

Does Access to Justice affect access to the economy?

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Abstract

This research examines the relationship between legal aid spending and unemployment, based on the hypothesis that legal aid supports property and employment rights, encouraging labour market participation. Using panel data from 160 local authorities between 2011 and 2022, the analysis combines legal aid spending from the Ministry of Justice; unemployment rates from the Annual Population Survey; and additional control variables for local authorities. A log-transformed fixed-effects model on legal aid per capita and unemployment shows a positive coefficient, significant at the 90% confidence level. However, this unexpected positive significance disappears once variables are first-differenced. Attempts to account for long-term effects using time-lagged variables were unsuccessful, as no lags were significant after differencing. While the findings do not support a clear relationship between legal aid spending and unemployment, they highlight a gap in empirical research in this area. This suggests caution for policymakers seeking to link legal aid spending to economic outcomes, and points to the need for further investigation.

AI Statement: I confirm that generative AI was not used in during the research or drafting of this paper.

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Introduction

In recent years, the UK justice budget has lagged behind inflation and population growth, with the cost effectiveness of individual elements of the budget being constant sources for debate, such as legal aid (UK Parliament Committees, 2024). The IFS estimates that justice spending remains below levels in 2002-03 when adjusted for population levels and inflation (Dominguez and Zaranko, 2025). However, little empirical work has asked how this decrease in real funding affects labour market outcomes. In conventional macroeconomic labour market models, employment is dictated by collective demand and supply decisions (Frank, 2020, p738). While macro-models do consider the influence of direct government policy such as minimum wage and income supplements, these models often ignore the influence of legal institutions by assuming employment and property rights are strongly protected (Hadfield, 2022).

Contrasting this, institutional economists argue that states must actively ensure property and contract rights as a prerequisite for economic activity (Acemoglu & Robinson, 2013). Empirical studies link weak legal systems to reduced investment and growth (Hadfield, 2022), and evidence suggests access to justice can alter incentives to work (Acemoglu, Johnson & Robinson, 2005). This research builds on these theories by testing the hypothesis that UK regional per-capita legal aid spending boosts employment by strengthening individuals' confidence in property and labour rights. legal aid is a government funded system through which individuals can receive free legal representation and advice. In line with the wider justice system, over the past decade real-term spending on legal aid has fallen (Rachel, Hazel, and Edwards, 2022). Figure 1 shows the flow of effects that this hypothesis proposes ties legal aid to unemployment.

Figure 1: Legal aid to Employment theory flow chart



Literature review

Institutional economics theory

A useful starting point for understanding the potential relationship between legal aid and employment lies within the framework of institutional economics. This field, specifically the area of ‘new institutional economics’ emphasises the role that institutions, and enforcement mechanisms play in shaping economic outcomes (Rutherford, 2001). In particular, the quality and accessibility of the justice system is considered essential to the stability and efficiency of modern economies (Alston and Mueller, 2025). Daron Acemoglu (2005) argues that a state’s capacity to facilitate growth depends in part on its ability to allocate resources through institutions like the legal system. These systems work by providing individuals and businesses with the certainty needed to engage in transactions, enforce contracts, and resolve disputes, which reduces transaction costs and increases efficiency. Extending this idea by assessing historical trends, Acemoglu, Johnson, and Robinson (2005) argue that institutional quality is a fundamental driver of cross-country income differences, with inclusive legal systems playing a pivotal role in promoting long-run investment and economic development.

From this perspective, legal aid, can be seen as a mechanism that supports labour market functioning via economic inclusion. By enabling individuals to resolve employment disputes, enforce their workplace rights, or access entitlements, legal aid reduces the burden of legal uncertainty and help sustain labour force participation. Acemoglu and Wolitzky (2011) emphasise the role of legal and institutional structures in mitigating coercive labour practices and fostering voluntary, productive labour relations. More conceptually, Hadfield and Bernier (2005), in their discussion of legal institutions underpinning contract enforcement, list legal aid among the mechanisms that support functional markets. In theory then, legal institutions, when made accessible through public funding, bolster the labour market by enhancing confidence, reducing risk, and supporting human capital investment. However, measuring the influence of legal aid in practice is empirically difficult, with the wide array of factors influencing individual employment choices, coupled with the differing legal systems in place throughout the world.

Methodological challenges

While institutional theory provides a rationale for how legal aid might influence labour market outcomes, research on this relationship remains limited. Although a considerable body

of literature discusses the virtues of an inclusive legal system and its theoretical economic benefits, there is a relative scarcity of econometric studies directly linking free legal services to employment. More critically, such studies are difficult to replicate or generalise due to cross-country variations in legal systems. Data constraints further compound these challenges. In many jurisdictions, detailed data on individual legal system users, particularly those eligible for publicly funded legal aid, are either unavailable or restricted (Rachel, Hazel, and Edwards, 2022).

A *Frontiers in Public Health* report by Rachel, Hazel, and Edwards (2022) attests to this difficulty in assessing wider literature. They highlight gaps in economic evaluations, particularly following the impact of the UK's LASPO Act (2012), which decreased legal aid funding. Nine studies were reviewed, eight from the UK and one from the US. Most used mixed methods, including case studies, randomized controlled trials, and quasi-experiments. Rachel, Hazel, and Edwards cite this variation in target groups, intervention types, and differences between the UK and US contexts as a key barrier to evaluating the effects of legal aid.

Abel and Vignola (2010), in their review of cost benefit analysis studies focused on civil legal aid also criticised the lack of long-term studies into its effects. They argued that many studies were limited in their discussion of the statistical significance of their results. They also suggest that the benefits of legal aid are likely to only be realised over longer periods of time. These findings reinforce the need for additional studies which assess the economic impact of legal aid.

Econometric research

Despite these limitations, several studies have attempted to estimate the effects of legal access on wider macroeconomic indicators. For example, Espinosa, Desrieux, and Ferracci's (2018) natural-experiment study of a 2008 French labour-court reform provides one of the most direct pieces of evidence that access to justice matters for local labour-markets. By studying the closure of roughly 23% of French labour courts in a difference-in-difference analysis, they show that an increase in the distance to the nearest tribunal reduced firm creation by 6.3%, job creation by 4.6%, and job destruction by 4.0%. These findings imply that when workers and employers face friction in the justice system, hiring slows.

Tsintzos and Plakandaras (2020) studied a weakly unbalanced panel of European countries to examine how aggregate judicial spending, encompassing courts, legal aid, and prosecution, affects productivity. Across pooled-OLS, fixed-effects, and random-effects specifications, they found that higher per-capita judicial budgets were positively and significantly associated with output per worker. Although the study did not isolate legal-aid spending from other components of the judicial budget, it suggests that broad investments in judicial capacity bolsters productivity by reducing legal uncertainty and enforcing contracts more effectively.

Deseau, Levai and Schmiegelow, (2019) also applied the panel approach, using the density of professional judges per 100,000 inhabitants as a proxy for the supply of accessible justice in a dynamic panel spanning 105 countries from 1970 to 2014. Their estimates indicate that a 1% increase in judge density raises the five-year GDP-per-capita growth rate by 0.86%, with diminishing returns and larger effects in lower-income economies. While their focus is on long-run growth rather than employment, they note that improved judicial capacity is linked in country-level case studies to higher entrepreneurship rates and more stable labour-market outcomes.

Supporting this, Aberra and Chemin's (2021) randomized field experiment in rural Kenya provides evidence of legal aid's impact on investment and credit. Through a difference-in-difference analysis of free legal representation, they observed significant improvements in households' property-rights security, which in turn lead to higher investment in productive assets and expanded access to credit. Compared to controls, treated households exhibited a 17% increase in days worked, showing that the provision of free legal services increased individuals' ability to participate in the labour market.

Cunningham's (2016) evaluation of the U.S. Federal Legal Services Program illustrates how legal aid can shape broader institutional behaviour. Analysing city-level rollouts of legal-aid grants from 1960 to 1985 using OLS and WLS regressions, he documents a short-run 7% rise in reported crimes and a 16% increase in crimes cleared by arrest, followed by gains in property values over a decade. These results suggest that the mere prospect of litigation, enabled by legal aid, can alter incentives and market dynamics, hinting at indirect channels through which legal-aid provision influences local conditions.

Wider Research

Despite the limited econometric studies linking legal aid directly to employment, a growing body of other research highlights the wider economic benefits of funded legal advice. A 2024 report by the Access to Justice Foundation and Bar Council estimates that every £1 invested in free specialist advice covering debt, welfare, housing, immigration and employment rights, returns £2.71 to the Treasury and generates £4.5 billion in social value across 500,000 beneficiaries (Munro and Preece, 2024). Drawing on provider data and success-rate assumptions, their model suggests that each advice recipient corresponds to an average of 0.39 additional employable household members, reflecting improvements in stability, health and labour-market readiness. While these results derive from simulation rather than panel-data estimation, they show the mechanics of how early legal intervention can unlock suppressed labour-supply.

There is also broad research discussing the impact of legal aid on the wellbeing of recipients, for example, Desmond (2015) demonstrated that access to legal representation in eviction proceedings can significantly reduce the risk of subsequent job loss. Similarly, Sandefur (2014) reviewed a range of RCTs in the United States showing that even modest legal assistance, such as providing a paralegal in small claims disputes, can improve both legal outcomes and broader socioeconomic indicators. In a survey of fifty Legal Aid cost benefit analysis studies on behalf of the World Bank, Harley, Capita, Markovic, Panter, and Scott-Moncrieff, (2019), identified employment prospects as a constant theme throughout the literature. However, the studies surveyed focus on a wide range of countries, and do not include econometric analysis conducted on the UK, suggesting a blind-spot that this research may be able to help fill.

Wider survey data also suggests a strong link between legal issues and employment. Through survey of individuals legal problems, Farrow, Currie, Aylwin, Jacobs, Northrup and Moore, (2016), of those surveyed with one or more legal problem, employment issues made up 16.4% of the total, the third most common issue behind consumer and debt matters. They argue that the legal issues individuals face, even those not directly related to employment, cause high levels of stress and loss of security, potentially leading to loss of employment and housing. A 2015 Citizens Advice network survey reported that 47% of advisers observe litigants-in-person in family courts suffering workplace disruptions, lost hours, strained

employer relations and even job loss, when forced to navigate complex procedures alone (Vaughan, 2015). In their study of Legal Aid in Scotland, the Law Society of Scotland argues that legal-aid representation alleviates these pressures by handling procedural burdens, thus enabling claimants to maintain stable employment and focus on their profession (Hammond and Vermeulen, 2017).

Summary

In summary, the literature suggests that there is evidence that the quality of the justice system and availability of legal aid influences employment, and broader economic indicators. Yet they also reveal limitations. Many studies such as Tsintzos and Plakandaras (2020) are based on aggregate justice spending data which includes legal aid but does not focus on its individual effect. Others focus on the growth effect of the justice system but do not consider its potential impact on labour, such as the research of Deseau, Levai and Schmiegelow, (2019). This research aims to disentangle this aggregation by focusing specifically on Local authority level legal aid and unemployment in the UK.

Data

Data Summary

To model the potential relationship between legal aid and unemployment, a balanced panel of data has been constructed at the local authority level for England and Wales between 2011 and 2022, excluding Scotland and Northern Ireland, which have independent Legal Aid systems. This couples Unemployment and legal aid spending per capita with a series of labour market control variables used throughout the literature. The panel does not include data for all local authorities, as suppressed and missing variables within the data have been removed, causing a portion of local authorities to be dropped. This process is explained in more detail within the data cleaning section below.

The final panel consists of 1920 observations for each variable, split across 160 local authorities, giving 12 periods of data for each authority. All data has been aggregated to the financial or calendar year and matched by Local Authority District. Table 1 shows summary statistics for this data, with further details on each variable below.

Table 1: Data summary statistics

Variable	Observations	Mean	ST-Dev	Min	Max	Kurtosis	Skewness
BOE	1920	0.79	0.43	0.17	1.75	2.97	0.42
CPI	1920	2.47	1.86	0.40	7.90	6.30	1.90
HPR	1920	7.59	3.45	3.17	34.77	10.47	2.13
LAC	1920	17.96	16.71	0.01	212.28	33.91	4.36
NVQ	1920	42.31	12.79	14.80	83.40	3.20	0.88
PRO	1920	32.67	7.75	17.80	72.20	5.93	1.41
SER	1920	0.37	0.06	0.25	0.63	4.76	1.16
UNP	1920	5.84	2.68	0.90	16.30	3.88	0.95

Civil legal aid per capita (LAC) is measured by regional civil legal aid spending using “Total Value” (in thousands of £) from the Ministry of Justice’s Legal Aid Agency quarterly releases ([link](#)), aggregated to annual totals and divided by ONS mid-year population estimates ([link](#)). This per-capita measure reflects the relative funding within each local authority across each period for free advice and representation in civil cases.

One significant assumption underpinning data is that the local authorities recorded are those where the legal aid provider is located, but not necessarily where the legal aid client is located. This assumes that clients do not need to travel far to reach their local legal aid office, however, this may not always be the case. The Ministry of Justice and legal aid Agency do not publish details of client office travel times, meaning we cannot know what effect this may have. For many larger local authorities this is unlikely to be an issue, however, for small authorities such as those within London, where an individual may easily travel to a different authority to receive aid, the data may be less reliable. For example, the maximum per capita value occurs within the City of London, (£3220.14), however, this may be an artifact of large numbers of offices concentrated within the city.

Unemployment rate (UNP) (%), has been sourced from the ONS NOMIS Unemployment rate - aged 16-64 ([link](#)). This is the main dependent variables to be investigated in this

research. It shows the number of individuals unemployed as a proportion of the active population. This is a particularly useful measure for this analysis as it controls for individuals who are seeking work but for exogenous reasons are not able to find it, possibly for legal reasons that this research hopes to detect.

Labour productivity (PRO) measured in output per hour worked, (£) has been sourced from the ONS Subregional productivity: labour productivity indices by local authority district, which publishes annual GVA per hour worked by Local authority ([link](#)). This measure helps to capture the labour productivity within each local authority, a factor that can influence local employment levels driven by the reallocation of workers to more efficient regions (Cruz, 2023).

Proportion of population with NVQ4+ qualifications (NVQ) was sourced from NOMIS annual population survey data via query was used ([link](#)). The data from 'Table 19 Qualification by age – NVQ, all people aged 16-64 NVQ4+' has been divided by the population data within each local authority to give a percentage. This controls for the rough education within each local authority. Theo Sparreboom and Anita Staneva (2014) found that education can be a strong predictor of employment, particularly amongst young people. This aligns with the work of Jianxian Chen, Xiaokuai Shao, Ghulam Murtaza, and Zhongxiu Zhao (2014) who show such a relationship emerging in China.

Annual Bank of England Bank Rate (BOE) is taken from the BoE's official historical series database ([link](#)). To extrapolate the actual rate within each year, the most recent base rate was used, or the average of rates across a year in cases where the rate changed multiple times in one year. These rates give a useful control for the costs of investment, and by proxy, the cost for expanding output and employment. The base rate influences firms' investment decisions and hiring capacity. Higher rates typically dampen capital expenditures and slow job creation.

Consumer price index and housing (CPI) is sourced from the CPIH Annual Rate, base-year 2015, published by ONS ([link](#)). Like the bank interest rate above, the rate of inflation influences individual and firm investment decisions. Inflation also affects real wages, which can influence labour supply decisions.

House-price-to-earnings ratio (HPR) (median price divided by median annual workplace earnings) for each Local authority is sourced from ONS's House price (existing dwellings) to

residence-based earnings ratio ([link](#)). This measure records the ratio between gross annual residence earnings and house prices. Controlling for this ratio helps to account for the long-term living costs of workers within each authority. High living costs may hinder labour mobility, especially for lower income workers. Housing supply has been shown to be a significant determinant of economic development at the regional level, and significant relationships can be found (Wouter Vermeulen, Jos van Ommeren, 2009). However, broader literature has not always been conclusive on this relationship (Robert A Moffitt, 2002).

The share of residents working in the service sector (SER), was used to capture the industrial composition within each local authority. This was sourced via query of the NOMIS annual population survey data via query ([link](#)). From ‘table 11a Employment by age and industry (SIC 2007), Aged 16 – 64, G-Q Total Services (SIC 2007)’. Total Services has been divided by the population within each local authority following the same method for calculating legal aid per capita. This gives a relative proportion of each local authority employed in service sector industry, helping to control for fluctuations in local employment caused by changes in local industrial makeup.

Software and data-cleaning

RStudio and R was used for data preparation and cleaning, utilising the packages tidyverse, readxl, runner, sf, tmap, vtable, moments, and gt. To build the models, and run statistical tests, the software Gretl was used. To construct the panel, missing entries were excluded. These missing observations were most often caused by suppressed or missing values marked as “-“ or “!” within in the annual population survey data. Alongside this, although most local authorities remain static, some changed over the period. Where it was not possible to align these local authorities, they have also been omitted from the data. Removing these missing values from all data fields resulted in a remaining sample of 1920 entries.

This process ensured that all remaining local authorities shared a complete time series and that the Gretl regressions ran without error. Specifically, balanced panels are required to test for a unit-root via the Im, Pesaran, and Shin test used later. All the data cleaning combined left 160 Local authorities remaining across the total 317 in the UK. There is risk here that missing variables and omitted local authorities could produce bias within the results of the models if the missing and omitted values are non-random (Wooldridge, 2019, pp468-469). However, to improve the proportion of local authorities represented in this sample would

require more detailed data sets with fewer missing and omitted values, this may be possible utilising unpublished APS and CC data, but it was not possible for this research because of the focus on publicly available data sets.

Natural Log transformations

The high levels of skewness and kurtosis within HPR, LAC, NVQ, POD, PRO, SER, and UNP show that the data is skewed toward one end of each distribution and has tails of anomalies longer than the normal distribution (Table 1). For use in the model estimations, each these variables have been transformed into their natural log, indicated by a log prefix. Applying the natural log transformation reduces the influence of extreme values, compresses the scale of the data, and helps to normalize the distribution, making the variables more suitable for linear regression models (Wooldridge, 2019, p187). The improvement made by the natural log can be seen in the two figures below (Figure 1 and 2), which show a histogram of legal aid per capita with and without natural log. This process aligns with the approach of Deseau, Levai and Schmiegelow, (2019), who apply natural logs to their Access to justice and GDP per capita measures.

Figure 2: Legal aid per-capita Histogram

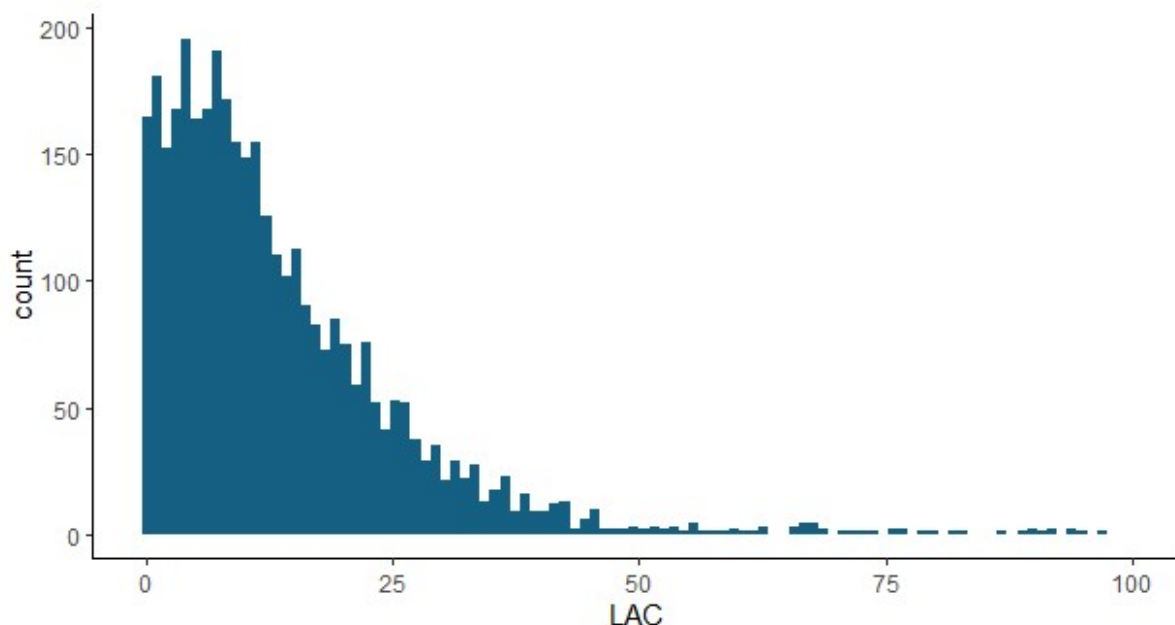
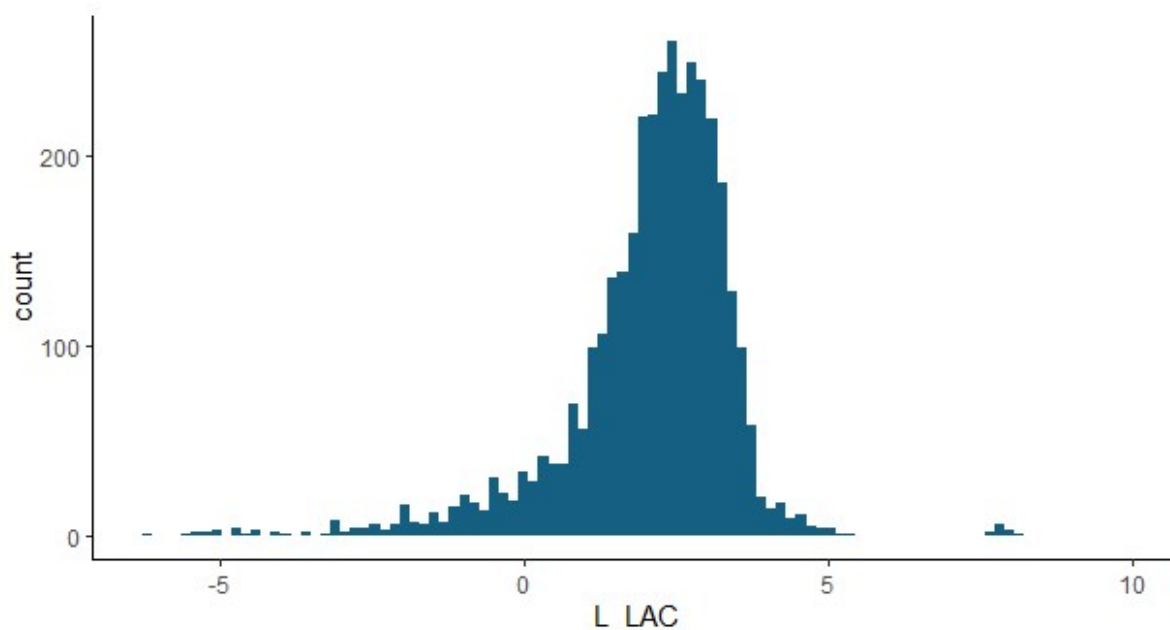


Figure 3: Log of Legal aid per-capita Histogram



This has important implications for the interpretations of the model specifications, specifically in the case of the dependent and explanatory variables logUNP and logLAC. The interpretation of a log-log β_t coefficient is elasticity, or a one percent change in X results in a β_t percentage change in Y, (Wooldridge, 2019, p187).

First difference transformations

To test for stationarity within the data, an Im, Pesaran, and Shin test was run for each variable. This test applies an Augmented Dickey-Fuller test across all complete local authority groups within the dataset and returns an average test statistic (Baltagi, B H. 2021, p344). The IPS test statistics for many variables are not more negative than the critical values at 10% significance level (-1.64), meaning that we cannot reject the null hypothesis of a unit root (Table 2). The null hypothesis can however be rejected for logLAC (-1.83731), which can be rejected at the 1% level (-1.75). The test statistic for logUNP is (-1.613), which is close to the 10% significance threshold of (-1.64), suggests weak evidence against the null of a unit root, despite those found within the control variables. While failing to reject the null does not definitively confirm the data is nonstationary, the test provides no evidence against a unit root which may result in spurious regression results.

If the series is integrated of order one, I(1), taking the first difference is a standard transformation to induce stationarity, making the variable suitable for regression analysis (Wooldridge, 2019, p380). To trial this, the first difference of each variable was calculated

and re-tested using IPS. As with the natural log transformations, this approach also matches that taken by Deseau, Levai and Schmiegelow, (2019). The results indicate that, for all variables except $\Delta\log\text{PRO}$, the null hypothesis can now be rejected. This implies that the differenced variables are stationary and valid for use in a fixed effects panel model. However, since $\Delta\log\text{PRO}$ remains non-stationary, its inclusion could lead to spurious results. Therefore, $\Delta\log\text{PRO}$ was excluded from the final model specifications. Table 2 shows these results side by side.

Table 2: Im, Pesaran, and Shin, test results

Variable	IPS T-bar result	Variable	IPS T-bar result
logLAC	-1.83731	$\Delta\log\text{LAC}$	-3.52249
logPRO	-0.01354	$\Delta\log\text{PRO}$	-1.51564
logUNP	-1.61312	$\Delta\log\text{UNP}$	-4.14581
logNVQ	-1.21802	$\Delta\log\text{NVQ}$	-4.09317
logCPI	-1.57118	$\Delta\log\text{CPI}$	-2.08383
logBOE	-2.37468	$\Delta\log\text{BOE}$	-2.27434
logSER	-1.95118	$\Delta\log\text{SER}$	-4.15067
logHPR	-1.30962	$\Delta\log\text{HPR}$	-3.72528

Although first differencing eliminates potential issues from non-stationarity, it also removes information about long-run trends and persistent relationships between variables (Wooldridge, 2019, p386). Because of this, and the evidence against a unit root within the two main variables of interest, logLAC and logUNP, both the differenced and non-differenced models will be shown as results, while the differenced approach is preferred for the reasons outlined above.

Methodology

Functional form and Pooled OLS

To begin investigating the relationship between legal aid provision and regional unemployment, the analysis employed a pooled Ordinary Least Squares (OLS) model. This initial approach provided a useful benchmark and allowed for the exploration of the functional form linking legal aid expenditure per capita to changes in unemployment rates. The pooled OLS approach returned the following coefficients, (for significance of control variables see Annex, Model 1 and 2).

Table 3: Initial Pooled OLS results

Model	Variable	Coefficient	Std. Error	t-ratio	p-value
OLS Log	logLAC	0.202286	0.0395481	5.115	<0.0001 ***
OLS ΔLog	ΔlogLAC	0.0128515	0.0359071	0.3579	0.7209

Note: Confidence intervals: 99***, 95** and 90*.

The results are only statistically significant for the Log legal aid per capita model, not the differenced result. The functional form taken forward was a collection of the variables found to be significant across either of the initial OLS results (See annex Pooled OLS). This resulted in the following functional forms:

Model 1: using a log-log approach

$$\log UMP_t = \beta_0 + \beta_1 \log LAC_t + \beta_2 \log NVQ_t + \beta_3 \log CPI_t + \beta_4 \log BOE_t + \beta_5 \log SER_t + \beta_6 \log HPR_t + u_t$$

Model 2: using a delta log-delta log approach

$$\Delta \log UMP_t = \beta_0 + \beta_1 \Delta \log LAC_t + \beta_2 \Delta \log NVQ_t + \beta_3 \Delta \log CPI_t + \beta_4 \Delta \log BOE_t + \beta_5 \Delta \log SER_t + \beta_6 \Delta \log HPR_t + u_t$$

While pooled OLS was helpful for suggesting a functional form, it relies on strong assumptions, most notably, that there are no unobserved factors correlated with the regressors and that the error term is homoscedastic and serially uncorrelated (Wooldridge, 2019, pp54-57). In the context of legal aid and employment, this is unlikely to hold. Local authorities may differ in unobserved, time-invariant ways, such as historical economic structures,

institutional capacity, or persistent demographic characteristics which could confound the results if not controlled.

Fixed effects

To account for such unobserved heterogeneity, the analysis transitioned to fixed and random effects approach, which are designed to address limitations of pooled OLS in panel data contexts (Baltagi, 2021, p16). Fixed effects models are particularly useful when unobserved variables are correlated with regressors, while random effects models are more efficient when the strict exogeneity assumption is plausible. Fixed effects models were favoured for their ability to difference out unobserved, time-invariant factors such as historical economic conditions or geographic traits that might bias the estimates. This is done by applying a fixed coefficient across each individual local authority, and across each year included.

This is particularly important in a context where time-invariant differences between local authorities, such as historical economic conditions, institutional structures, or geographical factors, could bias the estimates. For example, rural areas may face structural barriers such as access to legal services where individuals must travel larger distances to reach their local office. This difficulty for rural clients of legal aid is commonly cited by legal aid providers (The Law Society, 2024). Alternatively, areas with lower legal need for unobserved reasons may have lower demand for legal aid. By transforming the data to control for individual-specific effects, the FE model isolates this potential regional variation, helping to isolate the relationship between legal aid and employment.

Fixed effects also help to control for time variation shocks, such as the COVID-19 pandemic or strikes in the Judicial system, when combined with time dummies, by accounting for common trends across all regions. However, the model cannot estimate coefficients on time-invariant variables within each group and relies on the assumption of strict exogeneity, meaning that explanatory variables must not be correlated with time-varying errors. Many of these variations, however, can be captured within the time-variant controls included alongside the main explanatory variable. Despite these limitations, the fixed effects model produces more reliable estimates of the impact of legal aid spending over time, even though time-varying unobserved factors may still affect the results.

Breusch-Pagan test

A Breusch-Pagan test was used to determine whether there is significant panel-level heterogeneity that would justify using a Fixed or Random effects model over a pooled OLS approach (Baltagi, B H. 2021, p81). The test returned a p-value near zero, indicating that there was statistically significant variation across panels (See Annex: Model 5). The null hypothesis was therefore rejected, meaning that each local authority has a unique intercept, making the Fixed effects model appropriate than the pooled OLS approach.

Hausman test

The Hausman test was used to choose between a fixed effects or random effects model by testing whether the individual-specific effects are correlated with the explanatory variables (Baltagi, B H. 2021, pp89-90). In this case, the test returned a p-value near zero, leading to the rejection of the null hypothesis that there is no correlation (See Annex: Model 5). This implies a violation of the random effects assumption, meaning that the random effects estimator is inconsistent. As a result, the fixed effects model is preferred, as it allows for arbitrary correlation between the unobserved individual-specific effects and the regressors, thereby providing consistent estimates (Wooldridge, 2019, pp380-81).

Although the random effects model produced statistically significant results, the fixed effects estimates are likely to be more robust and are therefore used as the primary basis for interpreting the relationship between legal aid per capita and employment. The estimated coefficients for legal aid per capita are substantively similar across both models. This consistency suggests that the main relationship is not strongly influenced by time-invariant unobserved heterogeneity, which helps to confirm the robustness of this approach. The random effects specification is included within the annex for completeness but are not discussed further here.

Autocorrelation and Heteroscedasticity

To test for the presence of autocorrelation in the final model, a Wooldridge test for autocorrelation in panel data was conducted (Baltagi, B H. 2021, pp139-140). The test returned a p-value close to zero, leading to the rejection of the null hypothesis of no first-order autocorrelation. This suggests that autocorrelation is present, which can result in underestimated standard errors if not properly addressed. Alongside this, a Wald test was performed to assess the presence of heteroscedasticity, also returning a near zero p-value. The

rejection of the null hypothesis in this case indicated that the error variance was not constant across observations, suggesting that heteroscedasticity was present (Wooldridge, 2019, p564).

Given the presence of both autocorrelation and heteroscedasticity, Arellano robust standard errors were applied to all model estimates reported within this research. While this correction improves the reliability of inference by providing valid standard error estimates under these conditions, it does come at the cost of reduced efficiency. In particular, standard errors tend to be larger, which lowers the precision of coefficient estimates and makes statistical significance harder to achieve. This occurs because for most cases the errors are positively serially correlated (Wooldridge, 2019, p400).

Time lags

The first differencing of variables to solve the unit root issue discussed in the Data section comes at the cost of a loss of long-term information held within each level variable. To test methods accounting for this, lagged values of legal aid expenditure per capita were tested within the fixed effects model. This allows for the identification of delayed responses in employment outcomes while helping to mitigate reverse causality (Wooldridge, 2019, p380). Specifically, if higher legal aid spending is both a response to and caused by regional unemployment, current-period legal aid levels may be endogenous, and the benefits of legal aid may only be seen in prior periods. By using lagged regressors, the model exploits time-variation in legal aid that is less likely to be simultaneously determined with current unemployment levels, potentially reducing endogeneity and offering a more credible estimation.

However, the use of lagged variables introduces several limitations. First, if the unobserved determinants of unemployment are serially correlated, lagged legal aid variables may still be correlated with the error term, violating the strict exogeneity assumption and leaving endogeneity concerns unresolved. Second, including multiple lags increases the dimensionality of the model, potentially leading to multicollinearity, inflated standard errors, and reduced statistical power (Wooldridge, 2019, pp400-401). Furthermore, with a relatively short panel, the inclusion of several lagged terms reduces degrees of freedom and may overfit the data.

To explore the potential presence of a lag structure, a series of fixed effects models incorporating progressively lagged values of legal aid per capita were estimated. The table

below summarises these results, where each lag variable was tested alongside all control variables (Table 4). Lags of log legal aid per capita returned significant results only in the pre-differenced values, while all lags tested of the log difference approach returned insignificant results. This suggests that the introduction of lag variables does not recover any efficiency lost through the process of first differencing for this data. As the differenced approach was suggested to be the more robust estimation via IPS testing, and this approach shows no significance across lag variables for legal aid, lags were not included within the final models.

Table 4: Lag Legal aid per-capita p-value and Akaike criterion results

	Log Akaike criterion	Log P-value		Log difference Akaike criterion	Log difference P-value
LAC	704.2772	0.0752*	Δ LAC	1263.998	0.2341
LAC_1	688.9239	0.0640*	Δ LAC_1	1271.080	0.9293
LAC_2	622.2316	0.0018***	Δ LAC_2	1247.997	0.4302
LAC_3	529.4402	0.0135**	Δ LAC_3	1217.728	0.8478
LAC_4	479.9745	0.1724	Δ LAC_4	1166.219	0.5195

Note: Confidence intervals: 99***, 95** and 90*.

Results

The table below shows the coefficients, standard errors and significance for each of the tested variables for the two main models, with their meaning discussed below. For robustness, both differenced and non-differenced approaches are compared.

Table 5: Results for Log fixed effects, Log difference fixed effects, and Log difference random effects

Variable	Model 1 Log fixed effects	Variable	Model 2 Log difference fixed effects
Constant	4.042 *** (0.456)	Constant	−0.053 *** (0.0053)
logLAC	0.010 * (0.056)	ΔlogLAC	0.055 (0.037)
logNVQ	−0.782 *** (0.104)	ΔlogNVQ	−0.029 (0.128)
logCPI	0.010 (0.010)	ΔlogCPI	−0.008 (0.011)
logBOE	−0.010 ** (0.012)	ΔlogBOE	−0.069 *** (0.011)
logSER	−2.002 *** (0.186)	ΔlogSER	−1.960 *** (0.206)
logHPR	−0.916 *** (0.103)	ΔlogHPR	−0.275 * (0.146)
Akaike criterion	783.223	Akaike criterion	1293.770
Adjusted R-squared	0.466	Adjusted R-squared	0.112
LSDV R-Squared	0.664	LSDV R-Squared	0.126
Observations	1920	Observations	1760

Note: Results rounded to three decimal places. Confidence intervals: 99***, 95** and 90*.

Model 1, Log fixed effects

In the log difference fixed-effects model, Model 1, legal aid expenditure per capita returns a positive coefficient of 0.01 that is statistically significant at the 90% confidence interval. This result suggests that for each 1% increase in legal aid per capita, unemployment is projected to

increase by 0.01%. Alongside this, the adjusted R-squared of 0.46 suggests that the model explains 46% of the variation in unemployment through these estimators. Although Model 1 reports the lowest Akaike Information Criterion (783), a measure of the model's entropy relative to other models, and hence appears best by that metric, its advantage is largely mechanical (Akaike, H. 1974). The first differencing in Model 2 removes one observation per panel year and strips out low-frequency variation, lowering the log-likelihood and inflating AIC. Given that Model 1 violated stationarity testing, its superior AIC cannot outweigh the risk of spurious regression.

While this is partly consistent with the initial hypothesis that legal aid provision does influence unemployment, it is inverse to the relationship that institutional economic theory and wider commentary on legal aid suggests. This may be in-part due to a potential bi-directional relationship between legal aid and unemployment. For example, local authorities experiencing rising unemployment face heavier legal-aid caseloads. Given that the time-lag variables tested did not separate out a potential bi-directional relationship and showed no significance within the preferred differenced approach, little concrete information can be concluded from this result. Further research may consider using data aggregated at the individual level to better capture the behaviour changes, though this data is not published by the Ministry of Justice, it may be accessible to researchers working with them.

The control variables for house price ratio (HPR), service sector proportion (SER), and education levels (NVQ), are each found to be statistically significant to the 99% confidence interval. Similarly, the Bank of England base rate (BOE) is found to be significant within this model to the 95% confidence interval. The results suggest that a 1% increase in the proportion of individuals with NVQ4+ qualifications reduces unemployment by 0.78%. This aligns with the work of Theo Sparreboom and Anita Staneva (2014) discussed above. A 1% increase in the Bank base rate is estimated to reduce employment by 0.03%, aligning with the theory outlined within the data section. Similarly, a 1% increase in the house price ratio, effectively decreasing house price affordability, is estimated to reduce employment by 0.91%. This too aligns with the literature (Wouter Vermeulen, Jos van Ommeren, 2009).

Model 2, Log difference fixed effects

When the stationarity problem is remedied by first-differencing the variables, the significance of legal aid per capita disappears. While the coefficient of 0.055 suggests that with a 1%

increase in legal aid per capita, unemployment is projected to increase by 0.055%, this estimate is not statistically different from zero and therefore provides no robust evidence of an effect. The fragile nature of the result suggests that the apparent relationship in Model 1 is driven by common trends rather than a true behavioural link. In short, once non-stationarity is addressed, the data provides no reliable evidence that higher legal-aid spending causes higher or lower unemployment.

This contrasts with the work of, Espinosa, Desrieux, and Ferracci's (2018), Tsintzos and Plakandaras (2020), and Deseau, Levai and Schmiegelow, (2019), whose panel approaches found statistically significant relationships between access to justice and macroeconomic indicators. A potential reason for this difference may be that their research focused on wider or more proximate indicators such as aggregate judicial spending and judges per inhabitant. When considered as a whole, the justice system may demonstrate significant effects on unemployment, however, the results of this research suggest that such a relationship does not hold when applied to the individual funding for legal aid.

Alternatively, this result may underestimate the benefits of legal aid as outlined by Abel and Vignola (2010) because of the exclusion of long-term effects. First differencing the data within this model has excluded any longer-term influence that legal aid may have on employment, however, when lag-variables were tested for this data (See Time lags section above), results across all lags were insignificant. Tsintzos and Plakandaras (2020) tested for but found no unit-root within their data, and thus did not make a first-difference transformation, which may be a significant cause of the difference in results.

Adjusted R squared values echo this, with Model 2 explaining only about 11% of the variation in unemployment, contrasted to the 46% of variation in unemployment explained within Model 1, this significant decrease suggests the loss of long-run effects by first differencing the data. Although this process has removed the unit-root issue present in many of the variables used, it has come at the cost of a large portion of the predictive power of the model.

Of the control variables, education levels (NVQ) lost its significance when first differenced in Model 2. As with legal aid, this may too be due to the loss of long-term effects caused by differencing the data. The control variables for house price ratio (HPR), service sector proportion (SER), and the Bank of England base rate (BOE) all remained significant, with the

base rate's significance increasing to 99% confidence interval, while the housing price ratio's significance fell to the 90% confidence interval. Importantly, all coefficients maintained the same sign, showing that regardless of the model each control variable maintains the same relationship. This helps to confirm the robustness of the model regarding controlling for local economic conditions that influence employment.

Conclusion

The findings of this research suggest that the initial hypothesis, that legal aid spending has a statistically significant effect on unemployment, cannot be confirmed. Although a non-differenced specification indicated statistical significance, the IPS tests for non-stationarity suggests that these effects are spurious. This result stands in contrast to institutional economic theory, which suggests that publicly funded legal aid, by underpinning property and contract rights, should reduce transaction costs, bolster trust in the justice system, and encourage labour-market participation. Yet the fixed-effects estimates reveal no statistically significant effect of per-capita legal aid spending on unemployment once non-stationarity is addressed.

The absence of a direct relationship may reflect the indirect, long-run nature of institutional change, legal aid budgets alone may not immediately translate into the formal legal solutions. At the same time, it highlights the empirical difficulty of isolating a single institutional metric, when many mechanisms (court efficiency, legal literacy, enforcement norms) determine how legal institutions shape economic outcomes. While institutional economics provides a powerful narrative for why access to justice should matter for employment, these results show that such a clean relationship is difficult to prove using the data available.

Methodologically, the log differenced fixed-effects model controls for time-invariant regional characteristics and ensures stationarity of the variables used, but this comes at a cost of sustained long-term effects of legal aid. Additionally, the low R-squared shows that much of the variation in unemployment remains unexplained. Although this research experimented with including lags of per-capita legal aid spending in the fixed-effects specification and found none of statistical significance, future research might benefit from more explicitly modelling the dynamic relationship between legal aid and unemployment with wider data. An instrumental-variables approach or dynamic panel estimators could offer a more nuanced

view of how changes in legal aid funding unfold over time (Wooldridge, 2019). Testing this hypothesis may also be improved with richer data aggregated at the legal aid client level rather than the legal aid provider level. This approach may be possible with internal data gathered by the Ministry of Justice and available to associate researchers, but not available publicly.

For policymakers, these results caution viewing legal aid as influencing employment despite the suggestions of some stakeholders. While legal-aid provision remains vital for ensuring fair access to justice and upholding social cohesion, this research suggests that its effect on unemployment is limited.

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Annex

Model 1: Pooled OLS, log

Dependent variable: logUNP

Standard errors clustered by unit

	Coefficient	Std. Error	t-ratio	p-value	
const	2.25912	0.459978	4.911	<0.0001	***
logLAC	0.232361	0.0385095	6.034	<0.0001	***
logNVQ	-0.481732	0.0909731	-5.295	<0.0001	***
logCPI	-0.0629724	0.0110414	-5.703	<0.0001	***
logBOE	0.0805456	0.0113833	7.076	<0.0001	***
logSER	-0.428856	0.210006	-2.042	0.0428	**
logHPR	0.116160	0.0893679	1.300	0.1956	

Mean dependent var	1.659598	S.D. dependent var	0.469460
Sum squared resid	312.9558	S.E. of regression	0.404468
R-squared	0.260036	Adjusted R-squared	0.257715
F(6, 159)	39.08382	P-value(F)	6.11e-29
Log-likelihood	-982.9043	Akaike criterion	1979.809
Schwarz criterion	2018.729	Hannan-Quinn	1994.129
rho	0.599361	Durbin-Watson	0.740048

Model 2: Pooled OLS, Delta-Log

Dependent variable: $\Delta \log \text{UNP}$

Standard errors clustered by unit

Coefficient	Std. Error	t-ratio	p-value
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const	-0.0537227	0.00619171	-8.677	<0.0001	***
$\Delta \log \text{LAC}$	0.0545130	0.0362400	1.504	0.1345	
$\Delta \log \text{NVQ}$	-0.0266112	0.126031	-0.2111	0.8330	
$\Delta \log \text{CPI}$	-0.00853295	0.0106330	-0.8025	0.4235	
$\Delta \log \text{BOE}$	-0.0687722	0.0110871	-6.203	<0.0001	***
$\Delta \log \text{SER}$	-1.93518	0.198365	-9.756	<0.0001	***
$\Delta \log \text{HPR}$	-0.245371	0.141409	-1.735	0.0846	*
Mean dependent var	-0.077964	S.D. dependent var	0.340104		
Sum squared resid	180.7101	S.E. of regression	0.321070		
R-squared	0.111834	Adjusted R-squared	0.108795		
F(6, 159)	23.44684	P-value(F)	9.73e-20		
Log-likelihood	-494.2977	Akaike criterion	1002.595		
Schwarz criterion	1040.907	Hannan-Quinn	1016.754		
rho	-0.427053	Durbin-Watson	2.499279		

Model 3: Fixed-effects, Log

Included 160 cross-sectional units

Time-series length = 12

Dependent variable: logUNP

Standard errors clustered by unit

	Coefficient	Std. Error	t-ratio	p-value	
const	3.95858	0.445170	8.892	<0.0001	***
logLAC	0.104123	0.0543420	1.916	0.0572	*
logNVQ	-0.768642	0.104673	-7.343	<0.0001	***
logBOE	-0.0248980	0.0124388	-2.002	0.0470	**
logSER	-2.00847	0.186856	-10.75	<0.0001	***
logHPR	-0.903535	0.100731	-8.970	<0.0001	***
Mean dependent var	1.659598	S.D. dependent var	0.469460		
Sum squared resid	142.2764	S.E. of regression	0.284726		
LSDV R-squared	0.663596	Within R-squared	0.465802		
Log-likelihood	-226.1454	Akaike criterion	782.2909		
Schwarz criterion	1699.704	Hannan-Quinn	1119.842		
rho	0.225458	Durbin-Watson	1.381732		

Joint test on named regressors -

Test statistic: $F(5, 159) = 148.907$

with p-value = $P(F(5, 159) > 148.907) = 4.22893\text{e-}58$

Robust test for differing group intercepts -

Null hypothesis: The groups have a common intercept

Test statistic: Welch $F(159, 594.0) = 7.00174$

with p-value = $P(F(159, 594.0) > 7.00174) = 4.17249\text{e-}69$

Test for omission of variables -

Null hypothesis: parameters are zero for the variables

logCPI

Test statistic: $F(1, 159) = 1.03691$
 with p-value = $P(F(1, 159) > 1.03691) = 0.310089$

Distribution free Wald test for heteroskedasticity -
 Null hypothesis: the units have a common error variance
 Asymptotic test statistic: $\text{Chi-square}(160) = 2116.79$
 with p-value = 0

Test for normality of residual -
 Null hypothesis: error is normally distributed
 Test statistic: $\text{Chi-square}(2) = 71.0715$
 with p-value = $3.68994\text{e-}16$

Model 4: Fixed-effects, Delta-Log

Included 160 cross-sectional units

Time-series length = 11

Dependent variable: $\Delta \log \text{UNP}$

Standard errors clustered by unit

	Coefficient	Std. Error	t-ratio	p-value	
const	-0.0528557	0.00526989	-10.03	<0.0001	***
$\Delta \log \text{LAC}$	0.0545558	0.0372336	1.465	0.1448	
$\Delta \log \text{NVQ}$	-0.0287631	0.128399	-0.2240	0.8230	
$\Delta \log \text{CPI}$	-0.00835512	0.0106512	-0.7844	0.4340	
$\Delta \log \text{BOE}$	-0.0691128	0.0111053	-6.223	<0.0001	***
$\Delta \log \text{SER}$	-1.95972	0.205522	-9.535	<0.0001	***
$\Delta \log \text{HPR}$	-0.274850	0.146399	-1.877	0.0623	*

Mean dependent var	-0.077964	S.D. dependent var	0.340104
Sum squared resid	177.9767	S.E. of regression	0.334147
LSDV R-squared	0.125269	Within R-squared	0.112771
Log-likelihood	-480.8851	Akaike criterion	1293.770
Schwarz criterion	2202.300	Hannan-Quinn	1629.524
rho	-0.434438	Durbin-Watson	2.538236

Joint test on named regressors -
 Test statistic: $F(6, 159) = 22.7929$
 with p-value = $P(F(6, 159) > 22.7929) = 2.69283\text{e-}19$

Robust test for differing group intercepts -
 Null hypothesis: The groups have a common intercept
 Test statistic: Welch $F(159, 540.0) = 0.179205$
 with p-value = $P(F(159, 540.0) > 0.179205) = 1$

Distribution free Wald test for heteroskedasticity -
 Null hypothesis: the units have a common error variance
 Asymptotic test statistic: $\text{Chi-square}(160) = 3296.33$
 with p-value = 0

Model 5: Random-effects (GLS), Delta-Log

Included 160 cross-sectional units

Time-series length = 11

Dependent variable: $\Delta \log \text{UNP}$

Standard errors clustered by unit

	Coefficient	Std. Error	z	p-value	
const	-0.0537227	0.00619171	-8.677	<0.0001	***
$\Delta \log \text{LAC}$	0.0545130	0.0362400	1.504	0.1325	
$\Delta \log \text{NVQ}$	-0.0266112	0.126031	-0.2111	0.8328	
$\Delta \log \text{CPI}$	-0.00853295	0.0106330	-0.8025	0.4223	
$\Delta \log \text{BOE}$	-0.0687722	0.0110871	-6.203	<0.0001	***
$\Delta \log \text{SER}$	-1.93518	0.198365	-9.756	<0.0001	***
$\Delta \log \text{HPR}$	-0.245371	0.141409	-1.735	0.0827	*

Mean dependent var	-0.077964	S.D. dependent var	0.340104
Sum squared resid	180.7101	S.E. of regression	0.320979
Log-likelihood	-494.2977	Akaike criterion	1002.595
Schwarz criterion	1040.907	Hannan-Quinn	1016.754
rho	-0.434438	Durbin-Watson	2.538236

'Between' variance = 0

'Within' variance = 0.111654

theta used for quasi-demeaning = 0

Joint test on named regressors -

Asymptotic test statistic: Chi-square(6) = 140.681

with p-value = 7.1983e-28

Breusch-Pagan test -

Null hypothesis: Variance of the unit-specific error = 0

Asymptotic test statistic: Chi-square(1) = 61.2279

with p-value = 5.08363e-15

Hausman test -

Null hypothesis: GLS estimates are consistent

Asymptotic test statistic: Chi-square(4) = 9.84407

with p-value = 0.0431378