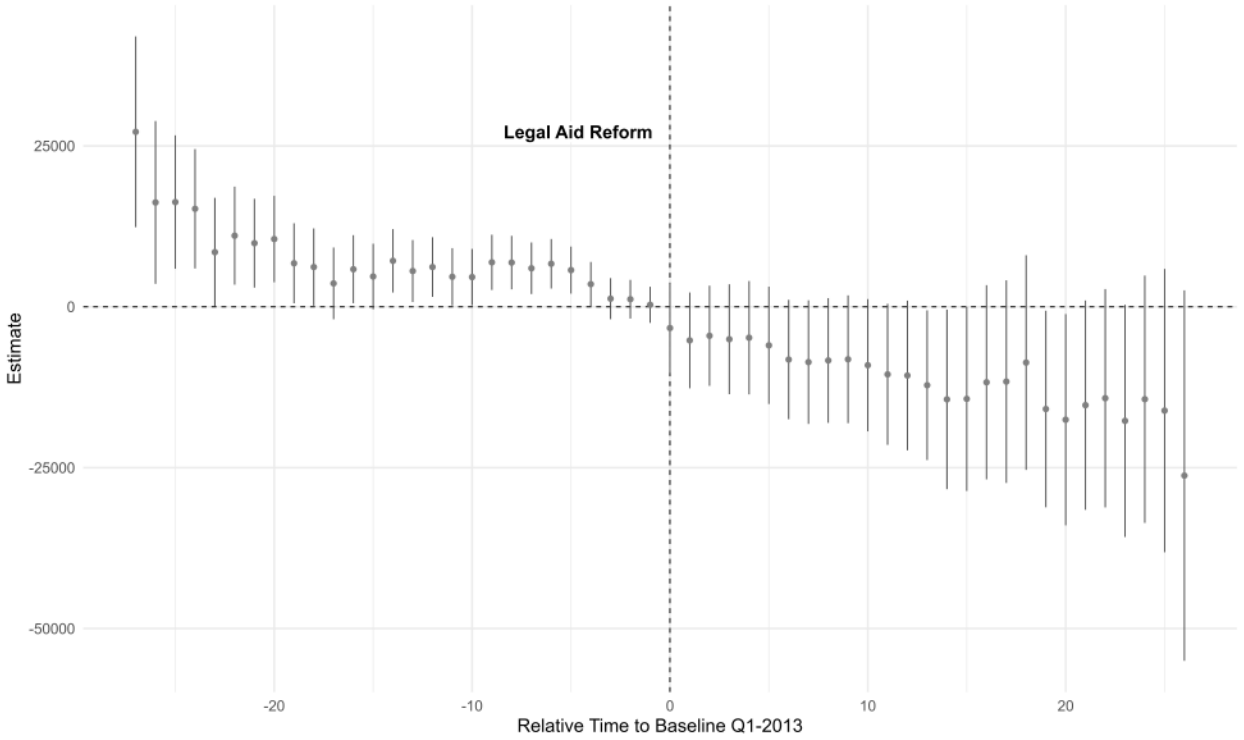


FIGURE 42: Mean House prices - Staggered

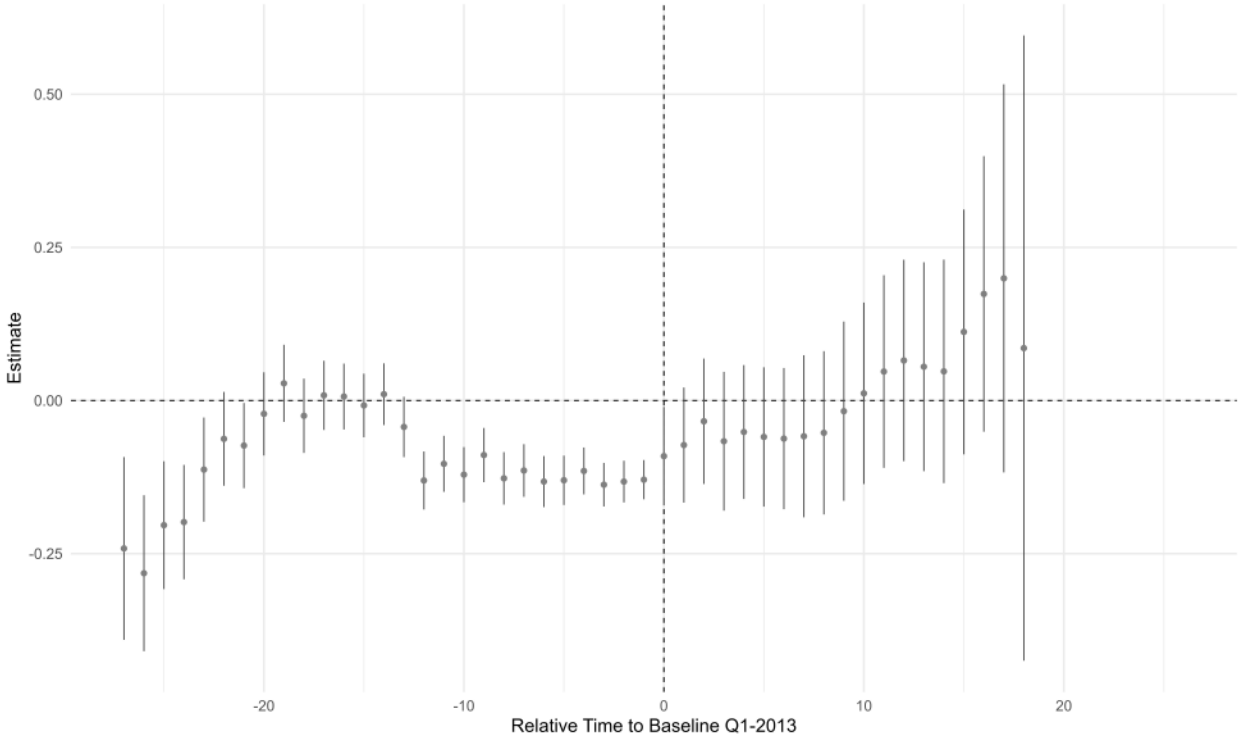


FIGURE 43: Households at risk of homelessness - Staggered

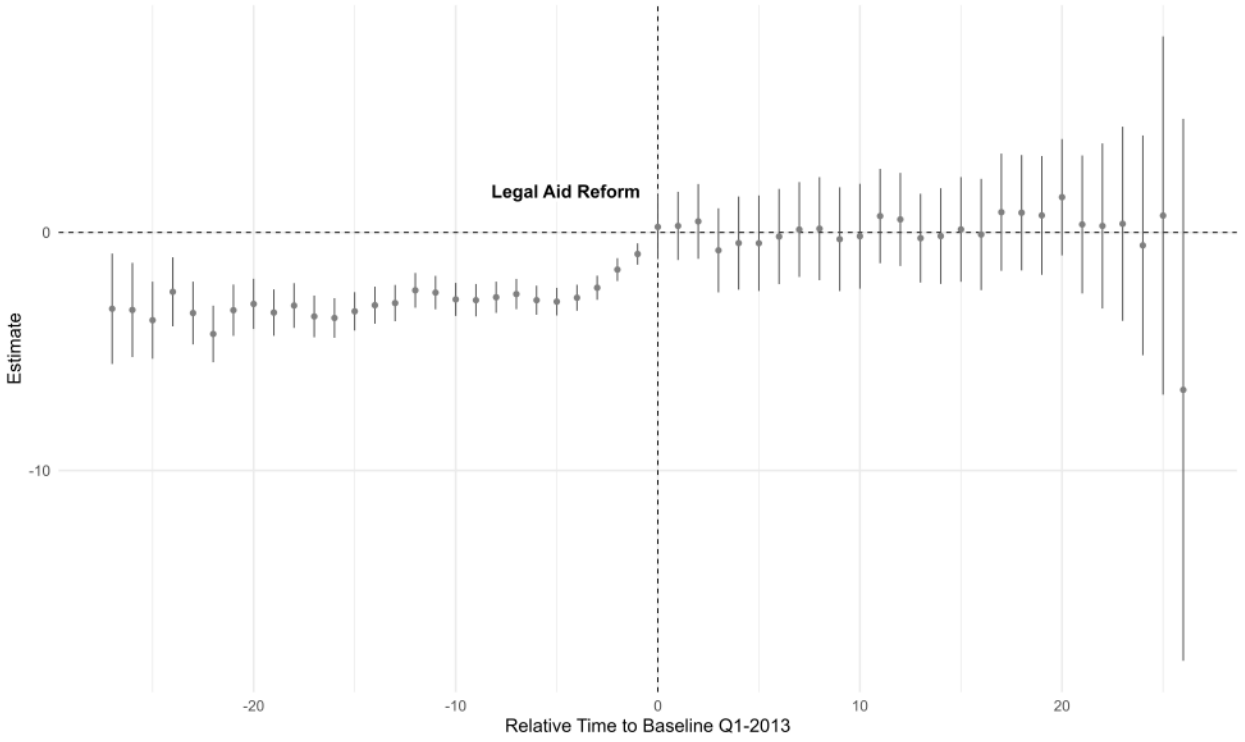


FIGURE 44: Households at risk of homelessness - Staggered

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.

E Appendix

E.1 Background Information

E.1.1 Eviction process in the county courts of England and Wales

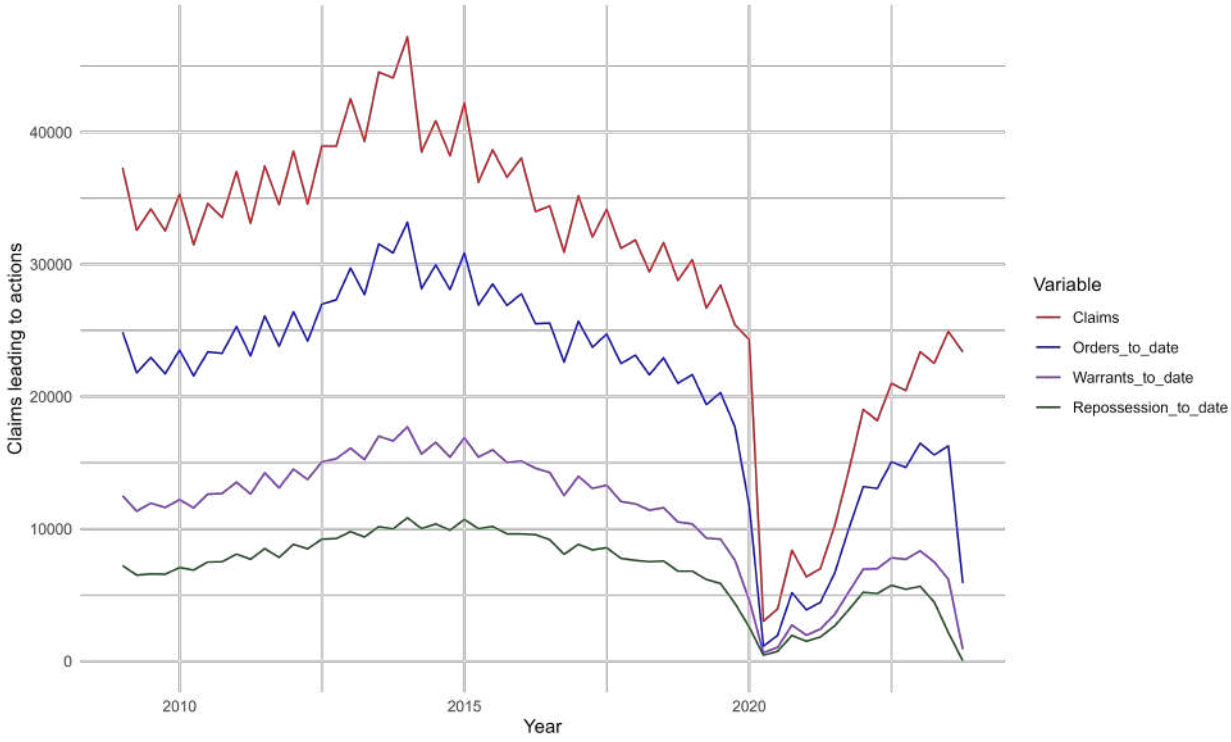


FIGURE 45: Number of possession claims that lead to orders, warrants and repossessions over time

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.

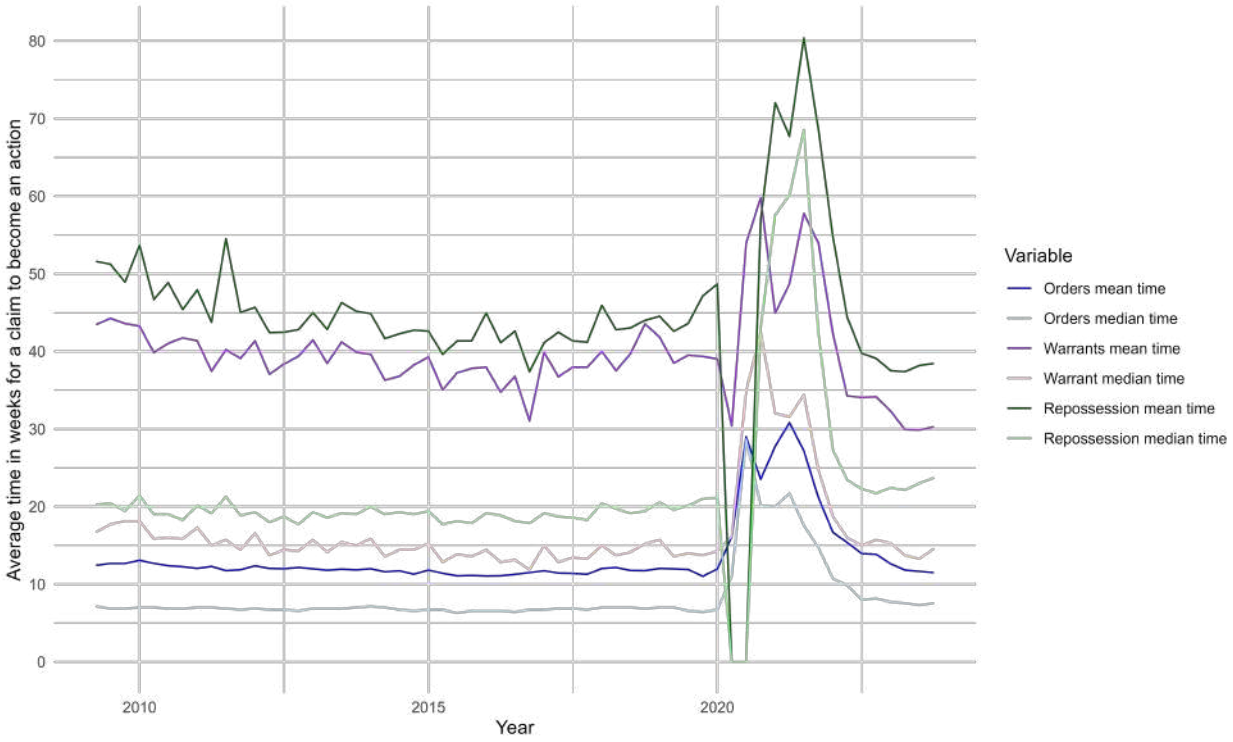


FIGURE 46: Average time for possession claims to become an order, warrant and repossession over time

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.

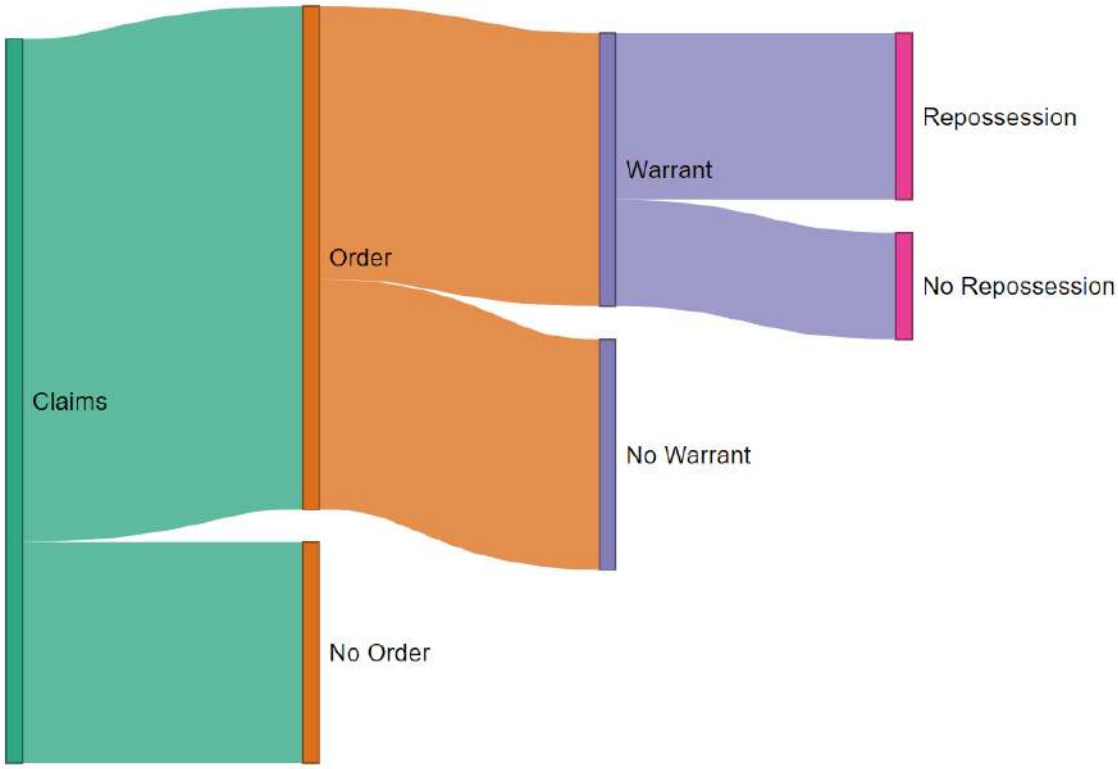


FIGURE 47: Flowchart eviction proceedings outcomes

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

E.2 Excess Death Definitions

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

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

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