Crime and Benefit Cuts

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Abstract

In this paper we look at the relationship between crime and benefit sanctions using a quasi-experimental setting induced by the introduction of the Jobseekers allowance (JSA) in Britain in October 1996. We look at crime rates in areas more and less affected by JSA before and after JSA introduction. In the areas more affected crime rose by more. These were also the areas with higher outflows from unemployment and particularly to people dropping off the register but not into work or onto other benefits. As such these results seem to reflect that benefit sanctions in JSA may well have caused individuals previously on the margins to engage in crime.

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1. Introduction

Economic models of criminal behaviour postulate that economic incentives matter for people's decisions on whether or not to partake in crime (Becker, 1968, Ehrlich, 1973, 1996). These could be monetary incentives generated by changes in the relative return to crime versus legitimate activities, or incentives generated by changes by in the nature of the criminal justice system.

From an empirical viewpoint the economic model of crime has been relatively successful. In his survey piece on the economics of crime Freeman (1999) states that 'most empirical evidence supports the role of incentives in the criminal decision' [Freeman, 1999, p.3530]. But it is more successful on some issues than others and commentators from other disciplines – like criminology and psychology – do tend to point out that the economic model is limited in scope.

The same is true of empirical work testing the economic model of crime. A huge amount of effort has been devoted to looking for connections between crime and unemployment and, as Freeman (1999) says, the evidence is fragile at best. A smaller body of work considers links between crime and low wages (Gould, Mustard and Weinberg, 2002, Grogger, 1998, Machin and Meghir, 2003). Here more of a consensus has been reached and low wages do seem to be significantly associated with crime. Other work shows that sanctions that increase deterrence do act to reduce crime and there is a large body of criminological evidence on this (Nagin, 1998).

In this paper we take a different approach to look at how economic incentives can affect crime. At its most basic the economic model of crime generates a crime participation equation of the form $C(W^c, W^l, p, S)$ where is W^c earnings from crime, W^l is earnings from legitimate activity (labour market earnings, E, if in work or benefits, b, if out of work), p is the probability of being caught and S is the sanction if caught. One

motivation for the large empirical literature on crime and unemployment is that those out of work have low levels of W^l, receiving benefits b. If u denotes those out of work then one can write $W^l = (1-u)E + ub$ so that the crime function becomes $C(W^c, (1-u)E + ub, p, S)$. In this simple framework an increase (decrease) in b will result in a fall (rise) in crime.

Rather than using this framework to estimate the relationship between crime and unemployment we propose to look at what happens when unemployment benefits are cut. The economic model of crime predicts a rise in crime in response to benefit cuts. We test this by setting up a quasi-experiment where benefit cuts resulted from a government policy change designed to clamp down on benefit recipients by toughening the conditions for benefit receipt. We look at what happened to crime before and after this policy change to study the links between crime and benefits.

The policy we study is the introduction of the Jobseeker's Allowance (JSA) to the UK labour market in October 1996. Justified as a simplification of the complex unemployment benefit system, JSA also brought about major changes to the entitlement to those benefits. The duration of non means-tested contributory benefits was reduced from 12 to 6 months and jobseekers were more closely monitored as rules were "changed to enable benefit to be stopped where the unemployed person's behaviour is such that it actively militates against finding work" (HMSO, 1994).

We frame our empirical analysis of the impact of the JSA introduction on crime by asking what happened to crime in areas that were more affected by the JSA as compared to those that were less affected. We therefore implement a difference-in-difference estimator of the impact of the policy introduction. Further, we assess the robustness of our results to looking at earlier time periods. This is important since results from our analysis of what happened to crime before and after JSA introduction would be

spurious if the same relationship held in earlier (non-policy intervention) time periods as well.

Our research approach using a policy intervention to generate exogenous variations can also be justified from another related, but somewhat different, perspective. Over the years a large amount of academic research has tried to evaluate the impact of government policy interventions on economic and social outcomes. Less work has considered how such policy interventions may spill over and affect other outcomes that are not explicit targets of the policy itself. In particular it may be that some policies do significantly impact on the policy variable of interest, but nonetheless may have effects (positive or negative) on other outcomes as a direct result of the policy intervention.

The focus on benefit cuts is particularly interesting here due to the large literature on the impact of changes in unemployment benefits on unemployment rates and durations (see Atkinson and Micklewright, 1991, and Machin and Manning, 1999). In this literature the focus is on whether benefit cuts do help in getting people back into work. Our focus suggests that even if such a policy has the desired impact on unemployment rates there could be a downside if people leave the unemployed stock but do not go into work (or education, training or other benefits). If benefit sanctions move people off benefits and they suffer income falls then this may lead to crime increasing. Thus rising crime would be an unintended consequence of the government policy intervention.

For such a mechanism to occur it is necessary that the policy has two effects. First, the policy would need to increase outflows from unemployment and thereby reduce the unemployment rate. Second, an increased number of the people leaving the unemployment register would need to not enter employment (or training and education). We present evidence that both of these seemed to occur on the introduction of JSA. Thus the policy induced an increased number of people not in work, education or training, or

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other benefits, but also not on the unemployment register. We argue that these people may thus be shifted into engaging in criminal activity in response to the benefit cuts. This is the hypothesis that we test in this paper.

The remainder of the paper is structured as follows. Section 2 describes the JSA system and its introduction and discusses the data we use. Section 3 discusses the empirical model and presents estimates of the cross-area associations between changes in crime and JSA introduction. Section 4 concludes.

2. The Jobseeker's Allowance and Data Description

The Jobseeker's Allowance

The Jobseeker's Allowance (JSA) was a new benefit for unemployed people which was introduced in the UK on 7 October 1996 to replace both Unemployment Benefit (UB) and Income Support (IS). It consolidated these two types of benefits into a single one for simplification purposes. The overhaul of the benefit system also established stricter job seeking rules linked to entitlement to address the government's concern that "there is still a common misunderstanding that people qualify for benefit by virtue of only being unemployed and not by what they are doing to find work" (HMSO, 1994)

From our perspective the most important feature of the policy was halving the length of benefit based eligibility on National Insurance contribution from twelve to six months. This change was implemented on 7 April 1996 so that it would take effect with the introduction of JSA the following October. This will form the basis of the at risk claimant population we consider later to measure the impact of the policy change.

The introduction of contributory JSA also implied a reduction in income for unemployed claimants who would have been previously entitled to UB. First, contributory JSA was now based on the old IS rate of benefits which gives lower benefit

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income compared to the old UB rate. As a result contribution based benefits for unemployed people aged under 25 was cut from £48.25 to £37.90 per week. Second, under contributory JSA the possibility to claim an extra £29.75 weekly for an adult dependent was no longer available. We can thus roughly estimate that the benefit income of a young unemployed person with enough NI contribution and an adult dependent would be more than 50 percent inferior under the new compared to the old system.

Entitlement for all forms of JSA was tightened with the introduction of a new set of rules intensifying the monitoring of job search activities. Claimants would now have to enter the Jobseeker's Agreement when they first sign on later used to impose stricter availability and actively seeking work tests. If the Jobseeker's agreement is not fulfilled benefit sanctions were imposed. Benefits could also be cut for two to four weeks under the new system if a claimant failed to attend a government programme enhancing employability. In addition, a sanctioned individual was no longer automatically entitled to hardship payment if otherwise entitled to income based unemployment benefit. The proof of the household "suffering hardship" as a result of the sanction would now be required.

JSA and Crime Data

The main source of data on JSA is the JUVOS (Joint Unemployment and Vacancies On-line System) Cohort, a longitudinal database of a 5 percent sample of all claims for unemployment-related benefits in the United Kingdom. The data has been collected daily by Employment Services staff since October 1982. Among the variables available in JUVOS we mostly make use of the following: length of claim; age of claimant through date of birth; location of claimant (postcode); sex; marital status; date of claim start; date

of claim end. We have extracted stock levels and outflow numbers for male [1] claimants in the quarters surrounding JSA introduction.

Due to the nature of the crime data we use (described below) our analysis is based on comparing what happened to crime rates in areas more and less affected by the JSA introduction. The level of area disaggregation we use is first based on the 43 police force areas of England and Wales. This is the level at which the Home Office publishes its crime statistics. Due to a boundary change that occurs in our sample period, we are forced to aggregate the South Wales and Gwent police force areas, leaving 42 areas, of which by far the largest is the London Metropolitan police force area. We could carry out the analysis at this level, but since large numbers of crimes are committed in London we prefer to, and are able to, break London down into smaller sub-areas. We can do this because we have access to the Metropolitan Police Crime Statistics System and therefore replace the Metropolitan and City of London police force areas with five London crime areas (Central, North-East, North-West, South-East and South-West London). More details on these data are given in Appendix 1.

For our basic analysis of the relationship between crime and JSA introduction we use Home Office data on offences reported to and recorded by the police at police force area level in England and Wales and at the borough level for the London Metropolitan Police Force. Periods of interests are the quarters before and after JSA introduction which correspond to July-September and October-December in 1996. We also benchmark our analysis of the period surrounding JSA introduction against earlier quarters when no such

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[1] We concentrate on the male population which account for three quarters of unemployed claimants and represent more than 85 percent of offenders sent to magistrates courts in England and Wales.

policy change occurred. This is done using data for five earlier quarter beginning in July 1995. [2]

We look at two different crime rates: the number of property crimes (defined as domestic burglaries and theft and handling of stolen goods) and the number of violent crimes (violence against the person) per one thousand of the population. The focus on the former might mean that one can plausibly ask if our interest in crime and JSA is partially motivated by possible links between crime and economic incentives (as we discussed in the Introduction to the paper) then why do we choose to look violent as well as non-violent crimes? The justification to look at non-violent crimes is very clear from the economic models of crime but many commentators would argue that violent crimes are much less likely (if at all) to be shaped by monetary factors. Our view is that this is essentially an unresolved issue and one can put forward arguments both ways. [3] As such we choose to

present empirical models of violent crime as well.

Measuring the Effect of JSA Introduction

Turning to the JSA data, the outflow from the claimant count in a given quarter was divided by the stock of claimants in the previous quarter to give a measure of the outflow rate. Between October and December 1996 this outflow rate stood at 14.65 percent. This compares to an average of 11.91 percent in the four preceding quarters. This is a first indication of a JSA 'effect' on unemployment flows.

Certain categories of claimants were more affected by the benefit policy overhaul than others. As noted above, the most 'at risk' group are those recipients who reached the new six -rather than twelve- month benefit cut off point in October 1996 when JSA was

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[2] We are limited to this time period by the Labour Force Survey (LFS) socio-economic controls we make use of in our model. The geographical indicator which enables us to aggregate to our crime areas level is only included in the LFS from June 1995.

implemented With the JUVOS data we can easily identify the group of claimants who were unemployed for three months at the start of the previous quarter (i.e. July). Dividing this population by the total unemployed stock in the same quarter gives us a measure of the proportion of at risk claimants.

If this measure picks up the impact of JSA we should see an increase in outflows before and after JSA introduction in areas with more at risk claimants in the quarter before introduction. This gives us a quasi-experiment setting much like that often used in the minimum wage literature where the proportion of workers beneath the minimum wage in the period before a minimum wage increase is used to identify the potential beneficiaries (Card, 1992; Machin, Manning and Rahman, 2003). Like the minimum wage literature it is, however, crucial that we analyse whether the pre-policy proportion is correlated with the variable of interest, in this case the outflow rate from unemployment.

Figure 1 considers this by plotting the change in the log(outflow rate) in the quarters surrounding JSA introduction against the proportion of at risk claimants in the pre-policy quarter for the 45 areas in our sample. The Figure reveals a clear positive relationship. The areas with a higher proportion of unemployed claimants who would reach six months in the quarter post JSA introduction also had higher increases in outflow rates. A regression line fit through the points shows the relationship to be strongly significant (coefficient = 1.73 with an associated standard error of 0.84). This is very much in line with the idea that JSA introduction was associated with significant increase in outflows from the claimant count.

Why should this matter for crime and criminality? A natural response would be that JSA type sanctions will reduce unemployment and that the people affected would

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[3] For example, Grogger (2000) presents a model of violent crime where economic incentives matter.

exit the claimant count to employment. However, when one examines the destination of those who flow off the count it is not obvious that this is the case.

Unfortunately a destination variable was only recorded in the JUVOS Cohort from September 1996 although it exists in national level data from as early as January 1995 (Sweeney, 1998). The Office for National Statistics kindly accepted to provide us with the earlier dataset containing destination information and postal districts for geographical identification.[4] Consequently we are able here to analyse the effect of the introduction of JSA on outflows to different destinations.

The destination data provides a large number of reasons for claim terminations. These can be aggregated into two broad categories. The first is mostly composed of claimants leaving the count that have found work, enrolled in full time education or training, or moved to other benefits. The second comprises those who withdrew their claim, or failed to sign in at the job centre. The former group could be referred to as unemployed claimants whose outflow is to 'somewhere' while the latter outflow is to 'nowhere'. We are not here implying that the 'nowhere' destination cannot be into employment or any other legitimate activity. We can nevertheless use this information to see if JSA induced a shift in the destinations of the outflows. We are particularly interested in whether the flow to 'nowhere' relative to 'somewhere' moved pre- and post-JSA introduction. There appears to be an increase with the percent of the outflow going to nowhere being an average of 20.5 percent in the four quarters preceding JSA introduction. This jumps to 23.8 percent in the quarter after introduction.

3 Crime and JSA Introduction

In this section we consider whether areas where JSA introduction had a greater impact differ in their evolution of crime rates before and after the policy change. We begin with a largely descriptive analysis and then turn to statistical difference-in-difference models.

Descriptive Analysis

Table 1 summarises crime rates in the quarters before and after JSA introduction for four groups areas, categorised by how much they were likely to be affected by JSA introduction. To do so we rank areas by the at risk population variable discussed above and then split them into four groups as 'Highest at risk JSA' through to 'Lowest at risk JSA'. The area groupings are given in the notes to the Table.

The relation with both property and violent crime rates is clear. Crime rises significantly in the 'Highest at risk JSA' areas before and after JSA introduction. Comparison of the change in crime rates before and after JSA between the 'Highest at risk JSA' through to 'Lowest at risk JSA' areas gives a difference-in-difference estimate of the impact of JSA on crime. This is given in bold in the Table. For both property and violent crime there is a significant positive estimate.

This is borne out for the 45 area before and after changes as shown in Figure 2. In areas with a higher proportion of unemployed claimants with at risk durations in the quarter pre JSA introduction, property crime rates rose by more. The relationship is strongly significant (coefficient = 2.73 with an associated standard error of 0.77). As such the descriptive analysis uncovers a significant positive association between JSA introduction and crime.

Basic Statistical Results From Quarters Surrounding JSA Introduction

A more formal statistical analysis is carried out in Table 2. The Table reports reduced and structural form estimates of the crime-JSA relation. The first two columns are reduced form regressions of the change in crime rates on the pre-introduction at risk population. The first is a simple regression of the change in crime on the initial period proportion and is simply the slope of the regression lines fitted through the data points given in Figure 2. The second sees how this is affected by adding in a set of demographic controls[5] and the change in the number of police. These are included so as to ensure that any correlation between crime changes and the initial at risk proportion is not due to compositional changes, or changes in police numbers, that occurred at the same time as JSA introduction. It is evident that inclusion of these controls does not much affect the estimated JSA impact which remains positive and significant.[6]

The last four columns report estimates from the structural model where the change in crime is related to the change in the outflow rate for area a in year t:

$$?\log(C_{at}) = a + ß?\log(O_{at}) + ??X_{at} + e_{at} \tag{1}$$

where C is the crime rate, O the outflow rate, X the control variables and e is a random error.

The first stage regression is:

$$?\log(O_{at}) = p + ?JSA_{a,t-1} + ??X_{at} + ?_{at} \tag{2}$$

where JSA is the at risk pre-JSA introduction proportion and ? is an error term.

The first stage regression without controls is simply the regression slope from Figure 1. This is not affected much by the inclusion of the controls where the coefficient

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us with this data.

[5] The estimated coefficients on these variables are not reported as our main concern is with the initial proportion.

[6] The focus is on 45 areas with the four London areas. However, results based on 41 police force areas using Home Office data prove similar. Coefficients (and associated standard errors) from specifications comparable to column (1) and (2) of Table 2 for property crime were 2.817 (1.150) and 2.878 (1.264).

on JSA rises from 1.73 to 1.78.   These regressions reconfirm the fact that JSA introduction did what one would expect to the outflow rate.

The final two columns of the Table show the structural model. The estimate of ß in equation (1) above is positive and strongly significant. There seems to be strong evidence that the areas which were more affected by JSA introduction were also those with bigger increases in outflow rates and, in turn, those with the biggest increases in crime.

Destination Differences

We established earlier that outflows to 'nowhere' rose between the pre- and post-JSA introduction periods. This seems in line with the notion that the benefit clampdown implicit in JSA moved more people off the register and not into employment, training or other benefits. We are thus interested in whether this flow to 'nowhere' is associated with higher crime.

Table 3 looks at differences in the outflow rate destination defined as the relative outflow to nowhere as compared to the outflow to somewhere. We term this the difference in outflow rate destinations. The Table is of the same structure as for the structural model in Table 2. A strong and significant JSA effect on crime emerges in the Instrumental Variable specifications. This is true irrespective of whether one includes controls or not. This gives us confidence that the relationship is being driven by the flows to nowhere.

Results Benchmarked Against Earlier Non-Policy Introduction Quarters

The results so far point to relative crime increases in areas where a greater proportion of workers were likely to be affected by JSA introduction. This is strongly suggestive of links between crime and benefit cuts and sanctions. But what if the same links existed in periods before JSA introduction? Our finding would turn out to be

spurious if the same kind of link did exist. Indeed were it the case that crime rates also rose in relative terms by a similar magnitude in areas with more people in the at risk duration categories in time periods when the JSA was not present, then our results could not be attributed to the introduction of JSA.

To rule out this possibility and ensure we are actually identifying changes resulting from JSA introduction we need to look at econometric models specified in the same way as those already considered for earlier time periods. Thus, Table 4 shows a set of results from regressions that benchmark the results against the relationship between changes in crime and the initial at risk proportion in earlier time periods. As these add a further differenced set of data, in the control periods where there was no JSA policy, one can think of these estimates as triple differenced, or difference-in-difference-in-difference estimates.[7]

Table 4 reports coefficients on the initial at risk proportion in the longer benchmarking period. The coefficients reported in the first column of the Table make it clear that our earlier estimates are not picking up a relationship that existed in earlier time periods. For both property and violent crimes the coefficient on the at risk proportion is seen to be significantly more positive in the period surrounding JSA introduction than in the comparison periods. The same is true when the control variables are added in Table 4.

The nature of the data, on the same areas followed through time, means that one can also adopt an even more stringent test by including area-specific trends in the estimating equation. The final column of the Table therefore additionally includes 45 area trend variables. The coefficients are marginally reduced by this inclusion and, of course as one would expect, the standard errors rise, yet the results remain robust to this.

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[7] Or alternatively one can think of the estimates as difference-in-difference estimates of the change model (i.e. double-differenced in changes rather than triple-differenced in levels). As such they compare the

Overall it seems that benchmarking against earlier time periods acts to reinforce and corroborate the findings presented before. There appears to be a stronger positive relationship between crime and the at risk proportion in the period surrounding JSA introduction. This is a robust finding and is in line with the idea that the altering of economic incentives brought about by the JSA may well have caused individuals previously on the margins to engage in crime.

Discussion

The empirical analysis shows that areas where JSA had a bigger impact of moving people off the unemployment register were areas where crime rose by more. It also shows that, rather than going into work or education/training, the outflows to 'nowhere' significantly rose in the areas where crime went up most. This, of course, is suggestive that the benefit cuts and sanctions imposed by JSA had an unexpected social cost on society by raising crime.

But the evidence, by its very nature, only indirectly confirms the existence of a positive link between benefit cuts and crime as we do not have individual data on whether the people who exited the unemployment register actually turned to crime. Getting such data is extremely hard and we are not aware of any representative source that exists. However, some qualitative research conducted around the time of JSA introduction (Vincent, 1998) is highly suggestive of the notion that crime was high in the thinking of disallowed and sanctioned individuals. In her analysis of 'effects on attitudes and behaviour' of her 30 interviewees, Vincent (1998) reports that four out the thirty respondents (three men, one woman) expressed the feeling that after losing their benefits "they had been driven to an extremity in which crime might be their last resort". The woman said:

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relationship between changes in crime and the initial at risk proportion in the treatment period surrounding

> *'If it wasn't for the children I should have had to go soliciting or something… I'd got to get the money from somewhere which is stupid anyway, because I could have turned to crime, shoplifting, couldn't I? Stealing out of somebody's house to sell something to get me some cash.'*

The male respondents also expressed the sentiment that property crime was an option to obtain some income and perhaps the only alternative for them under the circumstances.[8] One of them said:

> *'If it hadn't been helped out by my parents… I would have had to steal because I had no food in the cupboards, I think I had a couple of cans of beans, something like that, but I know for a fact I would have had to steal. They're asking for trouble, they're asking for people to steal."*

Moreover some basic statistical analysis on data extracted from the interviews of the sanctioned claimants[9] sheds some light on characteristics of those on the margins of crime participation. The individuals most likely to mention property crime as an alternative source of income are young males in urban areas. This reassuringly fits the commonly observed profiles of those more likely to engage in crime.

In addition we can make some comments on the reason why individuals were disallowed from JSA receipt. We find that the potential crime candidates are more likely to have lost their benefits for not having satisfactorily proven that they were 'actively seeking work' or 'failed to carry out a mandatory employment programme'. Interestingly these were two areas of the monitoring unemployment benefits claimants greatly strengthened by the introduction of JSA and the accompanying Jobseeker's Agreement.

This qualitative evidence complements our statistical work nicely and is in line with the notion that JSA introduction did, in fact, result in sanctioned and disallowed

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JSA introduction with the same relationship in the earlier (non-JSA) control periods.

[8] Vincent's (1998) report also highlights a general feeling of frustration or anger among the respondents. Whilst we would not want to make too much of this, work by psychologists in criminology (Dollard et al., 1939; Berkowitz, 1989) does highlight that such feelings can act as a determinant of violent crime.

[9] We are grateful to Sue Middleton from the CRSP at Loughborough University for giving us access to the original transcripts of the interviews.

individuals turning to crime as one would expect in a model where benefit cuts can alter individual's incentives to participate in crime.

4. Concluding Remarks

In this paper we look at the relationship between crime and benefit sanctions using a quasi-experimental setting induced by the introduction of the Jobseekers allowance (JSA) in Britain in October 1996. We look at crime rates in areas more and less affected by JSA before and after JSA introduction. In the areas more affected by JSA introduction, crime rose by more. These were also the areas with higher outflows from unemployment and particularly to people dropping off the register but not into work or onto other benefits. As such these results seem to reflect that benefit sanctions in JSA shifted people off the benefit system and raised crime.

These are important results for at least two reasons. First, they tend to confirm that economic incentives matter for crime. We reach this conclusion in rather a different way to the usual crime literature which focuses much more on labour market measures. Our approach, by focussing on changes before and after a policy change, tends to emphasise the dynamics of the relation between crime and incentives. Second, the results show how some government policies may have unintended consequences. The results are in line with the idea that removal of benefits, benefit cuts and sanctions need not have got people back into work but, at least for some of those exiting the claimant count, there was a social cost to society resulting from higher crime.

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Figure 1:
Changes in Relative Outflow in the Quarters
Before and After JSA Introduction and
Initial Quarter Proportion of At Risk Claimants

The circles are proportional to the population size of the 45 areas
Population weighted regression line fit through data points:
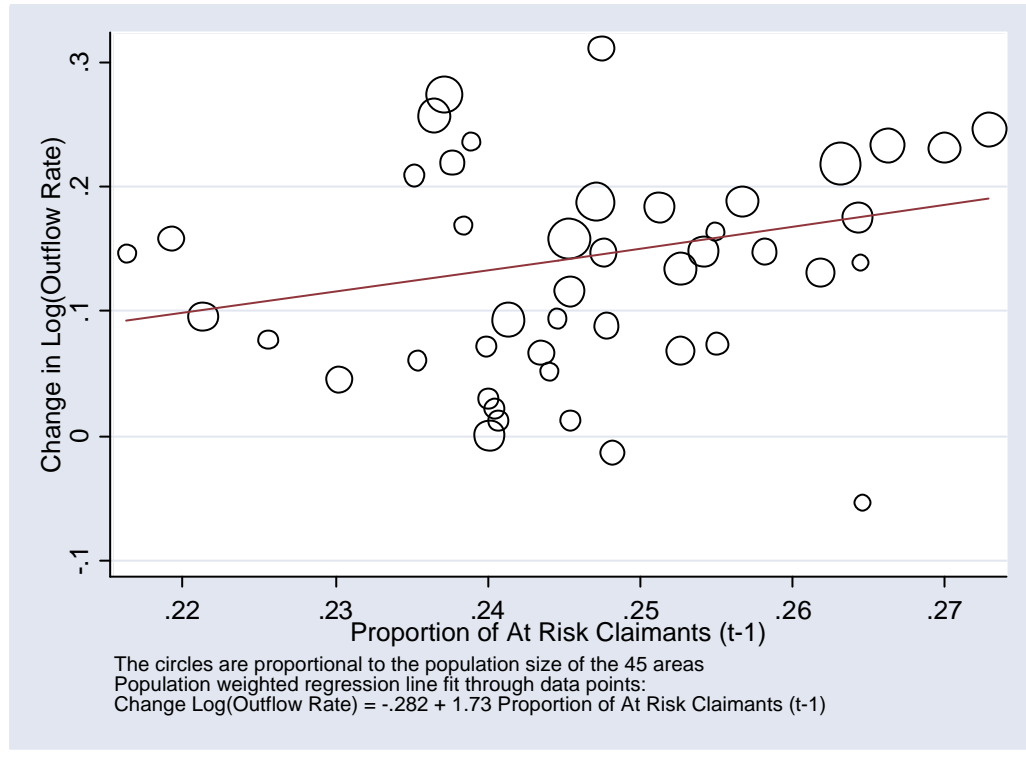Change Log(Outflow Rate) = -.282 + 1.73 Proportion of At Risk Claimants (t-1)

Figure 2:
Changes in Property Crime Rates in the Quarters
Before and After JSA Introduction and
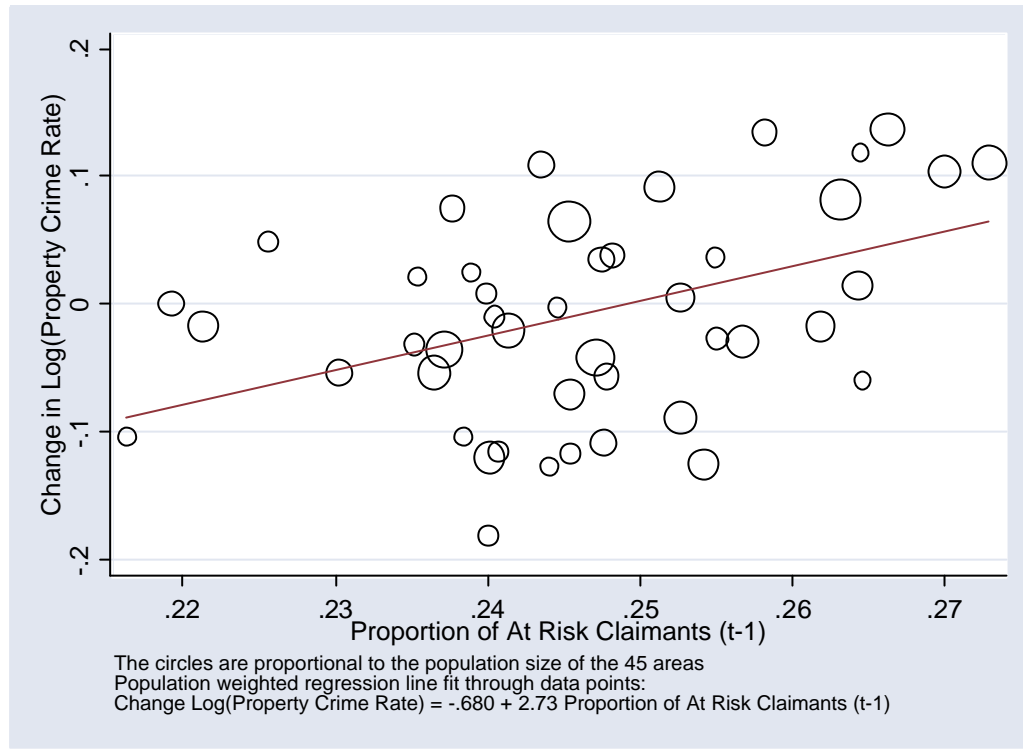# Initial Quarter Proportion of At Risk Claimants

The circles are proportional to the population size of the 45 areas
Population weighted regression line fit through data points:
Change Log(Property Crime Rate) = -.680 + 2.73 Proportion of At Risk Claimants (t-1)

	Quarter Before JSA Introduction	Quarter After JSA Introduction	Change (Standard Error)
Property Crime			
Highest at risk JSA	15.87	17.01	1.14 (1.30)
2nd Highest at risk JSA	14.87	14.58	-.295 (1.01)
2nd Lowest at risk JSA	13.21	13.01	-.205 (1.13)
Lowest at risk JSA	11.73	11.47	-.266 (.502)
Highest – Lowest	4.13 (1.70)	5.55 (1.95)	**Difference-in-difference = 1.41 (.448)**
Violent Crime			
Highest at risk JSA	2.29	2.38	.089 (.290)
2nd Highest at risk JSA	1.63	1.56	-.073 (.125)
2nd Lowest at risk JSA	1.57	1.47	-.101 (.083)
Lowest at risk JSA	1.31	1.20	-.114 (.290)
Highest - Lowest	.981 (.319)	1.18 (.368)	**Difference-in-difference = .203 (.076)**

Notes: Areas are split into four (almost) equal sized groups of police force areas (three groups of 11 and one of 12 areas). The groupings are based upon the male unemployment rate in the quarter prior to the introduction of JSA. Areas in the Highest group had more than 25.50 percent of at risk male claimants. Areas in the 2nd Highest group have between 24.54. and 25.50 percent of at risk male claimants. Areas in the 2nd Lowest group have between 23.84 and 24.54 percent of at risk male claimants. Areas in the Lowest group have less than 23.84 percent of at risk male claimants. The areas in each group are as follows: Highest – Central London, Cumbria, Kent, Merseyside, Norfolk, Northamptonshire, North-East London, North-West London, Nottinghamshire, South-East London, , West Midlands; 2nd Highest – Cleveland, Derbyshire, Essex, Humberside, Leicestershire, North Wales, South-West London, South Yorkshire, Sussex, West Mercia, West Yorkshire; 2nd Lowest – Avon & Somerset, Bedfordshire, Devon & Cornwall, Dorset, Durham, Greater Manchester, Hampshire, Lincolnshire, Northumbria, Staffordshire, Suffolk, Wiltshire; Lowest – Cheshire, Dyfed-Powys, Cambridgeshire, Gloucestershire, Hertfordshire, Lancashire, North Yorkshire, South Wales/Gwent, Surrey, Thames Valley, Warwickshire.
Standard errors are in parentheses.

Table 2:
Changes in Log(Crime Rates) Equations
in the Quarters Before And After Introduction of JSA

	Reduced Form OLS Model		Structural IV Model			
A. Property Crime Models	Change in Log(Property Crime Rate), OLS		Change in Log(Outflow Rate), OLS		Change in Log(Property Crime Rate), IV	
Proportion of At Risk Claimants in Pre-JSA Quarter	2.73 (0.77)	2.69 (0.90)	1.73 (0.84)	1.78 (1.07)		
Change in Log(Outflow Rate)					1.58 (0.67)	1.51 (0.83)
Area Controls	No	Yes	No	Yes	No	Yes
R-Squared	.198	.343	.076	.154		
B. Violent Crime Models	Change in Log(Violent Crime Rate), OLS		Change in Log(Outflow Rate), OLS		Change in Log(Violent Crime Rate), IV	
Proportion of At Risk Claimants in Pre-JSA Quarter	2.71 (1.17)	2.61 (1.25)	1.73 (0.84)	1.78 (1.07)		
Change in Log(Outflow Rate)					1.57 (0.82)	1.47 (0.94)
Area Controls	No	Yes	No	Yes	No	Yes
R-Squared	.154	.297	.076	.154		

Notes: Coefficients (heteroskedastic consistent standard errors) reported. The sample size in all regressions is 45 areas. All regressions weighted by area population. The demographic controls entered were logs of – change in average age, change in lower quartile wage, change in proportion of young male, change in number of police/security personnel.

Table 3:
Changes in Log(Crime Rates) in the Quarters
Before And After Introduction of JSA – Using Outflow Destinations

	Structural IV Model			
A. Property Crime Models	Difference in Outflow Rate Destination, OLS		Change in Log(Property Crime Rate), IV	
Proportion of At Risk Claimants in the Pre-JSA Quarter	1.68 (0.27)	1.67 (0.32)		
Difference in Outflow Rate Destination			1.63 (0.39)	1.51 (0.83)
Area Controls	No	Yes	No	Yes
R-Squared	.540	.558		
B. Violent Crime Models	Difference in Outflow Rate Destination, OLS		Change in Log(Violent Crime Rate), IV	
Proportion of At Risk Claimants in the Pre-JSA Quarter	1.68 (0.27)	1.67 (0.32)		
Difference in Outflow Rate Destinations			1.62 (0.57)	1.57 (0.61)
Area Controls	No	Yes	No	Yes
R-Squared	.540	.558		

Notes: Coefficients (heteroskedastic consistent standard errors) reported. The sample size in all regressions is 45 areas. All regressions weighted by area population. The demographic controls entered were logs of – change in average age, change in lower quartile wage, change in proportion of young male, change in number of police/security personnel.

Table 4:
Benchmarking Against Earlier Time Periods
(Models Estimated Over JSA Introduction Quarter and Four Previous Quarters)

	(1)	(2)	(3)
	Basic Specification,	(1) + Controls	(2) + Area Trends
A. Property Crime Models			
Proportion of At Risk claimants in Quarters Surrounding JSA Introduction, OLS	2.75 (.892)	2.81 (.932)	2.69 (1.04)
R-Squared	.163	.180	.245
B. Violent Crime Models			
Proportion of At Risk claimants in Quarters Surrounding JSA Introduction, OLS	2.59 (1.17)	2.73 (1.22)	2.49 (1.34)
R-Squared	.464	.471	.533

Notes: Coefficients (heteroskedastic consistent standard errors) reported. Sample sizes is 225 (45 areas over 5 quarters). All regressions weighted by population. The demographic controls entered were – change in average age, change in lower quartile wage, change in proportion of young male, change in police/security personnel. All equations include dummy variables for time period.

Appendix 1

London Crime Data

Crime data for the five areas aggregating into the London Metropolitan Police Force were compiled from the "Metropolitan Police: Crime Statistics System (ME)". This database was provided by the University of London Computer Center (ULCC) who are in charge of the National Digital Archive Data (NDAD). The information contained relates to offences, clear-ups, arrests, victims of crime, property stolen and reports classified as "No Crime". Each record includes details on the nature of the crime, the date at which it took place and various geographical indicators indicating where it was reported. We have attempted to match as precisely as possible this data to the quarterly property and violent crime statistics for the London Metropolitan Police Force of recorded crimes from the Home Office (HO).

We have only taken into account records which relate to an "an offence not-cleared-up" and "an offence not cleared up". The categorization of a crime into the property or violent category is dependent on the five digit Home Office Classification Code in each record. Property crimes consist of domestic burglaries (HO Codes 2800 to 2804, 2900 and 3000) and theft and handling of stolen goods (HO Codes 3700, 3702 to 3799, 3900 to 4999 and 5400 to 5499). Violent crimes comprise violence against the person (HO Codes 100 to 899, 1100 to 1599 and 3701) and robbery (HO Codes 3400 to 3499). The data is aggregated into five areas: Central (City of London, City of Westminster, Royal Borough of Kensington and Chelsea, and London Boroughs of Hammersmith and Fulham), North West (London Boroughs of Camden, Brent, Harrow, Barnet, Bexley, Ealing, Islington and Haringey), North East (London Boroughs of Waltham Forest, Redbridge, Enfield, Hackney, Tower Hamlets, Havering, Barking and Dagenham and Newham), South East (London Boroughs of Southwark, Lewisham, Bromley, Greenwich, Croydon and Sutton), and North West (London Boroughs of Richmond on Thames, Hounslow, Hillingdon, Wandsworth, Lambeth and Merton; Royal Borough of Kingston upon Thames). The date at which the crime was recorded by the police determines the quarter in which it is included.

When data from the 5 areas is aggregated to compare it to HO recorded crime numbers for the London Metropolitan Police Force this procedure does not yield perfect matches. The discrepancies are however marginal and some basic statistical analysis confirm that we can safely use the generated data. The correlation between our aggregated property crime data and the HO figures from the second quarter of 1995 to the fourth quarter of 1996 is a reassuring 0.9989 and 0.9973 for violent crime. In terms of changes of log(crime rates), the measure used as dependent variable in our regressions, the correlation are 0.9995 for property and 0.9879 for violent crime.