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FISCAL ANARCHY IN THE U.K.
MODELLING POLL TAX NONCOMPLIANCE

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ABSTRACT

The U.K.'s experience with the poll tax reminds us that even in an economy with a relatively well developed detection and legal system, one cannot take tax compliance for granted. The experience of the poll tax provides a unique opportunity to study many dimensions of tax compliance. We model nonpayment rates in a short panel of data on the 366 English local authorities. The transparent observability of individual and aggregate liabilities makes reliable measurement of rates of nonpayment possible. Moreover, these rates rose to unprecedented levels as well as exhibiting considerable variation across authorities. This, together with the variation in local taxes both between districts and over time, creates an ideal opportunity for empirical investigation. Our empirical specification allows us to investigate the determinants of compliance as a function of authority characteristics from census and other geographical data. Moreover, the analysis takes seriously the possibility of neighbourhood influences across authority boundaries. Our empirical results confirm the idea that higher taxes lead to larger compliance problems and that attempts to enforce compliance have a positive effect. Neighbourhood effects on non-compliance were less conspicuous, figuring significantly, if at all, only in the final year.

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1 Introduction

One of the most notorious fiscal reforms of recent times occurred when the Thatcher government in the UK introduced a poll tax to replace a long-standing system of local property taxation. Such taxes most often occupy the pages of public finance textbooks and are rarely seen in practice, most obviously because they are regarded as unfair. Experience with the poll tax contains many lessons for public finance economics. One of its most serious problems, and one which was virtually unanticipated, was that of non-compliance. A combination of political protest, perceptions of unfairness and substantial increases in average tax rates in the year of introduction undermined willingness to pay to the extent that nonpayment rose well above fifty per cent in a number of areas. In fact this problem became so severe, along with the social unrest manifested in the poll tax riots of 1990, that abandonment of the tax was promised within the first year of its operation, following the replacement of Mrs Thatcher as prime minister.

In understanding the problem of inducing compliance with the poll tax, we emphasise the possible importance of three phenomena which have heretofore received scant attention in the empirical analysis of tax compliance. First, is the issue of learning. How do individuals appraise the possible costs and benefits of compliance when a new system is put in place? One important way, which echoes ideas in the recent literature on herd behaviour in economic situations, is by observing the behaviour of others (see Banerjee (1992), Sah (1991)). The second, related issue is social influences on compliance, as for example via some stigma that surrounds non-payment. Third is the issue of political rebellion if the system of taxes is perceived to be unjust. From the Boston Tea party to the Thatcher poll tax the unwillingness of the population to comply with a tax that is deemed unjust has been a catalyst for political action[2].

The poll tax provides a unique opportunity to study some of these dimensions of tax compliance. We model nonpayment rates in a short panel of data on the 366 English local authorities, each of which set its own poll tax level. The transparent observability of individual and aggregate liabilities makes reliable measurement of rates of nonpayment possible. Moreover, these rates rose to unprecedented levels as well as exhibiting considerable variation across authorities. This, together with the variation in local taxes both between districts and over time, creates an ideal opportunity for empirical investigation. Our empirical specification allows us to investigate the

2 For further examples of tax revolts in history, see Wolf (1993).

determinants of compliance as a function of authority characteristics from census and other geographical data. Moreover, the analysis takes seriously the possibility of neighbourhood influences across authority boundaries.

The existing empirical literature on tax compliance has studied how reporting of income varies with tax rates controlling for other influences[3]. Unlike most of these studies, we actually observe the fraction of evaders (as do the tax authorities). For comparison with these existing studies it will be interesting to observe how sensitive this is to the tax rate, as well as to other factors such as income, ethnic composition and political complexion of the local authority.

We start from a standard formulation where individuals weigh up the benefits of nonpayment against the possible costs of prosecution and associated stigma. Stigma and externalities in the prosecution technology both imply interdependence in taxpayers' actions. We use this to derive their behaviour in equilibrium. This interdependence also means that we have to consider the formation of expectations about the non-compliance of *others*, both in own and in neighbouring authorities. To accomplish this, our paper draws on the recent literature which empirically analyzes neighbourhood effects in economic models[4].

The conceptual framework needed to study the poll tax is somewhat at variance with the canonical model of evasion in much of the existing literature, as developed by Allingham and Sandmo (1972) and Srinivasan (1973) among others and reviewed in Cowell (1990). This typically focuses on the declaration of income when detection is probabilistic. Poll tax nonpayment had more to do with defiance than deceit. Taxpayers could not hide their liability except by hiding their existence or migrating to another authority. Not registering to vote in order to avoid payment of the tax became a stock anecdote during this period. In so far as sanctions were probabilistic it was because the efficacy of and willingness to invoke sanctions were uncertain.

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3 In this spirit, Poterba (1987) studies the response of capital gains taxes to changes in tax rates using US data from 1965-82, finding an elasticity of evasion of about 0.37. This is similar to findings of other studies of the US such as Clotfelter (1983).

4 Case and Katz (1992) investigates such effects among youth in Boston. Besley and Case (1992) investigate how they might affect tax setting behaviour among US states. For a general treatment see Case (1992).

Our data, however, relate to non-payment by those registered as poll tax payers. Non-compliance of the sort that we are modelling was a gamble on prosecution rather than on detection, which ought to have been close to automatic for most people.

The history of the poll tax also sheds much interesting light on discussions of tax reform. The textbook case for funding local government by a flat rate tax in certain contexts is well known. In making the case against, compliance problems due to perception of unfairness are rarely voiced. Though protest against the introduction of the poll tax was not unexpected given the distributional impact of the reform, that this should have led to such high levels of noncompliance was something no commentator to our knowledge anticipated. Problems of implementation of this sort rarely figure seriously in theoretical discussion of tax reform in developed countries, presumably since they are felt to be an issue of secondary importance. This is a case in which clearly they were not.

The poll tax (or "community charge", as it was formally known) was introduced in Scotland in 1989 and a year later in England and Wales, replacing a 600 year old system of property taxation. The latter suffered from very little non-compliance. Payments were to be at a flat rate within each district, except for a limited number of income support recipients and special groups. The unpopularity of the poll tax was aggravated by increases in average local tax rates in the year of its introduction (Ridge and Smith 1990). A widespread campaign of nonpayment was initiated (see Appendix IV) with the support of certain opposition members of parliament (one of whom served a jail sentence for nonpayment). There were riots in central London in 1990 and by the end of the year opinion polls showed over 90% expressing discontent with the tax[5]. A challenge to the Prime Minister in October 1990 led to an internal leadership election in which all candidates promised abolition, at which point the demise of the tax was assured. The budget of April 1991 announced a shift in the burden of local finance from local to central taxation, allowing a general reduction in the second years' poll tax levels[6]. Noncompliance with the discredited tax continued however to rise and it was eventually replaced by a return to property taxation in April 1993.

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5 Market and Opinion Research International, 26 November 1990.

6 More precisely Value Added Tax was increased from 15% to 17.5% allowing an average cut of over £110 in poll tax payments.

A preliminary look at poll tax noncompliance is given in Figure 1.1. It illustrates clearly the geographical pattern of noncompliance and of its growth[7]. This makes plain both the country-wide increase in poll tax non-compliance throughout the period and the persistent geographic pattern with London and regions to the North consistently showing greatest non-compliance. Authorities controlled by the Labour Party are concentrated in these areas, which are also poorer and more urbanized. Below, we will examine such influences on compliance in detail.

The remainder of the paper is organized as follows. Section 2 develops a theoretical model of poll tax compliance. Empirical specification and issues in estimation are addressed in section 3 and the model then applied to data. Section 4 concludes.

7 The map divides England into the eight standard regions (clockwise from the top: North, Yorkshire and Humberside, East Midlands, East Anglia, South East, South West, West Midlands, North West), with the South East split to show Greater London and the Rest of the South East separately.

2 A model of poll tax compliance

This section develops a compliance model which, in a stylized way, captures the decision to comply with a poll tax demand. These were sent by mail to all residents of a particular jurisdiction. The model allows for two types of externality. The first operates through a stigma of non-compliance. We suppose that how individuals feel about their decision to comply is influenced by what others are doing and that the stigma associated with prosecution therefore depends on the number of other non-compliers in the individual's own and possibly also surrounding areas. The second externality operates through the detection technology: an individual is assumed to be less likely to be prosecuted, the larger the fraction of the local population that chooses not to comply. The idea is that the level of resources devoted to detection is supposed to be fixed in the short run and hence is diluted by having more non-compliers. These externalities motivate the importance of expectations in the decision of one individual to comply with their poll tax demand, as well as the possibility of neighbourhood influences. Externalities may also result in multiple equilibria as has been discussed in related contexts by Cowell (1990, ch.6), Sah (1991), Wolf (1993) and Tirole (1993). We give an example illustrating this in our model below.

We begin with a static model in which each individual in a particular locality receives a poll tax demand, which he/she must decide whether or not to comply with. Of those who decide not to pay, a certain fraction is punished. We assume that punishment can be modelled as a fine f_j which depends upon the authority j in which the individual lives[8]. In addition, a stigma may be attached to not paying, which we denote (in monetary equivalent terms) by

$$\xi_{ij} \equiv \xi(x_j, \tau_j, y_{ij}, \theta_{ij}, z_j) \tag{1}$$

where x_j is the fraction of other non-compliers in authority j, τ_j is the poll tax level in authority j, y_{ij} is the income or wealth of individual i in region j, θ_{ij} is a vector of personal characteristics of person i in authority j and z_j is a vector of relevant authority characteristics. We assume that this is decreasing in τ_j and we would expect to find it decreasing in x_j and increasing in y_{ij}.

The probability of being caught conditional on not paying is

8 In practice, the fine for poll tax compliance has been simply payment of court costs and has therefore varied little if at all between districts.

$$p_{ij} = p(x_j, e_j, \theta_{ij}, z_j) \tag{2}$$

where e_j is the amount of effort put into prosecution in area j. Note that we also put the number of other non-compliers into this function. This is intended to represent the fact that if there are many other non-compliers then it is less likely that any individual will be prosecuted for not paying, for any given level of effort. For simplicity, we suppose that individuals are risk neutral. It would be easy to generalize the model to relax this[9]. Thus an individual will choose not to comply provided that the expected return from doing so is positive, i.e. if

$$p(x_j^*, e_j, \theta_{ij}, z_j)\{y_{ij} - \tau_j - f_j - \xi(x_j^*, \tau_j, y_{ij}, \theta_{ij}, z_j)\} + \{1 - p(x_j^*, e_j, \theta_{ij}, z_j)\} y_{ij} > y_{ij} - \tau_j \tag{3}$$

or

$$p(x_j^*, e_j, \theta_{ij}, z_j)\left\{\frac{f_j + \xi(x_j^*, \tau_j, y_{ij}, \theta_{ij}, z_j)}{\tau_j} + 1\right\} < 1 \tag{3'}$$

where x_j^* is the *anticipated* non-compliance rate in local authority j. Note that all individuals in this world will cheat if they can get away with it. It would be easy to augment the model to include a class of relentlessly compliant taxpayers who pay their taxes regardless of whether they could get away with cheating in expectation. This is arguably more descriptive of the real world than the model developed here[10].

The fraction of non-compliers in district j is the fraction of individuals for whom (3) holds. It is clear from (3) that this is affected by the number of other expected non-compliers. We can write it as

$$x_j = h_j(x_j^*, \tau_j, e_j, f_j, z_j) \tag{4}$$

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9 If individuals were risk-averse then changes in the poll tax could have income effects of the sort which have been important in the theoretical literature on tax compliance. An increase in poll tax could in principle discourage default if the expected loss of income lead to greater risk aversion (see Cowell 1990, ch.4).

10 The delay in prosecution was typically long enough that the financial return to nonpayment in terms of accrued interest would probably exceed the ensuing fine even if prosecution were certain. Stigma must therefore have played a major role in discouraging default.

In equilibrium, we will suppose that the actual and expected rate of non-compliance agree with each other. An *equilibrium* is therefore described by a fixed point of (4), i.e. a non-compliance rate π_j such that

$$\pi_j = h_j(\pi_j, \tau_j, e_j, f_j, z_j). \tag{5}$$

Depending on the exact nature of the joint distribution of θ and y and the functions described above, there are possibly many equilibria.

It is equation (4) that motivates the empirical specification that we use below. Notice that we have confined ourselves to a situation in which ξ_{ij} and p_{ij} depend only on the expected amount of non-compliance in the individual's own jurisdiction. There are however reasons to think that there could also be some dependence on the expected amount of non-compliance in neighbouring jurisdictions, x_{-j}*. We have in mind a situation in which individuals are affected by the behaviour of others either because of casual social contact or else by being influenced in common media markets.

A further extension of the model might also allow for the neighbour's *poll tax* and not just its compliance rate to affect non-compliance. This captures the idea that if the poll tax is higher in surrounding areas then individuals may feel less upset about their own tax level, rasing their willingness to pay. This suggests putting τ_{-j} on the right hand side of (4). Hence instead of (4) we would have

$$x_j = h_j(x_j^*, x_{-j}^*, \tau_j, \tau_{-j}, e_j, f_j, z_j). \tag{6}$$

Another extension would be to introduce dynamics explicitly. There may, for instance, be slow adjustment to neighbours or own past compliance rates as information is revealed. The data, which come from three time periods, will permit us to look at this to some extent. If all decisions and variables are indexed by time, it is possible to have a model in which *past* values of x_j and x_{-j} affect current compliance. This could reflect some gradual adjustment of expectations. It could also reflect the ideas developed in Sah (1991). He allows past crime levels to influence future crime via a kind of learning process. This could involve learning about the probability of detection or else the number of other non-compliers. If individuals get to observe mostly the behaviour of individuals who are close to them in location, then this again could motivate the

importance of neighbourhood effects in patterns of compliance with the poll tax. This story is quite appealing in the context of the poll tax, where we believe that it took individuals a while to learn about the new system after it was reformed by the Thatcher government.

To fix ideas further, we consider a simple example to illustrate the types of equilibria that are possible in the model. Suppose that everyone has the same income level, there is no stigma, $p(x,e,\theta,z) = 1-\exp\{\theta(x-e-1)\}$ with $e>0$ and θ is uniformly distributed on [0,1]. An individual complies if

$$\theta(1+e-x^*) < \ln(1+\tau/f) \equiv \psi.$$

Thus (4) takes the form

$$x = \min\left\{\frac{\psi}{(1+e-x^*)}, 1\right\}$$

There are three types of equilibria, illustrated in figure 2.1.

1. ($e > \psi$): In panel 1, there is a unique equilibrium level of non-compliance at A.

2. ($1 > \psi > e > 2\sqrt{\psi}-1$): In panel 2 there are three equilibria; a high compliance equilibrium at A, an unstable medium compliance equilibrium at B and a completely anarchistic equilibrium at C, where nobody pays[11].

3. ($0 > 2\sqrt{\psi}-1 > e$ or $\psi < 1$, $1+\psi > e$): In panel 3, there is only an anarchistic equilibrium.

As we allow the level of e and the penalty ratio f/τ to vary, then we get one or other of these situations. The lower is e, the more likely is the anarchistic outcome. This is also true as we allow f to get smaller or τ larger. The relationship between poll tax and equilibrium noncompliance is illustrated in Figure 2.2 (for a case in which $e<1$ and multiple equilibria can exist).

In summary, the theory suggests a number of predictions. The first, and possibly most obvious, is that we would expect to observe the highest non-compliance rate in areas where the poll tax

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11 In a more realistic model this could be mitigated by the introduction of a class of relentlessly compliant payers.

is highest and enforcement weakest. Second, we would not be surprised to find that neighbours' poll taxes and compliance levels, might affect the decision to comply. Our next task is to develop an empirical specification which permits us to test for these things.

3 Empirical application

We test our model using aggregate data on the 366 local authorities in England, which include the London Boroughs, metropolitan districts and shire districts in England. Since we lack data on enforcement for 1992-3, our analysis for this year is incomplete. Hence, we focus primarily on data from 1990-2, the first two years in which the poll tax was in force. The data are derived from a number of official sources, full details of which are available in Appendix I.

The theory suggested that important variables would be local authorities' non-compliance rates, levels of the poll tax, levels of collection effort, fines and authority characteristics including incomes. To define compliance rates empirically, we use the ratio of revenues received within the year of demand to total collectable revenue. Subtracting this from one gives a measure for which higher values represent a greater extent of noncompliance. There are two possible sources of deviation of this from the theoretical ideal. Firstly, some individuals not having paid at the year end may still intend paying, and secondly, some individuals included as non-delinquent may have paid only because they were successfully prosecuted within the year. Notwithstanding, our measure is sufficiently close to the theory to make studying it worthwhile. Data on poll tax levels are readily available.

Enforcement of payment for delinquents was a lengthy procedure[12] and gives a variety of data on collection effort. We make use of the numbers of reminders, summonses, liability orders and calls for bailiffs within the year, forming our enforcement variables by dividing each of these by number of noncompliers in the authority. Including enforcement in the analysis reduces the sample by more than half, since some authorities failed to respond to the data collecting agency's requests on this point. This may generate a sample selection bias which we discuss further below. Fines were equal to the cost of the summons and therefore varied little across authorities and are excluded from our empirical analysis.

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12 The sequence of enforcement was as follows. Initially reminders were sent. If these were ignored noncompliers were summonsed to appear in court, and if individuals did not then make payment in full (plus the cost of the summons) they then received a liability order forcing them to pay their arrears in a prescribed form. This could be a monthly arrangement, an attachment of earnings or benefits, or so on. If any of these methods failed then bailiffs were called upon to visit and recover property equal to the value of the arrears.

Above, we suggested that neighbours might influence the extent of noncompliance within a neighbourhood. We define neighbouring authorities geographically. Thus the neighbours' value of a particular variable is the average (population weighted) value defined over all contiguous authorities.

Figures 3.1 through 3.3 look at the raw data. Figure 3.1 plots non-compliance in 1991 against that in 1990. Comparing to the 45° line, we see that non-compliance rose between these two years in the majority of authorities and from Figure 3.2 that this occurred in spite of a universal fall in second year poll tax levels. It is clear also from this second figure that there was a positive correlation between the change in non-compliance and change in poll tax. Figure 3.3 plots own change in poll tax non-compliance against that in neighbouring authorities giving a somewhat ambiguous picture. Figure 3.4 shows a negative correlation between change in one enforcement indicator and change in non-compliance.

For empirical purposes, we focus attention on the linear approximation to (6)

$$x_{jt} = a_0 + a_1 x^{*}_{jt} + a_2 x^{*}_{-jt} + a_3 \tau_{jt} + a_4 \tau_{-jt} + a_5 e_{jt} + a_6 Y_j + a_8 Z_j + \eta_j + \varepsilon_{jt} \qquad (7)$$

where Y_j is the mean income in the authority and we have brought together into a vector Z_j relevant observed features of the authority from z_{jt} and observed parameters of the distribution of θ_{ijt}. The theory outlined above suggests that $a_1, a_2, a_3 > 0$ and $a_4, a_5, a_6 < 0$.

Imposing linearity on (6) is somewhat restrictive, ruling out the possibility of multiple equilibria noted above. However, it is not realistic to think that we might be able to identify a non-linear relationship non-parametrically.

Note that since we have only one observation for the period of the poll tax on authority characteristics[13] and on incomes, we therefore treat both as fixed. This could be problematic

13 We also have heavily lagged data on similar characteristics from a ten years' earlier census, which we are able to use as instruments.

since the period in question was one in which the UK economy was moving into a recession which hit certain regions harder than others, and we therefore include regional dummies to allow for region-specific trends in non-compliance where possible[14].

The effects of unobserved authority characteristics enter through the final terms η_j and ε_{jt}. The first of these, η_j, is treated as an authority specific fixed effect and the second, ε_{jt}, as a time-varying authority specific shock. Despite these terms, the parameters of (7) could still in general depend on higher moments of the authority specific distribution of log income and other characteristics. If these higher moments are not constant across authorities then there is a need to worry about parameter heterogeneity. For instance, noncompliance in districts with more tightly bunched income distributions should be more responsive to changes in poll tax levels, as small changes are likely to change the sign of (3) for a greater proportion of households.

Estimation of the model depends crucially upon how we choose to specify the nature of the expectations underlying x_{jt}^*. Since the data is available for only three years this seriously limits the possibility of introducing sophisticated dynamics. We therefore assume that x_{jt}^* differs from the outcome x_{jt} by an error, denoted u_{jt}, unpredictable on the basis of any information assumed available to an individual. This could be justified either by an assumption of stochastically correct or "rational" expectations or perhaps by the belief that individuals learn quickly what is going on.

Either would imply that we would effectively be at equilibrium in each period and would need therefore to estimate the equilibrium relationship (5). This could be problematic in the general case because the form of the equilibrium relationship between x_{jt} and the other variables could be highly nonlinear and in the case of multiple equilibria indeterminate. For the linear specification (7), however, equilibrium is unique and the equilibrium relationship linear.

14 Note also that since there is no observed variation over time or authorities in the level of fines, we have therefore absorbed them into the intercept term.

$$x_{jt} = \frac{1}{1-\alpha_1}[a_{0t} + a_2 x_{-jt} + a_3 \tau_{jt} + a_4 \tau_{-jt} + a_5 e_{jt} + a_6 \ln Y_j + a_8 Z_j + \eta_j + \varepsilon_{jt} + \alpha_1 u_{jt} + \alpha_2 u_{-jt}]$$

$$\equiv b_{0t} + b_2 x_{-jt} + b_3 \tau_{jt} + b_4 \tau_{-jt} + b_5 e_{jt} + b_6 Y_j + b_8 Z_j + \eta_j + v_{jt} \qquad (8)$$

where $v_{jt} \equiv \varepsilon_{jt} + \alpha_1 u_{jt} + \alpha_2 u_{-jt}$.

Estimation of (8) by OLS would be biased by any correlation between the fixed effects η_j and the poll tax and enforcement variables. We adopt instead a two stage procedure as follows. Taking first differences in (8) eliminates the fixed effects together with all time-invariant variables, yielding an equation

$$\Delta x_{jt} = \Delta b_{0t} + b_2 \Delta x_{-jt} + b_3 \Delta \tau_{jt} + b_4 \Delta \tau_{-jt} + b_5 \Delta e_{jt} + \Delta v_{jt}. \qquad (9)$$

Estimates of (9) for the first two years are presented in tables 3.1-3.3. We begin by looking at OLS results for four cases, with and without neighbour effects and with and without enforcement variables. All regressions incorporate dummies for the nine English regions, included with the aim of proxying omitted regional differences in income or wealth changes which were undoubtedly a feature of the emerging recession.

The first and third columns in Table 3.1, which ignore enforcement but use the full sample, show a positive and significant effect of an increase in the poll tax on non-compliance. However, there is no significant effect from adding neighbours' non-compliance rates to the regression and a significant though oddly signed effect from neighbours' poll tax. That neighbours' noncompliance should enter insignificantly in the OLS regressions is particularly surprising given that one might at least have expected some spatial correlation in the error terms v_{jt}. Any significance of neighbours' variables disappears when enforcement variables are added. As we see in table 5.2, columns 1 and 4, the number of reminders issued and liability orders granted both have a negative and significant effect on poll tax non-compliance[15].

Even if we assume no contemporaneous correlation between v_{jt} and either poll tax or enforcement variables, feedback from default rates to the following year's value of these variables means that the error term Δv_{jt} in (9) is almost certainly correlated with all other variables on the right

15 None of these results are dependent on the inclusion of regional dummies.

hand side. OLS estimation is therefore almost certainly inconsistent and we therefore report IV results with data from the 1981 census used as instruments[16]. There is however, a concern about instrument validity. Some of these variables could be correlated with shocks in the non-compliance equation, e.g. If ethnic composition affected the way in which taxpayers responded to the poll tax change. We thus report Sargan's (1958) test of instrument validity for all our IV estimates[17].

Looking again at table 3.1, we find that the own poll tax effect remains strongly positively significant (with a similar magnitude). Any significant effect from neighbours' variables disappears. However, the Sargan test fails in both specifications. Adding enforcement variables, we get a pattern that is broadly consistent with the OLS estimates and, moreover, the Sargan test now passes comfortably. In results reported in Appendix II, we verified that this is not due to sample selection bias: the Sargan test failed when we ran similar regressions on the restricted sample of table 3.2 but without the enforcement variables included. Hence, it appears that the inclusion of the enforcement variables is essential for our instruments to pass a test of overidentifying restrictions[18].

In summary, we find that the own poll tax effect has a positive and significant effect on poll tax compliance; its sign and significance are stable across all of the specifications that we ran. One can say with some confidence that the effect was of the order of ten percentage points additional noncompliance for every £100 added to the level of the tax. Neighbour effects do not appear

16 The instruments used relate to own and neighbouring districts' levels of education, housing conditions and ownership, ethnic composition, car ownership, household age and composition, population and geographic area.

17 The test is based on correlation between the residuals of the instrumented equation and the instruments. It can be calculated as the product of sample size and the R^2 from a regression of the residuals on the instruments. Under the null of valid overidentifying restrictions the test is distributed as χ^2 with degrees of freedom equal to the number of instruments less the number of estimated parameters.

18 The R^2 from the first stage regressions, reported in Appendix III, show our instruments to be fairly weak predictors, however, of the important enforcement variables relating to reminders and liability orders. The estimated coefficients on these variables need to be treated with appropriate caution.

robust or significant. Among the enforcement variables the number of reminders issued and liability orders granted appear to have some influence, but the latter's significance depends upon whether instruments are used.

Remaining parameters of (8) can be picked up by noting the relationship

$$\bar{x}_{j.} - b_2\bar{x}_{-j.} - b_3\bar{\tau}_{j.} - b_4\bar{\tau}_{-j.} - b_5\bar{e}_{j.} = \bar{b}_0 + b_6 \ln Y_j + b_8 Z_j + \eta_j + \bar{v}_{j.}, \tag{10}$$

where $\bar{X}_{j.}$ is used to denote $\frac{1}{T}\sum_t X_{jt}$ for any variable X_{jt}. An estimate of the left hand side can be constructed by substituting estimated coefficients from the differenced regression (9), and (10) estimated by OLS (or some IV procedure) (see Hsiao, 1986, p.50). In other words, we proceed by regressing the estimated fixed effects from the first stage on fixed authority characteristics.

Our estimates of equation (10) are reported in Table 3.3. The estimated fixed effects are drawn from column 1 in table 3.2 and the regressors include contemporaneous characteristics of the authorities from the 1991 census and elsewhere. These characteristics include measures of political control and turnover on the register of poll taxpayers. The latter might affect the ability of authorities to effectively sanction individuals and hence increase non-compliance. Since turnover and migration may be influenced by the same unobservable factors that influence non-compliance, we also report some results where we instrument them with the 1981 characteristics used as instruments above. We also try instrumenting political control, which could be endogenous if increases in non-compliance lead to changes in the political complexion of local government.

Again we begin by reporting OLS estimates in the first column. The results accord broadly with expectation. The belief that avoidance of prosecution is easiest for those who are most mobile, particularly among the young, is confirmed in the strongly positive effects of turnover and migration among the young. The negative impact of owner occupation may reflect something similar. Other effects accord with the view that more prosperous households will be less prone to default because stigma will be greater[19]. The signs and significance of coefficients on average

19 Note that these effects conflict with the standard analysis advanced in the theoretical literature in terms of risk attitudes. This has it that more wealthy individuals will be less risk-averse and therefore *more* willing to gamble on not being prosecuted (see Cowell, ch.4).

wage, crowded accommodation and owner occupation all consistently support such a view. Little reliable evidence emerges of effects from age or ethnic composition, nor of any difference between areas under different local political control.

Subsequent columns differ from the first in that potentially endogenous variables - those relating to turnover and migration and to political control - are instrumented[20]. The results in this case are again broadly similar and the Sargan test passes at the 95% confidence level.

The analysis of the third year of data is limited by the lack of enforcement data which will not be available until April 1994. Hence, the best that we can do is to replicate table 5.1 using these data. In this table the hypothesis that neighbours' default rates may be important is supported. Without including these, the own poll tax effect is actually negative. Moreover, the Sargan test of instrument validity is rejected. However, including the neighbours' poll tax and default rate now yields a positive and significant effect of neighbours' default rate on own default as the theory would predict. In addition, this result persists after instrumenting with 1981 census data and the Sargan test is comfortably passed.

In light of our finding above that enforcement data makes a difference to our conclusions, we must treat these findings with caution. However, one might interpret our finding as saying that in the second two year period in which the poll tax was in force, the effect of neighbours' non-compliance became a significant determinant of non-compliance and the poll tax change a less important determinant. This is not inconsistent with a model in which individuals are affected by what is happening in neighbouring authorities during the later period of the poll tax.

20 The same ten-year lagged instruments were used as previously.

4 Conclusion

The UK's experience with the poll tax lasted only three years. However, the lessons for thinking about issues in tax compliance are more long lasting. It reminds us that even in an economy with a relatively well developed detection and legal system to enforce compliance, one cannot take tax payment for granted. The poll tax was widely perceived as unfair and the political opposition to the tax fuelled the non-compliance problem. This suggests that tax morale[21] may be more fragile than has heretofore been believed in stable democracies. The poll tax experience reminds us that tax morale matters for a workable system of taxation. In light of this it is interesting to ponder the fate of the new Council tax introduced in 1993 to replace the poll tax. Already there is evidence that the compliance problems being faced are more severe than those that were faced under the preceding property tax system, even though the council tax broadly follows it in its design[22]. This may be because tax morale is undermined by frequent changes in tax arrangements or it could be a permanent effect of the poll tax debâcle. The three year experience with the poll tax may have undermined tax morale (at least for local taxes) in a way that may have long term consequences if there is hysteresis in poll tax compliance.

Our empirical results confirm the idea that higher taxes led to larger compliance problems. They also confirm the idea that attempts to enforce compliance had a positive effect. Our search for neighbourhood effects on non-compliance had more mixed results. They appeared to be significant only for the third year of data where no enforcement information was available. However, they are consistent with the view that neighbourhood influences on tax compliance were a dominant influence in the final year.

Social psychologists and political scientists have long believed that tax compliance cannot be studied in a social vacuum. Whether the poll tax experience confirms or denies this is moot. However, it is an episode that enriches our understanding of tax compliance issues and gives way, naturally, to contemplating the importance of externalities. The results in this paper suggest that the standard economic influences of tax size and detection odds are at work, but other determinants cannot be ruled out.

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21 For a related analysis which emphasises the importance of political considerations and tax morale in compliance considerations see Pommerherne, Hart and Frey (1993).

22 "Warning against complacency as tax collection improves slightly", Local Government Chronicle, August 1993.

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Table 3.1 Effect of poll tax Dependent variable: Δ Noncompliance rate	OLS	OLS	IV	IV
Δ Poll tax (£1000s)	.937 (9.859)	.871 (8.597)	1.209 (6.320)	.947 (3.985)
Δ Neighbours' poll tax (£1000s)		.504 (2.550)		.574 (1.286)
Δ Neighbours' noncompliance rate		-.116 (1.451)		.032 (0.182)
Constant	.145 (9.649)	.205 (7.289)	.179 (6.978)	.220 (3.858)
Sargan test (p value)			.0003	.0008
Normality test (p value)	.0805	.0925	.0440	.0127
R^2	0.325	0.338		
Standard error	0.049	0.048	0.049	0.049
Number of obs	345	344	345	344

(Absolute t ratios in brackets)

Table 3.2 Effect of Enforcement Dependent variable: Δ Noncompliance rate	OLS	OLS	IV	IV
Δ Poll tax (£1000s)	1.030 (8.809)	.997 (7.702)	1.149 (4.799)	.991 (3.471)
Δ Neighbours' poll tax (£1000s)		.227 (0.905)		.190 (0.392)
Δ Neighbours' noncompliance rate		-.038 (0.366)		.112 (0.543)
Δ Reminders issued per noncomplier	-.014 (5.036)	-.013 (4.804)	-.027 (3.925)	-.025 (3.385)
Δ Summonses issued per noncomplier	-.017 (0.852)	-.017 (0.845)	.016 (0.372)	.005 (0.101)
Δ Liability orders granted per noncomplier	-.039 (2.728)	-.039 (2.677)	-.054 (1.776)	-.055 (1.730)
Δ Calls to bailiffs per noncomplier	.001 (0.033)	-.000 (0.011)	.082 (1.483)	.086 (1.444)
Constant	.144 (7.731)	.170 (4.772)	.152 (4.723)	.161 (2.396)
Sargan test (p value)			.5951	.5781
Normality test (p value)	.2554	.1495	.6251	.2749
R^2	0.604	0.606		
Standard error	.040	.040	.046	.046
Number of obs	151	151	151	151

(Absolute t ratios in brackets)

Table 3.3 Fixed effects regression Dependent variable: Fixed effects from (10)	OLS	OLS	IV	IV
Turnover	.087 (1.846)	.092 (2.007)	.115 (1.231)	.105 (1.140)
Migration among 15-24 year olds	.463 (2.510)	.477 (2.696)	1.251 (3.300)	1.242 (3.770)
Ln average wage	-.289 (1.963)	-.296 (2.808)	-.275 (1.718)	-.204 (1.732)
Ln average income	.018 (0.185)		.091 (0.811)	
Unemployed	.013 (1.322)	.013 (1.443)	.017 (1.553)	.016 (1.643)
Owner occupiers	-.024 (1.780)	-.024 (1.850)	-.039 (2.421)	-.042 (2.723)
Crowded accommodation	6.969 (2.355)	5.175 (2.349)	3.191 (0.891)	2.075 (0.792)
Ethnic minority population	-.217 (1.082)		-.205 (0.940)	
Lone parents	-.016 (3.101)	-.015 (3.257)	-.014 (2.417)	-.013 (2.746)
Students	-.001 (0.228)		-.002 (0.616)	
15-24 year olds	.805 (0.752)		1.271 (1.067)	
65 and over	.000 (0.161)		.002 (0.596)	
Metropolitan district	.063 (2.481)	.068 (3.216)	.091 (2.913)	.090 (3.737)
Labour controlled	.028 (1.682)		.035 (1.287)	
Conservative controlled	.015 (0.818)		-.008 (0.203)	
Constant	1.394 (1.873)	1.589 (2.575)	.988 (1.205)	1.010 (1.460)
Sargan test (p value)			.0012	.1037
Normality test (p value)	.1266	.0983	.1668	.0009
R^2	.494	.475		
Standard error	.062	.062	.067	.066
Number of obs	144	144	144	144

(Absolute t ratios in brackets)

Table 3.4 The third year Dependent variable: Δ Noncompliance rate	OLS	OLS	IV	IV
Δ Poll tax (£1000s)	-.071 (0.592)	-.024 (0.197)	-.216 (0.779)	.173 (0.525)
Δ Neighbours' poll tax (£1000s)		-.097 (1.097)		.101 (0.390)
Δ Neighbours' noncompliance rate		.136 (1.803)		.700 (3.057)
Constant	.045 (5.411)	.013 (0.531)	.048 (4.970)	.024 (0.407)
Sargan test (p value)			.2463	.9799
Normality test (p value)	.0000	.0000	.0000	.0000
R^2	.062	.073		
Standard error	.042	.044	.042	.046
Number of obs	351	351	351	352

(Absolute t ratios in brackets)

Figure 1.1
Noncompliance by region

Figure 2.1
Non-compliance equilibria

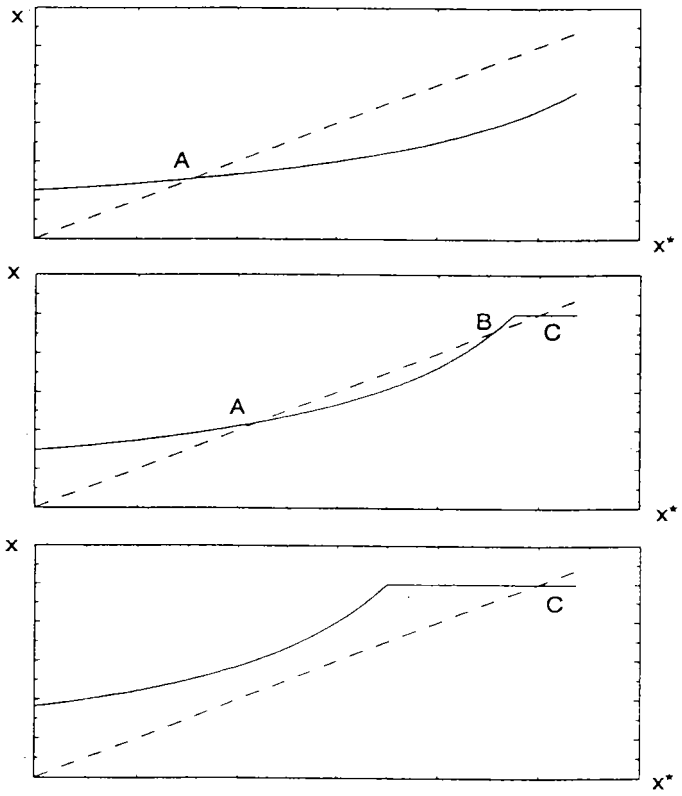


Figure 2.2
Effect of poll tax on
equilibrium non-compliance

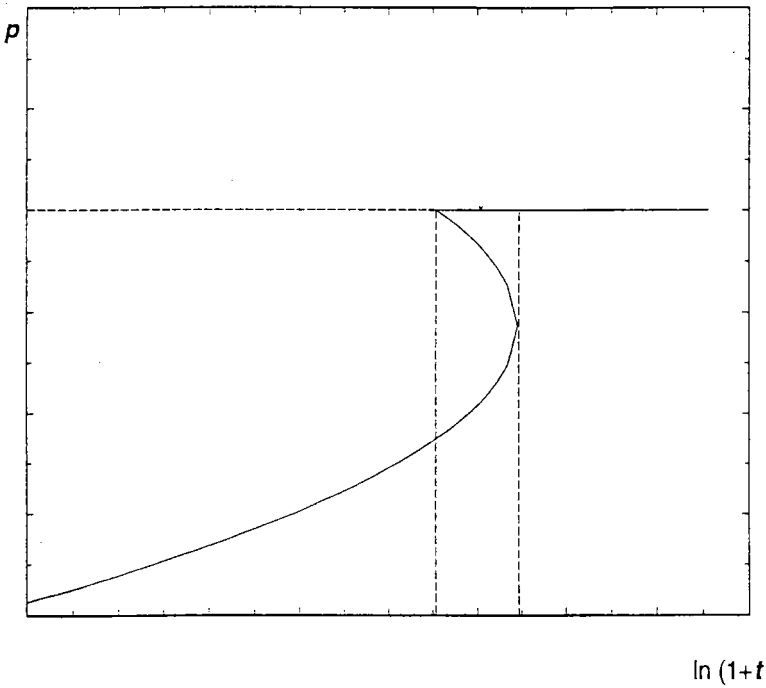


Figure 3.1
Noncompliance rates over time

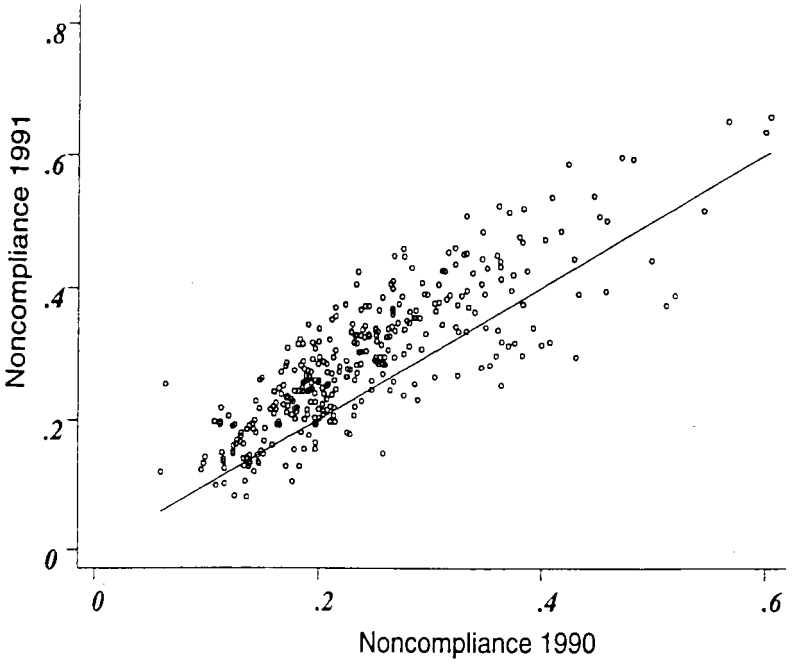


Figure 3.2
Poll tax and changes in non-compliance

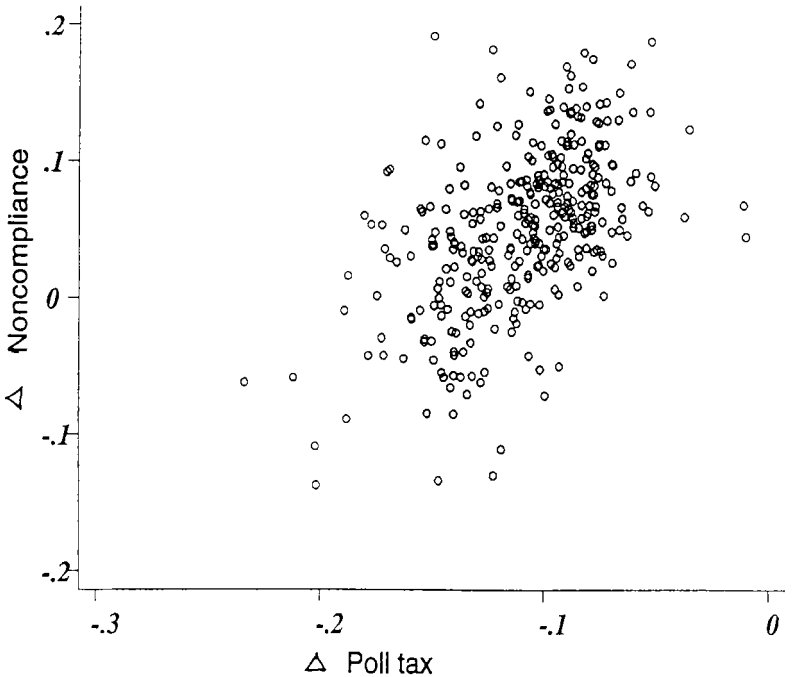


Figure 3.3
Neighbourhood effects
on non-compliance

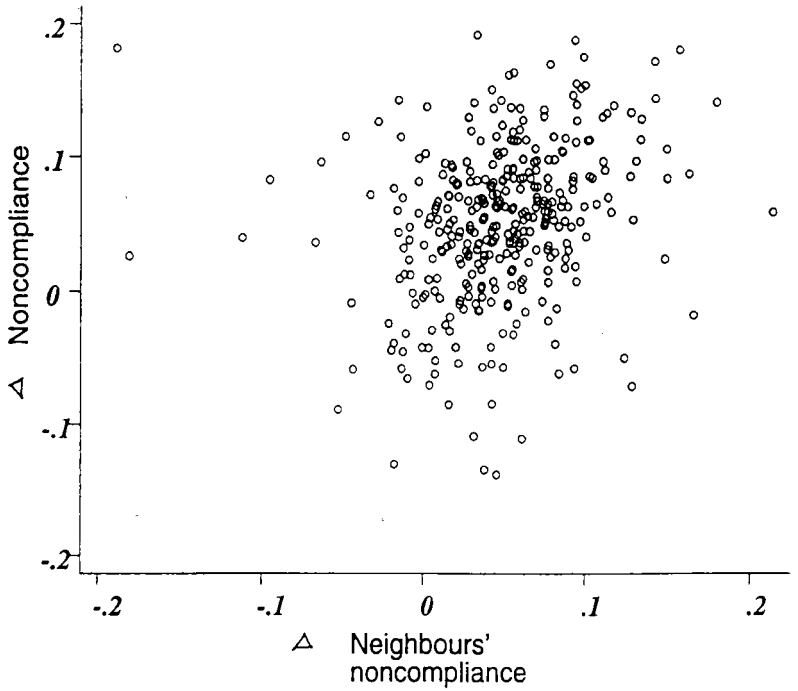
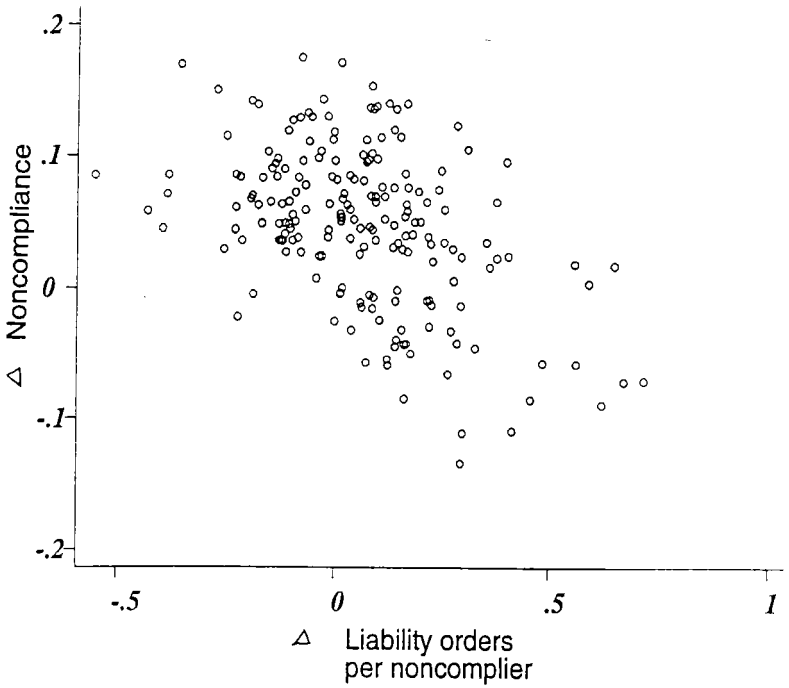


Figure 3.4
Enforcement effects
on non-compliance



Appendix I: Data

Sources: Department of the Environment (DoE)
Chartered Institute of Professional Finance Accountants (CIPFA)
Office of Population Census Surveys (OPCS)
New Earnings Survey (NES)
Inland Revenue Statistics (IRS)
Local Authority Associations' Year Books (LAA)
Ordnance Survey (OS)

Compliance rates: (DoE, CIPFA) Our measures of compliance rates take the ratios of revenues received within the year of demand (DoE) to collection fund income (CIPFA). Collection fund income is a measure of collectable revenue based on the number of individuals registered by the local authority. Our measure therefore fails to pick up non-compliance through non-registration. Other weaknesses are discussed in the main text.

Poll tax levels: (DoE) These are actual poll tax levels set by local authorities.

Collection effort: (CIPFA) We have good data on collection effort for the first two years for some authorities. The gaps may be endogenous (in that those authorities not responding to CIPFA may be atypical in terms of noncompliance rates). The sequence of enforcement is issue of reminder, followed by summons to attend at court. Conviction leads to a liability order. A call to bailiffs is a final option for recalcitrant nonpayers.

Incomes and Wages: (NES, IRS) Income statistics are only available (from 1993 IRS) at Shire County, Metropolitan Areas and Greater London level, there are 47 observations. Wage data are available at a similar level from the 1992 NES.

Other Characteristics: (OPCS, CIPFA, LAA) Data are from the 1981 and 1991 Censuses, with the exception of data for turnover on the poll tax register which comes from CIPFA and on political control which comes from LAA. Political control variables are for the year 1992.

Neighbourhood effects: (OS) Authorities are quoted as neighbouring if sharing a boundary. All neighbouring averages are population-weighted.

Exclusions: We exclude as evidently idiosyncratic the City of London and Isles of Scilly (on grounds of small size of resident population) and Westminster (which set a zero poll tax in 1991 and 1992, financing all spending through central grants). Authorities are also dropped where data is missing or obviously outlying and there is doubt as to the veracity of the data.

Appendix II: Sample selection bias

Table A.1 Effect of poll tax: selected sample Dependent variable: Δ Noncompliance rate	OLS	OLS	IV	IV
Δ Poll tax (£1000s)	1.158 (8.848)	1.097 (7.668)	1.311 (5.635)	1.043 (3.754)
Δ Neighbours' poll tax (£1000s)		.496 (1.756)		1.029 (2.367)
Δ Neighbours' noncompliance rate		-.112 (0.972)		-.099 (0.515)
Constant	.158 (7.764)	.217 (5.466)	.174 (6.135)	.278 (4.579)
Sargan test (p value)			.0001	.0001
Normality test (p value)	.0301	.0147	.0146	.0016
R^2	0.458	0.470		
Standard error	0.046	0.046	0.046	0.049
Number of obs	151	151	151	344

(Absolute t ratios in brackets)

Appendix III: First stage regressions

The R^2 and F statistics below relate to the first stage regressions for the final column of table 3.2. There are 38 instruments and 151 observations.

Instrumented variable	R^2	$F(38,112)$	p value of F
Δ Poll tax	0.505	3.00	0.000
Δ Neighbours' poll tax	0.735	8.16	0.000
Δ Neighbours' noncompliance rate	0.613	4.67	0.000
Δ Reminders issued per noncomplier	0.202	0.74	0.851
Δ Summonses issued per noncomplier	0.341	1.53	0.046
Δ Liability orders granted per noncomplier	0.286	1.18	0.247
Δ Calls to bailiffs per noncomplier	0.340	1.52	0.048

The R^2 and F statistics below relate to the first stage regressions for the third column of table 3.3. There are 38 instruments and 144 observations.

Instrumented variable	R^2	$F(38,105)$	p value of F
Turnover	0.389	1.76	0.013
Migration among 15-24 year olds	0.511	2.88	0.000
Labour controlled	0.727	7.37	0.000
Conservative controlled	0.729	7.43	0.000

Full details of the first stage regressions are available on request from the authors.

Appendix IV: The non-payment campaign

The document reproduced below was circulated in North London and gives an example of the nature of the non-payment campaign.

ISLINGTON AGAINST THE POLL TAX

THOUSANDS AREN'T PAYING THE POLL TAX — JOIN US!

How much will the Poll Tax be?

In Islington the poll tax is £498.62. And virtually everyone over 18 will have to pay it.

Why are Poll Tax bills so high?

Over the last ten years and more the government has been drastically cutting back on the money they have given local councils. On top of this the government calculations of Poll Tax levels are based on inflation of 4%, completely underestimating the amount of money Islington needs to provide services and ignoring the fact that many people will not pay the tax.

Who will have to Pay?

Everyone will be expected to pay the Poll Tax, no matter how rich or poor you are, or whether you live in a mansion or a bedsit. There are rebates for people on benefits and very low incomes. But even if you have no income at all, you will have to pay 20% — about £100 per year.

So what can be done?

Islington Against the Poll Tax is organising against this. We are calling on people not to pay the tax. We are asking trade unionists not to co-operate — and Islington NALGO members are in dispute over staffing levels — and also calling on the council not to collect the tax. If enough people refuse to pay, the tax will be unworkable.

What can I do?

Islington Against the Poll Tax is a borough-wide umbrella group which has affiliations from local trade unions, tenants' groups and Labour Parties. There are local groups in each ward in the borough which organise leafleting, street stalls, local meetings etc. We are developing a system of street representatives to distribute publicity and support people who aren't paying.

We need everyone to get involved. Fill in the form overleaf.

Make it Easy on Yourself — Don't Pay the Poll Tax!

Besley, Preston and Ridge 21 September 1993

The Poll Tax and Non-Payment

Some Questions Answered

In Scotland, over a million people are refusing to pay. In England and Wales, we can do even better, if we make sure non-payers are kept in touch through a properly organised campaign, and are given advice and support. Islington Against the Poll Tax will be offering full support and legal advice to all those refusing to pay. Here are the answers to a few questions people are asking about non-payment:

What happens if I don't pay?

Firstly, Islington Council will send you two reminders. Once the second reminder has been issued, you have seven days to pay the whole year's poll tax. After that if you don't pay, the council will go to a magistrates court for a liability order. You will receive a court summons — everyone should use their right to appear in front of a magistrates court.

What happens if a liability order is granted?

The council can either send in the bailiffs to take away your belongings or apply for attachment of earnings or deduction from benefits. They can only do one of these at a time but they can try them all.

What happens if the bailiffs come?

In Scotland bailiffs have been sent in but have never succeeded in taking anyone's belongings. This is where successful local organisation is so vital; making sure we can defend anyone from the bailiffs.

Will I be evicted if I don't pay?

No. Both private and council tenancies are safe, and home owners could not be forced to sell up.

Are Non-Payers given heavy fines?

No. The fines are for non-registration. Non-payment is a civil offence — there are no fines. But the council can apply for costs if they get a liability order.

Will they take all my wages?

They can't. The limits they can take are strictly defined in law — for example someone earning £100 could only lose a maximum of £10 a week. But again, the procedure is costly and difficult.

Will I lose my pension or benefits?

No. Poll Tax cannot be deducted from pensions, invalidity benefits, or other specified benefits. The maximum that can be deducted from income support is £1.75 a week for a single person. So this would be a lot cheaper than the Poll Tax, even with a full rebate!

Can they take money from my bank account?

Again, no. This is something that has happened over registration in Scotland but the law in England is different.

Can I lose my job?

Non-payment of Poll Tax is not a legal ground for dismissal, even if you work for the council.

Will I still be able to get credit or HP?

Yes. Credit agencies are interested in your ability to pay off loans, not the Poll Tax.