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Crime and Benefit Sanctions

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Abstract

In this paper we look at the relationship between crime and economic incentives in a different way to other work in this area. We look at changes in unemployment benefits and the imposition of benefit sanctions as a means of studying the way that people on the margins of crime may react to economic incentives. The paper relies on a quasiexperimental setting induced by the introduction of the Jobseekers Allowance (JSA) in the UK in October 1996. We look at crime rates in areas more and less affected by the policy change before and after JSA introduction. In the areas more affected by the tougher benefit regime 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, education/training or onto other benefits. Areas that had more sanctioned individuals also experienced higher crime rates after the introduction of JSA. As such the benefit cuts and sanctions embodied in the JSA appear to have induced individuals previously on the margins to engage in crime. Thus there appears to have been an unintended policy consequence, associated with the benefit reform, namely higher crime.

JEL Keywords: Crime; Benefit Sanctions; Jobseekers Allowance
JEL Classification: H00; J65

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 in the nature of the criminal justice system.

From an empirical standpoint the economic model of crime has had some success. 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 a huge amount of effort has been devoted to looking for connections between crime and unemployment, the evidence on which Freeman (1999) says 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, 2004). 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. Our approach is firmly grounded in theory recognising that, at its most basic, the economic model of crime (as in Becker, 1968, or Ehrlich, 1973) generates a crime participation equation of the form $C(W^c, W^l, p, S)$ where W^c is earnings from crime, W^l is earnings from legitimate activity (equal to 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 when they receive benefits b. If u denotes the proportion 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 following much of the literature by looking at the relationship between crime and unemployment, we study links between crime and economic incentives in a different way to other work in the literature by focusing on what happens to crime when unemployment benefits are cut, or sanctions are imposed. We consider this in a quasi-experimental setting where benefit cuts resulted from a government policy change designed to clamp down on benefit recipients by toughening the conditions for benefit receipt.

The policy we study is the introduction of the Jobseeker's Allowance (JSA) to the UK labour market in October 1996. JSA introduction was justified as a simplification of the complex unemployment benefit system and brought in a much more stringent, tougher regime. It brought about major changes to the entitlement to those benefits and introduced sanctions through a tightening of the monitoring of job search activities. 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). Overall JSA introduction resulted in a general toughening and tightening of the benefit regimes.

We frame our empirical analysis by asking what happened to crime in areas that were more affected by the JSA introduction 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. We also consider possible longer run effects looking at benefit sanctions in the twenty five quarters after JSA introduction.

Our research approach of 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 (unemployment in the case of JSA), but nonetheless may have effects (positive or negative) on other outcomes (like crime) as a direct result of the policy intervention.

The focus on benefit cuts and sanctions is of particular interest 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 one focus is on whether benefit cuts do help in getting people back into work. Our analysis 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 their income falls this may impact on crime, resulting in an unintended consequence of the government policy intervention. Thus, despite our focus on a particular UK benefit reform, our findings are potentially of general applicability and relevance to welfare to work programmes the world over.

The remainder of the paper is structured as follows. Section 2 describes the JSA and its introduction, discusses the data we use and presents some descriptive analysis. Section 3 discusses the empirical model, presents estimates of the cross-area associations

between changes in crime and JSA introduction and presents a discussion of some related work of a more qualitative nature. 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 introduced in the UK on 7 October 1996. It replaced both Unemployment Benefit (UB) and Income Support (IS) by consolidating these two benefits into a single one. It also established more stringent, 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). The policy halved the length of eligibility based benefit on National Insurance contributions from twelve to six months. This change was implemented on 7 April 1996 so that it would take effect with the introduction of JSA six months later. We use this reform to define a claimant population that is at risk of facing benefit cuts or sanctions.

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 income compared to the old UB rate. As a result contribution based benefits for unemployed people aged under 25 were cut from £48.25 to £37.90 per week (corresponding to a percentage reduction of 21%). Second, under contributory JSA the possibility to claim an extra £29.75 weekly for an adult dependent was no longer available. Thus the benefit income of a young unemployed person with enough NI

contributions and an adult dependent would be more than 50 percent lower under the new system as compared to the old.

Benefit entitlement was also tightened with the introduction of a new set of rules intensifying the monitoring of job search activities. Claimants have to enter the Jobseeker's Agreement when they first sign on and, if the agreement is not fulfilled, benefit sanctions are imposed. Benefits could also be cut for two to four weeks under the JSA if a claimant failed to attend a government programme aimed at enhancing employability. In addition, a sanctioned individual was no longer automatically entitled to hardship payment if otherwise entitled to income based unemployment benefit. Proof of the household "suffering hardship" as a result of the sanction would now be required.

One should also recognise that there have been many reforms to the UK benefit system over time (see Table 11.2 in Van Reenen, 2004). However, the period when JSA was introduced was actually a relatively quiet period in terms of reform, where there were no other policy changes taking place at or around the same time. This is important for the research design we adopt in that we would not want our effects to be confounded by other policy changes that occurred at the same time. For example, this would be a problem if one wanted to look at benefit reform changes contained in the New Deal programme that occurred later on in the 1990s (Van Reenen, 2004).

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

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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 available crime data, and the timing of JSA introduction, our analysis is based on comparing what happened to quarterly 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 has historically published 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 the London data and area configurations are given in the Appendix.

Our analysis uses Home Office crime 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. To start with the periods of interests are the quarters before and after JSA introduction which correspond to July-September and October-December in 1996. But we also benchmark our analysis of the period

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[1] We concentrate on the male population, accounting for three quarters of unemployed claimants and representing more than 90 percent of offenders sent to magistrates courts in England and Wales.

surrounding JSA introduction against earlier quarters when no such policy change occurred. This is done using data for five earlier quarters 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 justification to look at non-violent crimes is very clear from standard economic models of crime (Becker, 1968; Ehrlich, 1973) 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 14.7 percent. This compares to an average of 11.9 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 the October-December 1996 quarter when JSA was 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.

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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.

If this measure picks up the impact of JSA we should see a bigger 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-experimental 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 statistically significant (coefficient = 1.84 with an associated standard error of 0.86). This is very much in line with the idea that JSA introduction was associated with a 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 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. For example, a research summary on JSA by Rayner et al (2000) states that 'the initial increase in

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

movements off benefit was due to a 'weeding out' of those who were not previously assiduous in their job search or were claiming fraudulently' [p. 7].

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 agreed to provide us with the earlier dataset containing destination information and postal districts for geographical identification. Consequently we are able 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 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. Indeed 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 of areas, categorised by how much they were likely to be affected by JSA introduction. To do this 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 precise 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 after JSA introduction. Comparison of the change in crime rates before and after JSA between the 'Highest at risk JSA' and the '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 before and after changes shown in Figure 2. In areas that have more unemployed claimants with at risk durations in the quarter pre JSA introduction, property crime rates rose by more. The relationship is strongly significant (coefficient = 2.99 with associated standard error of 0.72). 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 of the form:

(1) $\qquad \Delta\log(C_{at}) = \alpha + \beta JSA_{a,t-1} + \gamma\Delta X_{at} + e_{at}$

where C is the crime rate in area a in quarter t, JSA is the pre-JSA introduction at risk proportion, e is a random error and X denotes a set of demographic controls included so as to ensure that any correlation between crime changes and the initial at risk proportion is not due to compositional changes occurring at the same time as JSA introduction.

The Table reports two sets of estimates of equation (1). The first (specifications (1) and (7)) is a simple regression of the change in crime on the initial period proportion excluding the control variables and so is simply the slope of the regression lines fitted through the data points given in Figure 2. The second (specifications (2) and (8)) adds in the controls.[4] It is evident that inclusion of these controls does not much affect the estimated JSA impact which remains positive and significant and, in fact, actually rises when they are added.[5]

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

(2) $\qquad \Delta\log(C_{at}) = \eta + \delta\Delta\log(O_{at}) + \theta\Delta X_{at} + \varepsilon_{at}$

where ε is a random error.

The first stage regression is:

(3) $\qquad \Delta\log(O_{at}) = \pi + \lambda JSA_{a,t-1} + \rho\Delta X_{at} + \xi_{at}$

with error term ξ.

The identification of an outflow rate effect on crime comes from the notion that the at risk proportion only affects crime through changes in the outflow rate induced by

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

the JSA introduction. This seems plausible in that the JSA policy change differentially affected outflows for particular duration groups and that, over and above working through increased outflows, one should not see any direct effect of this at risk duration group on crime. As one will see below, there is absolutely no evidence of a direct association between crime changes and the at risk proportion in earlier non-policy time periods. The only significant relationship between the changes in the outflow rate and the initial at risk proportion occurs in the quarters surrounding JSA introduction.

The first stage change in outflow regression without controls is simply the regression slope from Figure 1, which is reproduced in columns (3) and (9) of the Table. This estimate is not affected much by the inclusion of the controls, as the coefficient on JSA falls only moderately, from 1.84 to 1.63. These regressions reconfirm that the group more affected by JSA introduction were, as hypothesised, significantly more likely to flow from the claimant register once JSA was introduced.

The final two columns of the Table show the structural model. The estimate of δ from equation (2) above is positive and strongly significant. The Instrumental Variable estimate is the ratio of the reduced form coefficients on the at risk proportion. For example, for specification (5) it is the ratio of coefficients from specifications (1) and (3) (i.e. 2.99 / 1.84) or more generally it is β/λ from equations (1) and (3) above.[6] 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. These effects are present for both property and violent crime.

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[5] The focus is on 45 areas, incorporating the four London areas. However, results based on 41 police force areas using Home Office data proved similar (results available on request).

[6] To see this substitute (3) into (2) to derive the change in crime reduced form equation as $\Delta\log(C_{at}) = (\eta +\delta\pi) + \delta\lambda JSA_{a,t-1} + (\theta +\delta\rho)\Delta X_{at} + \delta\xi_{at} + \varepsilon_{at}$. Comparison with equation (1) makes it clear that $\beta = \delta\lambda$ (and that $\alpha = \eta +\delta\pi$, $\gamma = \theta +\delta\rho$ and $e_{at} = \delta\xi_{at} + \varepsilon_{at}$).

Destination Differences

We established earlier that outflows to 'nowhere' rose between pre- and post-JSA introduction periods. This is in line with the notion that the benefit clampdown implicit in JSA moved more people off the register, but not into employment, training or other benefits. We are interested in whether the increased flow to 'nowhere' was 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 (3), (4), (7) and (8). This is true irrespective of whether one includes controls or not. This gives us confidence that the relationship is being driven by the increased 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 identifying changes resulting from JSA introduction we need to look at 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]

The basic crime reduced form specification now becomes:

(4) $\Delta \log(C_{at}) = \alpha + \beta\, JSA_{a,t-1} + \beta^P\, I(t > \tau) * JSA_{a,t-1} + \gamma\, \Delta X_{at} + \gamma^P\, I(t > \tau)\Delta X_{at} + \varepsilon_{at}$

where $I(t > \tau)$ is an indicator function taking the value 1 for the period after policy introduction (at time τ) and 0 for the pre-policy periods ($t < \tau$). The key parameter of interest is now β^P, where the P denotes the policy on period.

Table 4 reports the β^P coefficients on the initial at risk proportion in the policy on 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 estimate of β^P is significant and positive, showing the effect from the at risk proportion to be more significantly positive in the period surrounding JSA introduction than in the comparison periods.[8] The same is true when the control variables are added in specifications (2) and (5) in Table 4.

The fact that there is only an association between changes in crime and the at risk proportion in the policy on period is shown in Figure 3. The Figure shows the estimated relationship in the policy on period as compared to the average of the four previous (non-policy on) quarters. This is taken from the estimated models with controls in Table 4

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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 relationship between changes in crime and the initial at risk proportion in the treatment period surrounding JSA introduction with the same relationship in the earlier (non-JSA) control periods.

[8] The coefficient on the level, corresponding to the pre-policy baseline period, was estimated as .02 (standard error = .33) for the column (1) property crime specification and .13 (.46) for the column (4) violent crime specification.

(specifications (2) and (5)). The shift in the slope from no relationship to a significant positive association is very clear.

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. Specifications (3) and (6) in the final columns of the Table therefore additionally include 45 area trend variables. The coefficients are marginally reduced by this inclusion, yet the results remain robust.

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, 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 of data 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:

> *'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.[9] 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[10] 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

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

seeking work' or 'failed to carry out a mandatory employment programme'. Interestingly these were two areas in the monitoring of claimants' unemployment benefit entitlement that were greatly strengthened by the introduction of JSA and the accompanying Jobseeker's Agreement.

This qualitative evidence complements the statistical work and is in line with the notion that JSA introduction did, in fact, result in sanctioned and disallowed individuals turning to crime as one would expect in a model where benefit cuts can alter an individual's incentives to participate in crime.

Longer Run Impact

One feature of the JSA system is that it imposes sanctions on people who do not fulfil the job search requirements imposed by the Employment Service. It is therefore possible to investigate whether there is a longer run impact on crime by looking at the relationship between changes in crime and changes in the proportion sanctioned over time. To do so we have put together a panel of crime and sanctions for the 41 police force areas for the 25 quarters following JSA introduction (between 1996 Quarter 4 and 2002 Quarter 4).[11]

Table 5 reports estimates of the relation between crime and benefit sanctions. As area fixed effects are included in all specifications, one can interpret the results as informing the following question: what happens to cross-area patterns of crime if sanctions increase by more in one area compared to another? In the basic specifications with and without controls (specifications (1) and (2) for property crimes and (5) and (6) for violent crimes) there is some support for the notion that more JSA sanctions are

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[11] Unfortunately we do not have access to the Metropolitan Police Crime Statistics System data for this time period and therefore have to look at the 41 area configuration as we are unable to disaggregate the London data.

associated with increases in crime. The effect is particularly strong for property crimes, but is rather imprecisely determined (with large standard errors) for violent crimes.

Since we have data for the 25 quarters following JSA introduction we can also explore whether this is a long lasting crime effect, or if there are temporal variations, particularly if any decay effect sets in once individuals learn and become accustomised to the new benefit regime. This is done in the specifications reported in the final two columns of the Table where the coefficient on the proportion sanctioned variable is allowed to differ across six periods (annual except for 1996Q4 to 1997Q4 which covers five quarters). The pattern is interesting, showing strong effects for property crime shortly after JSA introduction, with the estimated effects dying away thereafter (remaining mostly positive, but statistically insignificant).

From looking at benefit sanctions data after JSA introduction we are thus able to reconfirm the findings from the period surrounding JSA introduction, namely that there appears to be an unintended consequence of increased crime associated with the Job Seekers Allowance. Whilst the policy was successful in reducing the claimant count, the benefit cuts and sanctions that are an important part of the policy were associated with increased crime rates, at least in the period surrounding and the couple of years after its introduction.

Implications for Cost-Benefit Evaluation of JSA Introduction

On the basis of the shifts in the outflow rate, and the estimated connections with changes in crime, we have also carried out a 'back of the envelope' cost-benefit calculation to show the implications of the crime-benefits connection for evaluating the economic impact of the JSA policy. The link with crime that we have uncovered is an additional social cost that is not usually considered in economic evaluations of welfare to work programmes like JSA. Yet our evidence suggests this negative consequence of the

policy to be empirically important. This has general implications for evaluations of JSA type welfare to work programmes that ignore the crime link in that they will potentially over-estimate possible benefits of such policies.

Our calculations show JSA to have been a cost effective policy reform. Table 6 shows estimates of the net social benefits of JSA in the quarter following its introduction based upon two different outflow measures when we do and do not consider the link with crime. In the upper panel of the Table we estimate the net social benefit to be between £77 and £99 million. However, once we incorporate the average extra cost from the additional 7,484 property and 854 violent crimes we estimate occurred[12], using Home Office estimates of the social cost of crime from Brand and Price (2000), the net social benefit falls to a range of between £55 and £77 million. Thus, if the connection with crime was not considered one would overestimate the net social benefits by somewhere between 22 and 28 percent.

This establishes that the unintended consequence of the policy, where crime also rose as people were moved off the unemployment register by JSA, appears to be empirically important. Of course, one may be interested in whether this connection with crime is a universal feature of stricter benefit regimes embodied within policy changes to welfare to work type programmes. To properly answer this important question we need more evidence looking at the implications of stricter benefit regimes for crime in different settings and this should be an issue placed firmly upon the agenda for future research.

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[12] This is a very small proportion of the overall number of crimes in the quarter (around 1.309 million property and 114,000 violent crimes). The social cost of these crimes, according to the Brand-Price valuations, is over £3,000 million.

4. Concluding Remarks

In this paper we look at the relationship between crime and benefit cuts and sanctions using a quasi-experimental setting induced by the introduction of the Jobseekers allowance (JSA) in the UK in October 1996. We study crime rates in areas more and less affected by the policy 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. Studying the relation between crime and sanctions after introduction also confirms that areas where more people were sanctioned were those where crime rose by more. As such these results seem to reflect that benefit cuts and sanctions in JSA shifted people off the benefit system and raised crime.

These are important results for at least two reasons. First, they confirm that economic incentives matter for crime. We reach this conclusion in rather a different way to the usual crime literature by looking at changes before and after a policy change that induced benefit cuts and sanctions. This 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 one feature of the removal of benefits, benefit cuts and sanctions, at least in the context we study, was the imposition of a social cost to society due to higher crime.

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Figure 1:
Changes in Relative Outflow in the Quarters
Before and After JSA Introduction and
The 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:
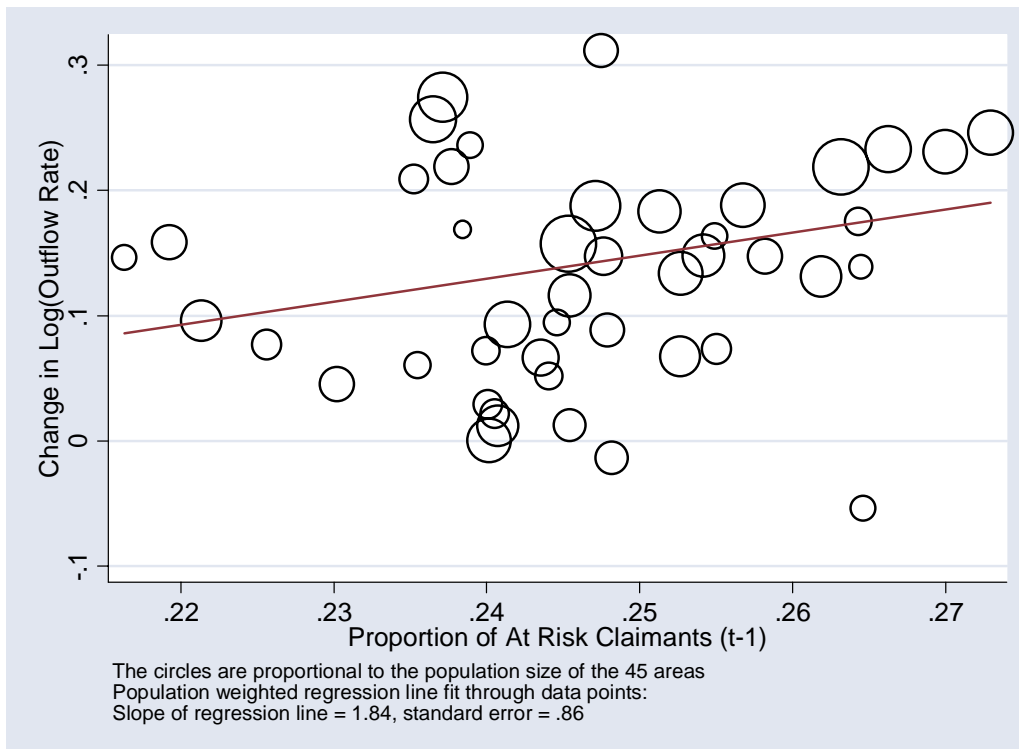Slope of regression line = 1.84, standard error = .86

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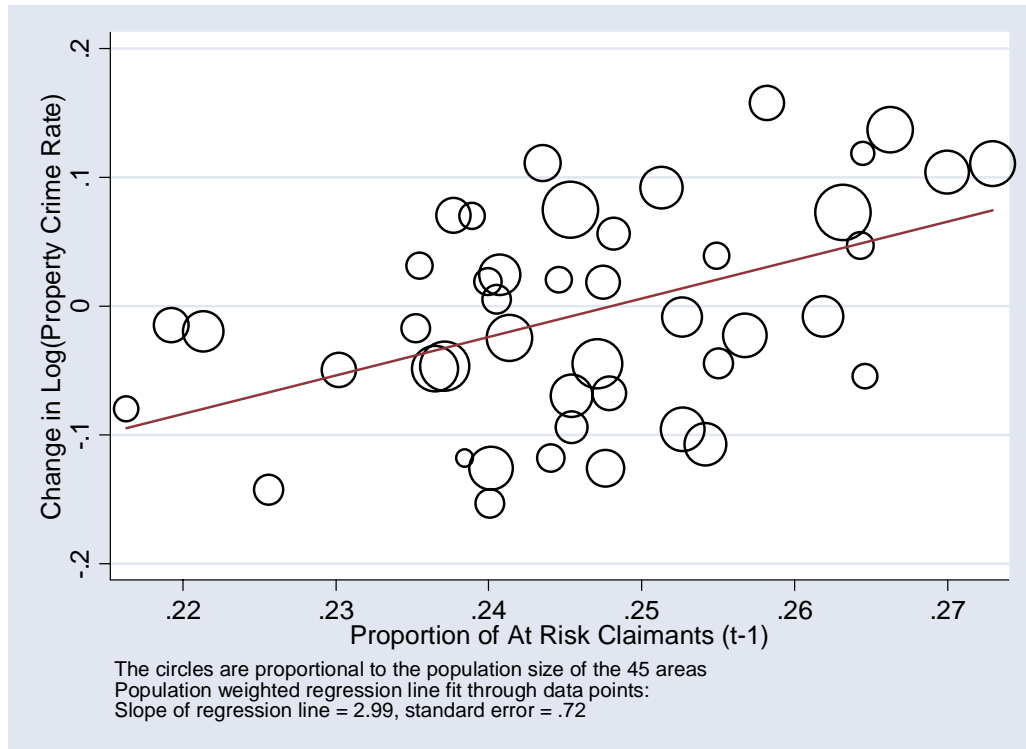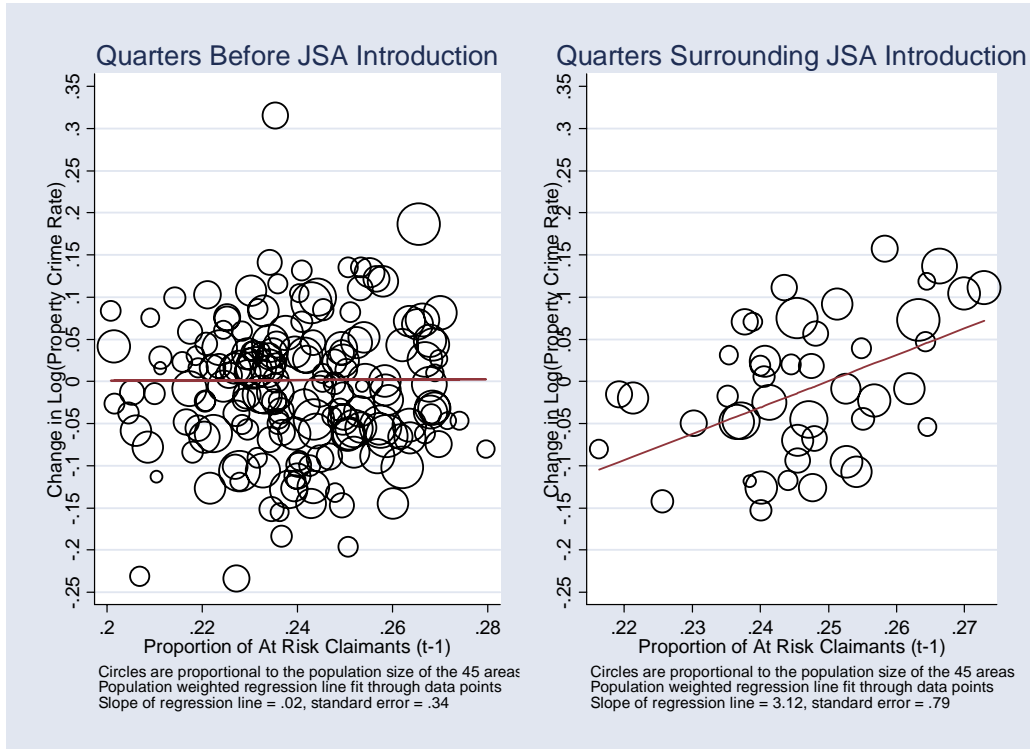**The Initial Quarter Proportion of At Risk Claimants**

The circles are proportional to the population size of the 45 areas
Population weighted regression line fit through data points:
Slope of regression line = 2.99, standard error = .72

Notes: The regression lines are those from specification (2), including controls, in Table 4. The gap between the slopes produces the difference-in-difference estimate of 3.11(.86) given in Table 4.

	Quarter Before JSA Introduction	Quarter After JSA Introduction	Change (Standard Error)
Property Crime Rate (Per 1000)			
Highest at risk JSA	19.93	21.36	1.43 (1.55)
2nd Highest at risk JSA	20.26	19.74	-.53 (1.29)
2nd Lowest at risk JSA	19.46	19.49	.02 (1.63)
Lowest at risk JSA	17.63	17.04	-.60 (.81)
Highest – Lowest	2.30 (2.39)	4.33 (2.62)	Difference-in-difference = **2.02 (.60)** [Percent of pre-policy mean = 10.4%]
Violent Crime Rate (Per 1000)			
Highest at risk JSA	2.45	2.55	.10 (.30)
2nd Highest at risk JSA	1.62	1.55	-.07 (.13)
2nd Lowest at risk JSA	1.60	1.48	-.11 (.08)
Lowest at risk JSA	1.24	1.14	-.10 (.12)
Highest - Lowest	1.21 (.29)	1.41 (.344)	Difference-in-difference = **.20 (.08)** [Percent of pre-policy mean = 11.4%]

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) and JSA Introduction

	Reduced Form OLS Model		Structural IV Model			
A. Property Crime	Change in Log(Property Crime Rate), OLS		Change in Log(Outflow Rate), OLS		Change in Log(Property Crime Rate), IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion of At Risk Claimants in Pre-JSA Quarter	2.99 (.72)	3.55 (1.15)	1.84 (.86)	1.63 (.98)		
Change in Log(Outflow Rate)					1.62 (.66)	2.17 (1.11)
Area Controls	No	Yes	No	Yes	No	Yes
R-Squared	.23	.35	.08	.21		
B. Violent Crime	Change in Log(Violent Crime Rate), OLS		Change in Log(Outflow Rate), OLS		Change in Log(Violent Crime Rate), IV	
	(7)	(8)	(9)	(10)	(11)	(12)
Proportion of At Risk Claimants in Pre-JSA Quarter	2.71 (1.20)	3.70 (1.25)	1.84 (.86)	1.63 (.98)		
Change in Log(Outflow Rate)					1.47 (.74)	2.26 (1.23)
Area Controls	No	Yes	No	Yes	No	Yes
R-Squared	.15	.26	.08	.21		

Notes: Coefficients (heteroskedastic consistent standard errors) reported. The sample size in all regressions is 45 areas. All regressions weighted by area population. The controls entered (from the Labour Force Survey matched to police force areas) were changes in the proportions of young people, ethnic minorities, 16 to 19 year olds in full time education, part-time employees, unskilled workers and workers in personnel and security services and the change in the lowest quartile wage. Note that specifications (3) and (9) and specifications (4) and (10) are respectively the same, first stage regressions.

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Table 3:
Changes in Log(Crime Rates) and JSA Introduction Using Outflow Destinations

	Structural IV Model			
A. Property Crime	Difference in Outflow Rate Destination, OLS		Change in Log(Property Crime Rate), IV	
	(1)	(2)	(3)	(4)
Proportion of At Risk Claimants in the Pre-JSA Quarter	1.76 (.24)	1.82 (.29)		
Difference in Outflow Rate Destination			1.70 (.35)	1.95 (.58)
Area Controls	No	Yes	No	Yes
R-Squared	.58	.66		
B. Violent Crime Models	Difference in Outflow Rate Destination, OLS		Change in Log(Violent Crime Rate), IV	
	(5)	(6)	(7)	(8)
Proportion of At Risk Claimants in the Pre-JSA Quarter	1.76 (.24)	1.82 (.29)		
Difference in Outflow Rate Destinations			1.82 (.29)	2.03 (.54)
Area Controls	No	Yes	No	Yes
R-Squared	.58	.66		

Notes: Coefficients (heteroskedastic consistent standard errors) reported. The sample size in all regressions is 45 areas. All regressions weighted by area population. The controls entered (from the Labour Force Survey matched to police force areas) were changes in the proportions of young people, ethnic minorities, 16 to 19 year olds in full time education, part-time employees, unskilled workers and workers in personnel and security services and the change in the lowest quartile wage. Note that specifications (1) and (5) and specifications (2) and (6) are respectively the same, first stage regressions.

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

	Basic Specification	Basic Specification + Controls	Basic Specification + Controls + Area Trends
A. Property Crime Models			
	(1)	(2)	(3)
Proportion of At Risk claimants in Quarters Surrounding JSA Introduction, OLS	2.98 (.78)	3.11 (.86)	2.99 (.85)
R-Squared	.19	.24	.30
B. Violent Crime Models			
	(4)	(5)	(6)
Proportion of At Risk claimants in Quarters Surrounding JSA Introduction, OLS	2.59 (1.29)	2.64 (1.28)	2.52 (1.04)
R-Squared	.47	.49	.57

Notes: Coefficients (heteroskedastic consistent Newey-West standard errors corrected for serial correlation) reported. The sample size is 225 (45 areas over 5 quarters). All regressions weighted by population. All specifications include quarter dummies. The controls entered (from the Labour Force Survey matched to police force areas) were changes in the proportions of young people, ethnic minorities, 16 to 19 year olds in full time education, part-time employees, unskilled workers and workers in personnel and security services and the change in lowest quartile wage.

Table 5:
Long Run Effect of JSA Sanctions
(Twenty Five Quarters Following Introduction)

	Crime and Benefit Sanctions			
	Basic Specification	Basic Specification + Controls	Time Varying Sanction Effects	Time Varying Sanction Effects + Controls
A. Property Crime				
	(1)	(2)	(3)	(4)
Proportion Sanctioned	.93 (.46)	1.09 (.45)		
Proportion Sanctioned, 1996Q4-1997Q4			4.41 (1.33)	3.77 (1.27)
Proportion Sanctioned, 1998Q1-1998Q4			1.85 (1.11)	1.97 (1.04)
Proportion Sanctioned, 1999Q1-1999Q4			-.69 (1.13)	-.71 (1.17)
Proportion Sanctioned, 2000Q1-2000Q4			1.00 (.94)	1.11 (.98)
Proportion Sanctioned, 2001Q1-2001Q4			.70 (.74)	.83 (.70)
Proportion Sanctioned, 2002Q1-2002Q4			.64 (1.08)	1.12 (1.05)
R-Squared	.95	.95	.95	.95
B. Violent Crime				
	(5)	(6)	(7)	(8)
Proportion Sanctioned	1.04 (.80)	1.58 (.82)		
Proportion Sanctioned, 1996Q4-1997Q4			4.16 (3.17)	3.64 (2.79)
Proportion Sanctioned, 1998Q1-1998Q4			1.07 (2.48)	.81 (1.43)
Proportion Sanctioned, 1999Q1-1999Q4			-1.61 (1.74)	-1.02 (1.71)
Proportion Sanctioned, 2000Q1-2000Q4			.21 (1.82)	.69 (1.77)
Proportion Sanctioned, 2001Q1-2001Q4			.81 (1.22)	1.49 (1.25)
Proportion Sanctioned, 2002Q1-2002Q4			2.14 (1.97)	3.14 (1.92)
R-Squared	.94	.94	.94	.94

Notes: Coefficients (heteroskedastic consistent Newey-West standard errors corrected for serial correlation) reported. The sample size is 1025 (41 areas over 25 quarters). All regressions weighted by population. All specifications include area fixed effects and quarter dummies. The controls entered (from the Labour Force Survey matched to police force areas) were changes in the proportions of young people, ethnic minorities, 16 to 19 year olds in full time education, part-time employees, unskilled workers and workers in personnel and security services.

Table 6:
Cost Benefit Calculations
for Quarter Following JSA Introduction (in million pounds, £M)

	Outflow Assumes All Unknown Destinations into Work (38,337 Outflows)	Outflow Assumes All Unknown Destinations into 'Nowhere' (29,213 Outflows)
A. Excluding the Crime Impact		
Extra outflow and average wage		
Number of Outflows [a] * £2,398 [b]	£91.9M	£70.1M
Administrative cost		
£500,205 [c] + 201,267 *£48 [d] + £22.8M [e]	£32.9M	£32.9M
Benefit savings		
£15M [f] + 38,337*£650 [g]	£39.9M	£39.9M
Net Social Benefit (Not including crime)	£98.9M	£77.1M
B. Including the Crime Impact		
Cost from additional property crime		
7,484 crimes * £765.80 [h]	£5.7M	£5.7M
Cost from additional violent crime		
854 crimes * £19,000 [h]	£16.2M	£16.2M
Total Additional Cost from Crime	£21.9M	£21.9M
Net Social Benefit (Including crime)	£77M	£55.2M

Notes:
a - Estimated effect of JSA on the outflow rate of 2.8 points (from 11.9% to 14.7%), a 23.5% increase. Out of a stock of 1,369,167 this represents 38,337 additional outflows. Unknown destinations represent 23.8 percent of total outflow. If all go to 'nowhere' then input from jobs on 38,377*.762 = 29,213 extra outflow only; b - Labour Force Survey hourly wage of individuals unemployed in previous quarter (3 and 4 of 1996): £ 5.27. This gives a quarterly wage of £2,398 per quarter; c – advertising costs; d – unit cost of unemployed placing up from £186 in 95/96 to £234 in 96/97 or an extra £48 applied to all outflow; e – costs of erroneous benefit payments due to computer system problems ; f – diminution in benefits from reduction to 6 months (from 12); g – average weekly benefit payment of £50 giving £650 saving per quarter for each extra outflow; h – average costs of crime computed from Brand and Price (2000).

Appendix

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).

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 highly reassuring at 0.999, and is 0.997 for violent crime. In terms of changes of in log(crime rates), the measure used as the dependent variable in our regressions, the correlations are 0.999 for property and 0.988 for violent crime.

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