

Sanctions within conditional benefit systems

A review of evidence

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This study reviews and consolidates international evidence on sanctions operating within welfare systems where benefits are conditional upon claimants' behaviour.

Recent reform of (workless) benefit systems in Britain and other post-industrial nations has sought to promote employment through work-related conditions backed by financial sanctions; the rationale being that sanctions will promote desired behaviour, and therefore favourable outcomes. This review examines an international body of evidence on sanctions, their impacts, outcomes and context, and explores how far the theoretical and political 'assumptions' that underlie them are met in practice.

The report:

- considers the different ways in which benefit sanctions may (theoretically) affect claimant behaviour;
- examines evaluative evidence of sanction impacts within unemployment benefit and US welfare systems;
- contextualises impact evidence within literature on the operation and administration of sanctions as well as the results of surveys and qualitative studies conducted with claimants;
- explores the effect of sanctions and conditionality on other (non-employment-related) behaviours;
- discusses how this evidence relates to the political justifications voiced in support of benefit sanctions.

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Introduction

This study reviews and synthesises international evidence on sanctions operating as part of conditional welfare systems. It considers evaluative evidence of sanction impacts, the wider literature on operational context and the qualitative experience of sanctioned claimants. It then discusses how this evidence relates to the political justifications voiced in support of benefit sanctions.

Background

The rolling programme of welfare reform under Gordon Brown's Labour Government sought to change the fundamental assumptions of many social security programmes to promote employment. This approach of increased sanctioned-backed conditionality has grown incrementally over time but has also broadened to include 'new' claimant groups. The move to conditionality has also extended purely work-related issues into a policy model that sees conditionality and sanctions as tools to change other behaviours, for example housing benefit sanctions as a means of preventing anti-social behaviour.

The extension of benefit conditionality and use of sanctions has not been confined to the UK, but is in evidence throughout Europe, North America and Australasia, with international and European bodies, such as the Organisation for Economic Co-operation and Development (OECD) and the European Union (EU), overtly promoting the use of (sanction-backed) activation policies (see, for example, OECD, 1996).

The theoretical approach to sanction effects

When considering the impact of benefit sanctions it is important to be aware of the different types of effect sanctions have (or might have) on claimants. There are four key dimensions to these differences:

- 1 The type of sanction: whether the sanction results from an administrative failure (e.g. not completing required paperwork/ failure to attend a meeting), or from a behavioural 'misdemeanour' (e.g. not 'actively looking' for work, or refusing employment). In practice this distinction may not always be clear-cut.
- 2 The type of effect: whether a sanction impact arises directly from lower levels of benefit entitlement (i.e. welfare use/caseloads and spending) (entitlement effects), or from the changed behaviour of claimants (treatment effects).
- 3 The timing of the impact: whether the effect is created by the threat of a sanction, or the sanction itself. There are three points at which sanctions can have an impact: 1) before the claim: the presence of sanction-backed conditions and the prospect of (potentially) being sanctioned in the future may deter potential claimants from entering the system (take-up effects), 2) during the claim, but before the sanction: either the result of the general threat of sanctions or an actual warning (threat effects) and 3) during the claim, following a sanction: a behavioural change or reaction to being sanctioned (imposition effects).
- 4 The type of outcome: whether the impact is short-lived and limited, or is sustained over the longer term. Sanctions have the potential to impact on claimants and their families in many ways. In the short term they may promote compliance or participation (intermediate outcomes), or encourage claimants to end their claim (thus affecting caseloads and spending), possibly to enter employment. In the longer term they may affect child welfare, earnings and material hardship.

Impacts: sanctions linked to employment-related conditionality

Despite the wide range of potential effects identified in theory, empirical studies exploring the effectiveness of sanctions focus almost exclusively on the post-claim impacts of imposed sanctions, with a small number also looking at the impact of direct warnings. Effects on take-up and on the presence of sanctions on the behaviour of the general claimant population are not considered. This limits the messages we are able to draw from impact studies, as well as their potential application to the current policy context.

Consolidating findings of the unemployment benefit and welfare evaluations identified, we can conclude that sanctions for employment-related conditions (full-family sanctions in the case of US welfare systems) strongly reduce benefit use and raise exits from benefits, but have generally unfavourable effects on longer-term outcomes (earnings over time, child welfare, job quality) and spill-over effects (i.e. crime rates). However, beyond this the evidence is harder to reconcile. For example, while unemployment benefit programmes tend to demonstrate positive impacts on employment, this is not the case for welfare studies.

To summarise:

- There is compelling and consistent evidence of the short-term effects of sanctions in unemployment benefit (UB) systems – raising unemployment benefit exits and job entry.
- Evidence of sanction effects in the US welfare system is more mixed. While more severe sanctions (in particular, immediate full-family sanctions) significantly reduce welfare caseloads, evidence on employment and earnings is far less conclusive or favourable (two studies indicating a negative impact, the third a positive one).
- Recent evidence from a single study of unemployment benefit sanctions (Arni *et al.*, 2009) suggests earlier UB exits prompted by sanctions result in poorer quality employment (lower earnings and job instability).

- The one study exploring spill-over effects found that sanctions had unfavourable impacts on local crime rates (Machin and Marie, 2004).
- An exploration of earning expectations found no effects from imposed sanctions (a finding that runs contrary to job search theory) (Schneider, 2008).

Despite this evidence there are still many unknowns. This creates a need for more evidence on the duration of effects, differences by claimant characteristics (e.g. gender) and inflow and take-up effects, as well as on a range of exit destinations and longer-term impacts, such as income, post-unemployment work sustainability and earnings.

Contextual studies of employment-related sanctions

The nature of the impact studies identified means that there are potential effects that are poorly covered by high-quality evidence. Thus the wider contextual literature that surrounds the 'core' impact and outcome evaluations is very useful both in its own right and in interpretation and generalisation of findings on sanction effects, particularly those studies offering direct insights into claimant experiences. Also of importance are those factors that affect the way that sanctions operate (for example, claimant knowledge of the system and bias in administration), factors which play a role in how efficient, effective or equitable they are.

Claimant knowledge and understanding: studies conducted with claimants demonstrate low levels of awareness of sanctions; while people knew penalties were part of the system they rarely knew when they could be imposed or how they could be reversed. This is significant because claimants who do not know what is expected of them and what will happen if they fail to meet these expectations are effectively being punished for a lack of understanding rather than (deliberate) non-compliance.

Administrative capacity and consistency: evidence suggests that administration of

sanctions is not rational and equitable. Studies conducted in the US have identified racial bias in the imposition of sanctions (Schram *et al.*, 2008; Schram *et al.*, 2009); there is also evidence from both Europe and the US of considerable geographical variation in sanction likelihood.

Characteristics of sanctioned claimants: a large body of research exploring the characteristics of sanctioned claimants demonstrates that the most vulnerable are the most disadvantaged. Claimants with human capital deficits (i.e. lack of work experience and qualifications), and/or facing practical barriers to work (e.g. not having access to a car) are more likely to be sanctioned. Exploration of demographic differences shows that young claimants, those with large families and those belonging to black and minority ethnic (BME) groups are at heightened risk.

Claimant motivation and behaviour: qualitative research with claimants offers little indication of deliberate non-attendance or non-engagement with services or in programmes; failure to attend or participate was more often a product of poor information and non-intentional behaviour such as forgetfulness. Studies also suggest that although claimants may be encouraged to attend meetings and participate in activities in order to avoid sanctions, they do little to change motivation or claimants' attitudes to work.

Such evidence raises important questions about the ability of sanctions to operate as intended, preventing or punishing deliberate non-compliance, and about whether they may merely compound existing inequalities and create further barriers to work for some claimants.

Other forms of conditionality

The use of conditionality and sanctions has not been confined to unemployment and social assistance benefits. Some important developments have taken place in which attempts have been made to make other kinds of benefit payments conditional on certain behaviours (for example, the Sure Start Maternity Grant and Conditional Cash Transfers).

Although covering a wide range of programmes in different policy areas (health, education, child

support, substance misuse) with very different objectives, the overriding message within each strand of policy is the inconsistency of results. For example, the US programmes PIP and PPI, which although closely matched in terms of content and purpose (both sanction parents who are unable to show that their child has been immunised), performed very differently. While the evaluation of PIP showed exceptionally favourable results on vaccination rates, PPI appeared to have very little, if any, impact. Similarly, evaluations of family cap policies, which sanction those having babies while claiming welfare benefits, show very mixed results against their objective of reducing birth rates.

Justifications

The majority of commentary on benefit sanctions and conditionality has focused on underlying issues of rights and responsibilities largely based on moral philosophy arguments. Critics have questioned the viability and fairness of such approaches, often from a social rights perspective. This review comes from a different perspective, a rigorous analysis of the evidence base on the outcomes of sanctions in practice, based on a clear set of theoretical a priori assumptions on the nature and basis of such evidence. To extend the review into the realms of moral philosophy would be difficult and inappropriate, and therefore we consider only those justifications of sanctions that can be informed by evidence. This means we focus on whether measurable aims of policies couched in terms such as 'equality, effectiveness and efficiency' have supporting evidence to underlie them.

Equality-based justifications: while sanction-backed conditionality ensures that claimants cannot 'opt out' of programmes designed to benefit them there are clear (yet under-researched) effects on benefit take-up. This means that although all claimants are subject to the same work-related activities and have access to the same services within mandatory programmes, those who are most disadvantaged may be (disproportionately) deterred from entering the programme or inclined to leave into inactivity or informal work. These same claimants are also those more likely to be sanctioned than others (indicating inequality in the imposition of sanctions). Equal access

to programmes and services does not mean equal quality in those provisions, neither does it necessarily lead to equality in outcomes. Indeed, evidence suggests that sanctioned claimants are less likely to enter sustainable employment or to make longer-term gains in income.

Efficiency-based justifications: it is argued that sanction-backed conditionality is efficient; that these approaches are best able to use available resources to maximise positive outcomes by ensuring claimants are better informed and realistic about opportunities (managing, in most cases lowering, expectations and reservation wages), that job search is 'optimised' and 'deliberate' job loss minimised. While sanctions may be efficient in terms of shortening unemployment spells, consideration of longer-term sanction impacts (see, for example, Arni *et al.*, 2009), in particular the negative effects of sanctions on job and earnings progression, demonstrates the problems of such efficiency arguments. Furthermore, while cutting caseloads is an efficient way of reducing expenditure, other factors, such as spill-over effects on crime rates, along with higher spending on in-work benefits, offset savings.

Effectiveness-based justifications (optimal models for changing behaviour): sanctions are designed to promote or prevent particular behaviours or actions, encouraging compliance with, or participation in, activities or programmes deemed to be in the best interests of claimants. This means that in order to operate effectively claimants must understand the behavioural conditions of entitlement and the penalties for breaching them. Crucially, however, qualitative evidence suggests the majority of claimants have only a limited understanding of the sanctions system. Additionally, descriptive statistics show that only a minority of sanctions imposed reflect a clear behavioural aspect (they are more frequently retrospective moral hazard sanctions, those imposed for leaving employment voluntarily or for misconduct), and are therefore unable to prevent 'undesirable' behaviour. There are also wider questions around the effectiveness of sanctions compared with rewards (sticks versus carrots). The psychological literature indicates that rewards may

produce better longer-term behavioural outcomes than negative punishments.

Conclusions

This report finds a gulf between the rhetoric and evidence on benefit sanctions. The evidence base is both small and limited in its coverage, and is hard to fit across the differing approaches to preventing poverty and promoting opportunity that arise in international policy design. The evidence from sanctions on US lone parents in receipt of social assistance is a small and inconclusive part of a very large evidence base. The quality and coverage of such evidence is mostly about aggregate falls in caseloads and gains to employment, with no specific attention to the independent causal effects of sanctions. Evidence from European unemployment benefits has more precise estimates of the effects of sanctions and shows both the risks of lower wages and higher job churning alongside the gains from reduced durations of unemployment.

The UK has committed itself to reducing and ultimately eliminating child poverty with a primary focus on parental employment to achieve this. In a policy approach that is committed to a high quality evidence base, future policy on 'welfare reform' that relates to sanctions needs to take a more rounded approach to the assessment and use of evidence. The rhetorical approach that sees no fundamental problem in ratcheting up conditionality and sanctions is too narrowly based on principles of moral philosophy, and takes too selective and ambivalent an attitude to the evidence.

The review leads us to make a number of recommendations:

- 1 To replicate the Arni *et al.* (2009) study in the UK – to test the effect of sanctions on earnings and sustainability of work;
- 2 To put in place better, more wide-ranging cost-benefit studies of conditionality and sanctions that look at displacement and spill-over effects;

- 3 To look more closely at the (potential) spill-over effects of conditionality and sanctions on the grey economy and informal work;
- 4 To ensure longitudinal datasets can capture the changes to conditionality that have already been put in place, for example by ensuring that the next wave of the Millennium Birth Cohort Survey has a module of questions to assess the effects of the new benefit conditions on lone parents according to their youngest child's age.

1 Background

Since the late 1980s British ‘welfare reform’ has brought significant change to programmes intended to promote employment for working-age people, highlighting the potential gains of increasing obligations to both change the culture of benefit systems and ensure behavioural compliance. Since 1997 more general employment conditionality spread from the unemployed across to other groups, primarily lone parents and people with incapacities for work. Such conditional entitlement has increasingly focused attention on the issue of sanctions: the removal of entitlement where behavioural obligations are not met. Arguments for this ‘ratcheting up’ extend on a continuum which at the most lenient end suggests perfectly informed claimants will act appropriately and thus changes will not result in heavy sanctioning. At the other end of the spectrum are the advocates of strict and frequent sanctioning as a means to tackle fraud and fecklessness and to improve incentives to leave benefits and enter work.

The extension of benefit conditionality and the use of sanctions has not been confined to the UK, but is in evidence throughout Europe, North America and Australasia, with international and European bodies, such as the OECD and the EU, overtly promoting the use of (sanction-backed) activation policies (see, for example, OECD, 1996). Different approaches to sanctions within welfare systems reflect the balance between the use of sanctions as a deterrent (in which their use is minimal) and as a punishment and a means of controlling claimant numbers (Holcombe *et al.*, 1998). Conditional Cash Transfers (CCTs) have also been introduced in developing countries, largely tied to conditions linked to child development: immunisation, health checks and school enrolment/attendance; this model is now being piloted in New York City (see Miller *et al.*, 2009). In the UK conditionality has also spread to areas other than employment, for example the Sure Start Maternity Grant and the Health in Pregnancy Grant – which are conditional upon receiving health advice from a doctor or midwife.

A large body of literature provides a lot of discussion and commentary on the overall policy approach and its justifications. The evaluation of outcomes is a much smaller literature (see Griggs and Bennett, 2009 and SSAC, 2006), especially where it attempts to distinguish the particular outcomes of benefit sanctions. This report aims to fill the gap and to focus specifically on the evidence from the design and use of benefit sanctions themselves, rather than the wider conditional approaches, and to draw together high quality impact studies.

This is contentious territory with a high political profile and both the pro- and anti-sanction advocates are often too certain that they are ‘right’. It is not our role here to engage with the ideological content of the debate or its place in overarching political strategy. Instead, our role is to take a rigorous approach to evidence. But being rigorous is not just a blinkered consideration of quantifiable ‘impacts’ of benefit sanctions, but also to have a clear understanding of the context in which the evidence arises. Interpretation is key, and interpretation without context carries the danger of ending up in an empirical cul de sac with little ability to generalise to other situations.

Any international body of evidence is both varied and skewed. Looking at benefit sanctions across countries, and across programmes, means standing over the differences in underlying policy contexts, in the particular aims and objectives of programmes, and in the rationales for sanctions. For instance, programme reforms that prioritise reducing expenditure over improving poverty outcomes often measure caseload and employment changes alone, while those prioritising poverty often have a wider set of outcome measures. In countries where policy objectives are more closely connected to taxpayer buy-in and public support irrespective of empirical evidence, one could argue that even the best specified impact results are of little, or at least secondary, importance to the political impetus for reform.

Current UK reform is subject to some uncertainties around the prospect of a new Government. However, the Welfare Reform Act 2009 and its preceding green and white papers, together with the Gregg review (2008) and accompanying discussion paper (*Realising Potential*), have all occurred in the absence of a high quality, publically available review of the evidence of sanction effectiveness.

The combination of policy reform and deficit reduction will lead to public spending constraints and a search for new gains in efficiencies and effectiveness. This potentially raises the stakes on benefit sanctions further, as they could be seen as a 'win-win' of lowering spending and increasing obligations, unless there is a fuller and wider appreciation of costs and consequences.

This report assesses the underling evidence base for the effects of sanctions, employing a systematic review of (pure) impacts, and contextualising this within qualitative and other evidence on the operation of sanctions and the experiences of sanctioned claimants. It culminates with a discussion of how this evidence relates to the equality, efficiency and effectiveness justifications voiced in support of benefit sanctions.

The report is broadly structured as follows:

- overall theory and approach for the review;
- the impact of sanctions linked to employment-related conditionality;
- context and interpretation of employment-related sanction effects;
- evidence of sanctions linked to other types of conditionality;
- justifications for sanction-backed conditionality;
- conclusions and recommendations.

2 The review's theory and methods

The aims of this report are:

- To produce a systematic review of international evidence on the impact of benefit sanctions on clients; and
- To critically explore the justifications behind them.

Our approach brings together a traditional social policy literature review with a Cochrane-style systematic review. We thus cover a wider range of evidence type and quality than is typical for a systematic review; this allows us to explore justifications and to look at qualitative evidence on outcomes as well as more discursive overviews of reform. All literature is reviewed in a systematic manner (i.e. with transparent search terms, search on relevant databases, and quality criteria for selecting publications for inclusion in the review – see Appendix 1).

Systematic reviews require limits to be placed on the scope of the study and the material under investigation. In this case the search was restricted to studies identifying the specific and distinct effects of sanctions within a conditional benefit system, published after 1985 (to correspond with the year the Restart scheme was introduced in the UK), available in English and which included a comprehensive and examinable methodology. Experimental, quasi-experimental and econometric observational studies were prioritised as true 'impact' studies. Contextual evidence, including complementary qualitative research, was also identified but was managed separately.

Theoretical approach to sanctions and their effects

Identifying sanction impacts requires a clear understanding of what sanctions are, how they

(are intended to) operate and of all potential causal effects within a conditional benefit system. All programmes have conditions of eligibility – age, residence, nationality, a contribution record – but failing to meet these eligibility conditions is rarely considered a sanction, rather as a 'disqualification' from entitlement. For instance, when a child reaches the age of 19 they are not entitled to child benefit.

Sanctions are intended to counter work disincentives created by benefits by ensuring claimants comply with the behavioural conditions of entitlement – individuals being 'required to regularly undertake some pre-specified action' (Bastagli, 2008, p. 127), reflecting what people do, or don't do, rather than who they are. Sanctions are essentially the enforcement of these conditions during a period of claim through the application of (financial) 'penalties'. As Besharov and Germanis argue:

Sanctions are essential to enforcing mandatory participation, because participation is not truly mandatory unless there is a consequence for not participating. (2004, p. 99)

The effects of sanctions in theory

Thinking about the different potential effects of sanctions led to the construction of a theoretical framework (Figure 1). This framework summarises our approach to the literature and has been used to assess the range and quality of evidence from the evaluations in the review. It also acts as a starting point for our discussion of effects of sanctions found in the theoretical literature.

There are four key dimensions within this framework: type of sanction, type of effect, timing of effect and type of outcome. The first distinction, 'type of sanction', while not an effect, is significant in that different types of sanction

have the potential to impact in different ways – administrative sanctions being more closely linked to entitlement effects.

Types of sanction

These different ‘categories’ of sanction are imposed for different types of non-compliance. It is wise to distinguish in theory between two such types, recognising that in practice it may be difficult to make such a clear distinction (in some cases administrative conditions being a proxy for wider behaviour):

- Administrative sanctions: primarily relate to issues of eligibility and directly to the claim such as responding to letters, attending interviews, giving information and other behaviour that is primarily to do with the process of the claim and continued entitlement.
- Behavioural sanctions: reflect failure to behave in accordance with ongoing (behavioural) conditions of receipt. In unemployment-related benefits these are primarily related to job search, e.g. actively looking for work, remaining available for employment and taking up offers of employment. In the widening ambit of conditionality (e.g. CCTs) such behaviour may be enrolment and attendance of children at school, taking up immunisation and being a ‘good’ tenant.

Where descriptive data from unemployment benefit programmes distinguishes sanctions by cause it frequently shows that administrative sanctions constitute a large proportion of those applied. For instance, Australian data demonstrates that, in 2008, 54 per cent were for a ‘failure to attend interviews’ with providers, against only 4 per cent for a refusal to take up an offer of employment.¹

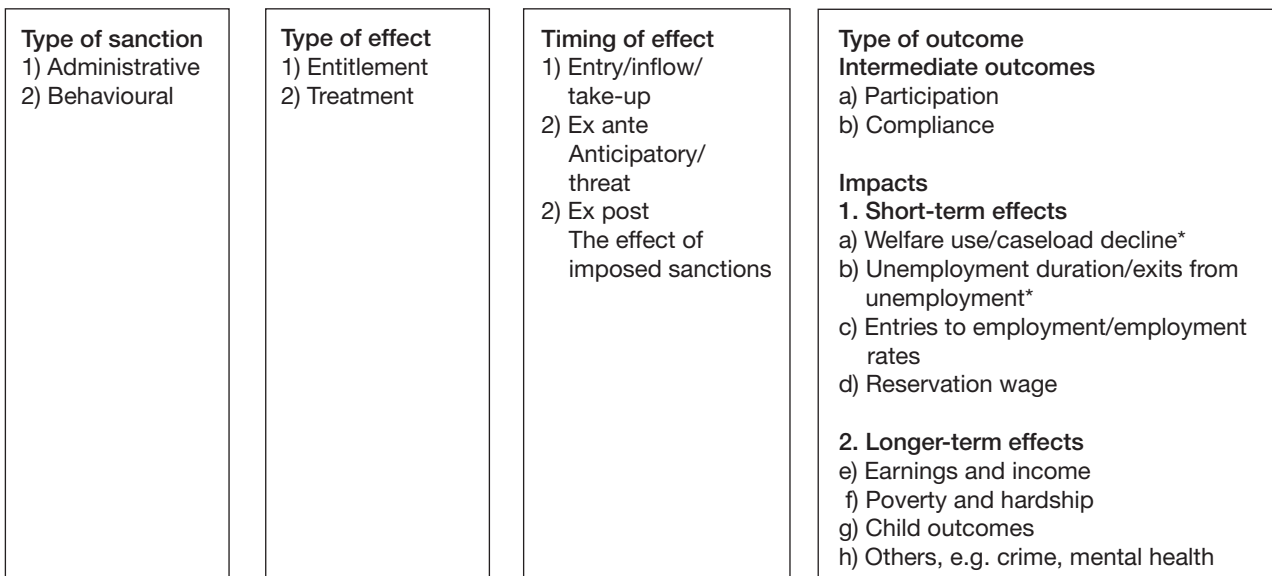
Type of effect

In this report we distinguish two (theoretically) distinct types of sanction effect:

- Entitlement effects: impacts that arise directly from lower entitlement to benefits (i.e. welfare use and spending). For example, if fewer people claim benefit then caseloads fall, resulting in savings to the benefit budget; and
- Treatment effects: impacts that arise from the changed behaviour or the changed circumstances of claimants caused by sanctions (e.g. employment rates, poverty and spill-over effects).

Increasingly, benefits are being made conditional to achieve a treatment effect (such as increasing employment) although in reality this aim may be coupled with entitlement objectives, such as reduced spending. Making participation in

Figure 1. Theoretical framework for identifying sanction effects



* Possible instances where pure entitlement effects may occur.

treatments mandatory is a way of ensuring that those with worse characteristics do not exclude themselves as they can (and disproportionately do) in voluntary schemes. But increasing conditionality also raises the likelihood of producing entitlement effects brought about by the mechanics of the system, for example US welfare claimants losing entitlement after receiving a full-family sanction and being removed from the rolls.

Emphasis on this type of short-term effect in evaluations reflects different policy objectives and priorities from those looking at treatment effects (beyond the simple calculation of who is on or off benefits, in or out of work, to wider and longer-term impacts). As a consequence, we find different emphasis placed on these two types of effect in the studies included in this review (some focusing entirely on entitlement effects, others exclusively on treatment). However, in reality, there is always a 'mixture' of entitlement and treatment effects and distinctions are rarely explicitly made in evaluative terms.²

Timing of effect

There are also important distinctions to be drawn on the basis of timing: first, the prospect of sanctions may impact on eligible individuals' decisions about making a claim (inflow effects); second, the prospect or threat of sanctions may influence the behaviour of claimants in the system (threat effects); finally, enforcement may affect the sanctioned claimant's behaviour (perhaps prompting compliance, perhaps an exit from the system). Figure 2 shows our approach to distinguishing effects by their timing in the benefit claim process.

Inflow: take-up effects

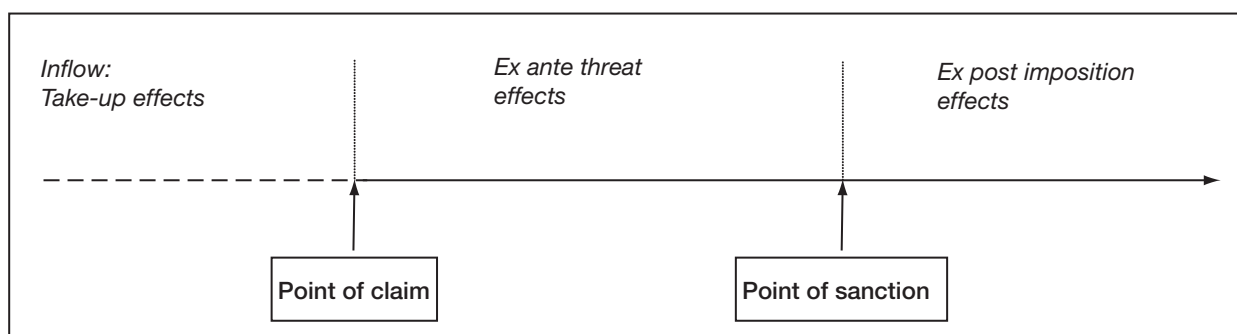
Take-up effects occur because increasing conditionality and sanctions increase the 'compliance costs' of the programme (Bennett *et al.*, 2009).³ These are well known and empirically proven to affect take-up through 'hassle' and stigma (see Currie, 2004).

Take-up effects can be either intended or unintended. In a number of US states high levels of up-front conditionality are part of an intended 'diversion' approach that includes tough requirements (such as pre-claim work conditions [Moffitt, 2003]) that put many hurdles in the way of entering the benefit programme (Meyers *et al.*, 2006).⁴ Wu *et al.* offer the following example to illustrate this point:

If a woman is told at an initial visit with a worker that she must participate in work-related activities before she can receive any benefits, she would not typically be counted as sanctioned if, failing to follow work requirements, she never enters the system. But in a state that requires work activities to begin only after 2 months of receiving benefits, the same person might receive benefits for 2 months and then would typically count as being sanctioned if she did not follow work requirements. (2004, p. 5)

Unintended (or, perhaps, less intended) effects on take-up result from the combination of compliance costs and the value of entitlement. This means that an exactly similar set of (tough) conditions for the receipt of unemployment benefits will have potentially higher impacts on take-up in countries or localities with a low flat-

Figure 2. Timing effects of sanctions



rate benefit than they would in countries where benefits are higher and earnings-related.

Ex ante threat effects

There are actually two potential direct effects of sanctions: 1) from anticipation of sanctions (whether general effects, resulting from the presence of sanctions in the system, or specific to those receiving warnings) and 2) from the imposition of sanctions. *Ex ante* effects (referred to as threat effects in this report) are the former and relate to impacts arising from the pressure placed on claimants to comply in order to avoid a sanction, whatever its precise nature. Such effects are often high in policy-makers' minds because they 'help influence the decisions benefit recipients make' (Kauff *et al.*, 2007, p. 2) and are seen as key to changing the culture of benefit systems. This is particularly true of general threat effects – the perception being that the mere existence of sanctions will change behaviour, without the need to impose more than are necessary to demonstrate the validity of the threat (i.e. the threat is a greater agent for changing behaviour than actual sanctions themselves). Claims for the efficacy of this approach extend to motivating and compelling recipients to move towards self-sufficiency (Bryner and Martin, 2005) while holding them 'accountable for their actions' (Holcombe *et al.*, 1998). Implementation of sanction policies reflects how the state perceives and attempts to balance the threat effect versus the punitive effect of sanctions (Holcombe *et al.*, 1998).

However, threat effects are often very difficult to identify and measure (in particular, separating general threat effects from those arising from the raised conditions they often accompany) and are often assumed to be a costless additional outcome from increased conditionality. Anticipation of a sanction will be higher in systems with high levels of conditionality and a high risk of sanctions than in less conditional systems with a lower sanction risk. In general there will be a raised likelihood of 'giving up' entitlement while on benefit in the face of such higher compliance costs in addition to the more direct effects from specific anticipation and warnings about sanctions.

Ex post imposition effects

Imposition effects are those directly brought about by a sanction. There are four primary behavioural responses to a sanction:

- (a) comply and remain on the programme;
- (b) not comply and remain on the programme (on reduced support);
- (c) exit the programme (with or without complying);
- (d) dispute and appeal against the sanction.

It is easiest to deal with d) first, the underlying administrative legal and adjudication aspects of conditionality and sanctions, because this does not feature much in the evaluation literature. Many regimes effectively do not allow formal rights of review (although this is not the case in the UK) and this, in part, reflects the position that a large proportion of sanctions are administrative ones based on simple failure to do something rather than on a more complex assessment of behaviour.

Outcome c), exiting the programme, is one that is easiest to identify from administrative data, although the destination of exit is sometimes unclear.

Distinguishing between a) and b), remaining on the programme and complying, and remaining on the programme and not complying (taking the hit of a sanction), depends upon how claimants respond to changes in their benefit income, and on the level of the reduction. There is also the potential to 'comply at the minimum level' – to do the least possible to remain on the programme and avoid a sanction but without optimal behavioural compliance.

Quality of evidence

Analytical rigour requires a clear approach to assessing evidence type and quality. We use common assumptions about differences between impacts and outcomes to ensure that the former refers to the results of programmes that are identified against a measured control (what would have happened in the absence of the programme), using either an experimental design or econometric

modelling techniques that enable evaluators to isolate and measure net effects of the intervention.

The length of time the impact is observed (the length of the evaluation period) is an important consideration. We know from long-term experimental studies of welfare reform in the USA that differences between the treatment and control groups can decline over time, so that five-year impacts are often much smaller than one-year and two-year impacts (see Grogger and Karoly, 2005).

The standard set of 'core' evaluation measures (e.g. caseload decline and employment rates) often represents an evaluative 'black box' that allows only partial understanding of how programmes work and their effects on participants. The wider contextual literature that surrounds the 'core' impact and outcome evaluations is thus very useful in interpretation and generalisation, particularly those studies offering direct insights into claimant experiences (this is presented in Chapter 4).

Policy context

It is often difficult to identify specific sanction impacts due to the complexity of different national benefit systems – programmes often being part of a 'patchwork' of provision and treatments. Conditional Cash Transfers in developing countries avoid this problem because, usually, there are no other programmes available to the target group. But for the majority of evidence from industrialised OECD countries, understanding where conditionality and sanctions in one programme fit alongside other 'sister' programmes is essential. Exits from the conditional programme may not mean exits from *all* benefit programmes. In Europe, unemployment insurance schemes tend to be underpinned by means-tested social assistance schemes and exits from the first into the latter would not constitute an exit. In the US, however, few programmes beyond Supplemental Nutrition Assistance (SNAPs⁵) exist for non-insured unemployed people. In the UK, the categorical means-tested safety net has for a long time meant that those exiting from one category sometimes reappear in another.

Income 'packaging' comprised of a number of different benefits is also common. In the UK for instance, Income Support (IS) and

Jobseekers Allowance (JSA) are only for adults and a separate Child Tax Credit pays money for co-resident children. Thus in the UK a full benefit sanction for a lone parent is equivalent to a partial sanction in the US Temporary Assistance for Needy Families (TANF) system.

Selection effects, characteristics and interpretation

Understanding sanction outcomes must also tackle 'selection effects'. In simple descriptive profiles 'the sanctioned' will be observed to have poor outcomes, but this is partly because of their underlying characteristics. Those most at risk of being sanctioned may not match the assumptions evident in the underlying principles and justifications. For example, the 'feckless' and the 'dependent' may be the targets of sanctions, but in practice it may be those with language and literacy difficulties and multiple disadvantages that are most affected.

It is important to know whether the characteristics of families affected by sanctions differ notably from those of the non-sanctioned, and whether these characteristics make compliance with benefit/welfare conditions more difficult (Pavetti *et al.*, 2003). Only by developing an understanding of the 'determinants' can we fully contextualise the evidence on impacts and effectiveness (Lee *et al.*, 2004). This is discussed in Chapter 4 of the report.

3 Sanctions linked to employment-related conditionality

Benefits for unemployment have conditions that claimants should be economically active or otherwise preparing for work. In the UK, for example, receipt of JSA is conditional upon being available for and actively seeking work, demonstrating this every two weeks at the jobcentre. The evidence for this type of conditionality and enforcement through sanctions is long-standing. Being available for work and accepting job offers have been conditions of entitlement for unemployment benefits since their introduction in the early twentieth century.

Thus provision to the unemployed has always rested on the assumption that they behave as such and seek employment rather than being economically inactive.⁶ However, the history of the UK public Employment Service (see Price, 2000) shows how approaches to entitlement and conditionality came under closer scrutiny in the mid-1980s.

The study of the economics of employment and unemployment also developed rapidly from the mid-1970s and clearly demonstrated that future employment prospects were progressively worsened by longer unemployment spells. Reducing unemployment duration (by moving jobseekers into employment) thus became a key aspect of policy.

The academic literature of the 1980s focused primarily on unemployment and programmes for the unemployed. The widening of employment-related conditionality to lone parents began in the late 1980s in the USA, along with the introduction of the US Government ‘waiver’ that allowed states to vary eligibility rules and treatments as long as an experimental evaluation was in place to assess impacts. This ‘waiver’ evidence led to a body of literature that today still dominates amongst high-quality studies of impacts (see Grogger and Karoly, 2005).

In the 1990s employment conditionality became a cross-national concern through a larger strategic discussion of active labour market programmes in both the OECD and the EU (Grubb, 2000). After 1997 the UK began a strategic widening of employment focus to groups who were not unemployed – lone parents and people with work-limiting health conditions. This began as a set of voluntary programmes, but over time has become more conditional, with the threat of sanctions being applied to a wider population. Planned reforms suggest a continuation of this trend in the future (see Griggs and Bennett, 2009).

Our discussion of the impacts of employment-based conditionality with sanctions proceeds in line with this historical development. We first look at the evidence of increasing conditionality and sanctions on unemployment benefit programmes. Then we consider such conditionality and sanctions on lone parents claiming social assistance in the US welfare reform literature. This approach to the evidence also allows us to cumulatively build the evidence base from a small number of direct measures of unemployment and employment to more complex evidence that looks at longer-term effects, such as poverty and child outcomes.

Theoretical approach to employment-related effects

There are two major areas of theory that relate to unemployment benefit sanctions. The first, job search theory (see Mortensen, 1986), focuses on three linked issues:

- 1 Job search intensity: the number of vacancies considered and applied for;
- 2 Job search effectiveness and job matching: the appropriateness of vacancies to the

skills, experience and 'employability' characteristics of the jobseeker; and

- 3 Reservation wage: the wage level sought by the jobseeker and the net income resulting from such a wage after taxes and benefit transfers.

The second, optimal unemployment insurance (see Fredriksson and Holmlund, 2006; Boone *et al.*, 2007; and O'Leary and Wandner, 1997), considers four:

- 1 The design of benefit provision to optimise job search.
- 2 The design of benefit provision to prevent abuse and 'moral hazard' (i.e. deliberate job loss).
- 3 Duration of entitlement and claims.
- 4 The cost-effectiveness of associated employment services – monitoring and administrative practices.

But how do these theoretical approaches fit alongside our discussion of potential sanction effects outlined in Chapter 2?

Moral hazard disqualification sanctions

At this point we can see that part of the second theoretical aim of optimal insurance, the prevention of moral hazard (failing to guard against job loss, because of the existence of the benefit 'safety-net'), does not fit into our theoretical model of sanctions and their effects in Chapter 2. This is because the sanction is a disqualification based on pre-claim behaviour, rather than on behaviour during the claim. Unemployment insurance systems have eligibility rules that seek to minimise this sort of risk by refusing entitlement where unemployment is linked to voluntary or reckless behaviour, specifically resignation or misconduct in work.

Sanction practice in unemployment benefits is dominated by this form of sanction. The proportions of disqualifying sanctions across British, US and Australian unemployment schemes show that they represent the largest single source of sanctions made. In Great Britain between 2000 and April 2009, such sanctions

represented 55 per cent of all those imposed by Jobcentre Plus and its predecessor.⁷ In Australia, 84 per cent of all 'serious failures' that led to sanctions in 2008 fell into this category.

This type of sanction can fit into the 'time' dimension of our theoretical framework as another factor that will reduce inflow into unemployment programmes. However, we must also be aware that such sanctions may be part of an undifferentiated count of incidence thus interpreted as job search sanctions in evaluations.

Job search sanctions

The basic job search model is based on the premise that receiving benefits will lessen search intensity by lowering the 'cost' of unemployment.⁸ Monitoring work-related obligations thus ensures that a level of intensity of job search continues and sanctions can enforce conditionality where there is insufficient activity.⁹ Additionally, job search will increase in advance of the end of entitlement; it is well established that the exit rate out of unemployment tends to increase shortly before the maximum entitlement date (Meyer, 1995; van den Berg, 1990) or prior to the introduction of compulsory Active Labour Market Programmes (ALMPs) (Geerdsen, 2006).

Svarer summarises the theoretical impact of unemployment benefit sanctions as follows:

*Workers face a trade-off between keeping a reduced search level and facing the risk of being caught [and sanctioned] or increasing their search level to comply with the eligibility criteria. Clearly, in both circumstances the utility of being unemployed decreases and the unemployed respond by lowering their reservation wages and if everything else is equal the exit rate from unemployment increases.*¹⁰ (2007, p. 13)

Lalive *et al.* (2002) built on the theory to posit sanctions as acting in two ways: first, by their anticipatory effects on claimants through the awareness of potential sanctions, and, second, by the effects that occur after sanctions are imposed. Laboratory experiments using economics students (with obviously different profiles of motivation, risk aversion and social skills to many unemployed) confirmed the

theoretical point that anticipatory (threat) effects of sanctions can be higher than effects that stem from imposing sanctions (Boone *et al.*, 2007).

At the heart of theoretical approaches to both job search and optimal benefit design are the improvements in effectiveness of job search and thus in the efficiency of the programme brought about by sanctions accompanying conditionality and monitoring. We can think of these potential changes as follows:

- Job search intensity could increase both in anticipation and on imposition of sanctions. Sanctioned individuals are often more closely monitored before entitlement is restored.
- Job search quality could be negatively affected by feelings of duress or optimised by treatments.
- Expected (reservation) wages, where they are 'too high', could be revised downwards through improved information about job vacancies, from job coaching and from the threat of lower income on benefits after a sanction, raising the relative attractiveness of lower wages.

This simple theoretical world is, however, complicated by the context of unemployment programmes and by the characteristics of the unemployed. There are, for example, several crucial characteristics that alter behaviour: motivation, risk aversion, social skills and ability. Such characteristics make interpretations of laboratory experiments on economics students (such as Boone *et al.*, 2004) difficult to apply to the real world. Because unemployment provision exists in specific policy contexts the evidence from evaluations, no matter how robust, can only be fully understood in context.

Evidence from evaluations of unemployment benefit programmes

Our literature searches produced a multitude of international studies of high evidential quality on employment conditionality and mandatory treatments, but only eleven that showed a distinct impact of unemployment benefit sanctions. All

are European and follow on from changes to unemployment programmes introduced across the continent since the mid-1990s. The work of Dutch labour economists has been at the forefront of the estimation of sanction effects. The work of Abbring *et al.* and van den Berg on Dutch UI and social assistance sanctions were the first studies to test the effects of sanctions using econometric analysis of administrative data. This data forms the main source for most of these studies, but has differing characteristics across them. Because data is collected post-claim, studies have not been able to capture any pre-claim inflow effects from non-take-up. Schneider (2008) uses specifically designed survey data that captures individual estimates of past, actual and expected earnings.

The majority of studies employ sophisticated econometric (statistical) modelling techniques to post-claim administrative data, controlling for selection into sanctions while estimating sanction effects. Approaches differ between those based on Abbring *et al.* (1996) and more recent studies that have adopted forms of propensity score matching to produce controls. We do not review the econometric approaches. However, there are three measurement issues that have to be borne in mind across all such studies:

- 1 How 'unobservable' characteristics that affect the likelihood of being sanctioned can be controlled for. Given that most data is administrative there is no ability to observe 'softer' characteristics (e.g. social skills). Similarly, previous labour market experience and history is only available in some of the studies.
- 2 The type of sanction, whether 'administrative' or 'behavioural', is mostly not identified. This means that there is a likelihood of bias in estimating the unemployment exit effects because it may be an exit that brings about the imposition of an administrative sanction (for instance, where claimants do not attend an appointment because they have found a job/are attending a job interview).
- 3 The type of 'exit'. While some studies look only at reduced unemployment spells, most

are able to follow an exit from unemployment by observing a job entry. Few studies look at other exits alongside 'job entries'. A minority equate benefit exits with work entry without sufficient evidence on the destinations of claimants.¹¹

Most of the studies look at a limited number of short-term outcomes, often unemployment spells, exits and job entry, in line with underlying concerns about the negative impact of long-term unemployment on work prospects. Just two look at longer-term or wider outcomes of sanctions: Arni *et al.* (2009) explore both threat and imposition effects on subsequent earnings and employment stability (alongside unemployment exits and job entry), and Machin and Marie (2004) look at the effects of sanctions on crime rates (described as spill-over effects). One study, Schneider (2008), estimates the impacts of sanctions on claimants' reservation wages.

A final general point from these studies is that almost all identify effects from a fairly low sanction rate (typically 2 per cent of claimants). There is no ability to generalise from these studies to assess what effects would come from a higher, more widespread and severe sanction approach. Indeed, Abbring *et al.* (2005) point out 'we should be careful in extrapolating our findings to a much stricter monitoring and sanction regime without considering the equilibrium effects of such a broad reform of the UI system' (p. 629).

We review the evidence from these studies through a series of questions and give a summary table of main characteristics of the studies and their findings in Appendix 2, Table 4.

What (and who) do the studies cover?

While all eleven studies look at unemployed claimants, it is to be expected that there is no common definition of such groups. Studies divide into those that consider:

- unemployment insurance (Abbring *et al.*, 2005; Arni *et al.*, 2009; Hofmann, 2008; Lalive *et al.*, 2002; Müller and Steiner, 2008; Svarer, 2007; Røed and Westlie, 2007). Svarer (2007) used an insured unemployed sample aged over 25;

- a specific unemployment assistance inflow sample – job-losers (Schneider, 2008; van den Berg *et al.*, 2004);
- a subgroup of general social assistance claimants that are defined as 'unemployed' (van den Berg *et al.*, 2004);
- a specific subgroup of the unemployed population – the young unemployed (Jenson *et al.*, 2003);
- the general unemployed population on both contributory UI and unemployment assistance – Jobseekers Allowance (Machin and Marie, 2004).

What types of sanction, with what range of severity, are observed?

Different types of sanction policy are evaluated in these studies and each system has a different mix of short, 'mild' penalties and longer more severe ones. Shorter, less severe sanctions tend to be associated with administrative sanctions that do not relate directly to observed job search behaviour. Van den Berg *et al.* (2004) report that Dutch social assistance sanctions consist of temporary reductions of 5 per cent, 10 per cent or 20 per cent of the benefits level for a potential maximum of six months but usually of only one or two months. Abbring *et al.* (2005), looking at Dutch unemployment insurance, identify sanctions as a temporary or permanent, full or partial reduction of the benefit level that, in practice, ranges from 5 per cent for four weeks to 30 per cent for 13 weeks. Røed and Westlie (2007) cite the Norwegian practice of 'discretionary sanctions' – a temporary loss of UI benefits usually for eight weeks, but which are underpinned by rights to alternative social assistance (at lower levels of payment). Svarer (2007) shows that the vast majority of sanctions are both of short duration and reflect missed appointments and that these sanctions 'drive the main results' (p. 9). Lalive *et al.* (2002) report Swiss 'policy relies more heavily on close monitoring and sanctioning than in other countries' (p. 25) and that sanction rates are the highest among these studies.

No study separately estimates the effects of different types of sanction and only a minority

estimate differences in effect that arise from differing severity.

What sanction effects are estimated?

All studies estimate the effects of actually imposed sanctions; in addition, Arni *et al.* (2009), Lalive *et al.* (2002) and Svarer (2007) estimate the effects of threatened sanctions. None consider take-up effects.

Of those studies looking only at short-term effects, Lalive *et al.* (2002) and Svarer (2007) both look only at unemployment exit rates, with Lalive *et al.* defining exits as a combination of those to a regular job, an ALMP and out of the labour market (to 'inactivity') (but do not decompose results between destinations) and Abbring *et al.* (2005) measuring 're-employment rates' with a wide definition that includes all 're-employment durations' (p. 615). Other studies looked at a broader range of post-unemployment destinations. For example, Müller and Steiner (2008) consider 'confirmed employment entry' and distinguish by type of employment, including subsidised employment and self-employment. Van den Berg *et al.* (2004) look at different 'exit destinations', but note that the most common is to employment (p. 223). Unknown destinations and migration out of the locality (reflecting the Netherlands' highly decentralised system) are also considered. Hofmann (2008) and Røed and Westlie (2007) attempt to separately measure the different destinations of programme leavers. In Hofmann's case to 'regular employment', 'other employment' and out of the labour force to 'inactivity', in Røed and Westlie's to regular employment, another benefit, education, non-participation (i.e. out of the labour market) and to an ALMP. Jensen *et al.* (2003) have distinctive exits dictated by the nature of the Youth Unemployment Programme (YUP) they are evaluating.

The most recent study by Arni *et al.* (2009) makes a large contribution to the literature by considering both short- and longer-term effects. It isolates both threat and imposition effects in terms of not only unemployment exits and entry to employment but also subsequent earnings and employment stability. This study significantly takes forward the empirical study of sanction effects.

What are the sanction effects?

The basic evidential finding across all studies that deal purely with short-term effects is a consistent, large and significant reduction in unemployment durations and/or increasing employment entry rates (see Table 4 for details of raw results). Müller and Steiner (2008), for example, find convincing evidence of an imposition effect: that sanctions increase 'regular employment' especially when they occur early in a claim. Additionally those studies looking both at imposition and threat effects (Lalive *et al.*, 2002 and Svarer, 2007) identified positive impacts on job entry (although in the case of Svarer's study these threat effects were only evident for men).¹²

However, the most recent study in this field takes analysis of effects further and while Arni *et al.* (2009) not only confirm such findings of short-term effects, they additionally show longer-term effects of reduced employment sustainability and of lower earnings. Specifically, employment duration following the unemployment spell was reduced by sanctions (a direct imposition effect). Controlling for such differences in employment duration, there were also consistent effects of lower earnings both immediately after leaving unemployment and after two years. Reduced earnings resulted from both threatened and imposed sanctions.

Of course, it is possible to argue that the overall effect of sanctions is an efficient one despite different short-term and longer-term impacts. The argument is that if sanctions reduce unemployment duration this can give rise to an overall net increase in income from longer periods of earnings despite any changes in earning levels or job retention. However, Arni *et al.* (2009) demonstrate that this is not the case: 'over a period of two years following the exit from unemployment, the net effect of benefit sanctions is negative' (p. 33). This is an extremely important additional finding from this study. It has significance for the empirical justifications of sanctions that we return to consider further in Chapter 6.

Exits from unemployment benefit spells are not always to employment. Røed and Westlie (2007), for example, find more exits to education than to employment. They also find that the probability of exiting to a different type of benefit falls after a sanction. However, the generalisability of findings

on such 'other' exits tend to be fairly low, as there are often specific features of national systems which dictate these pathways. A similar problem of generalisability comes from Jensen *et al.*'s (2003) study of youth unemployment in Denmark. The fixed-term nature of the benefit, together with clear seasonality of entry into education programmes (i.e. the start of each academic year), leaves estimates of distinct sanction effects uncertain.

Schneider's exploration of impacts on earning expectations (2008) finds no effects from imposed sanctions (a finding that runs contrary to job search theory, which indicates that sanctions will directly reduce the reservation wage [*ibid.*, p. 4]). It is not clear how such a finding would be generalisable to other samples or countries as the claimants observed were social assistance claimants and had expectations for earnings at the bottom end of the wage distribution.

The only study to consider wider spill-over effects explores the role of sanctions in property and violent crime (Machin and Marie, 2004). Authors find that property crime rates in areas that had more sanctioned individuals rose. Additionally, they find that areas with higher levels of exits from unemployment that were not into employment (dropping off the register but not being in education/training or on other benefits) also had higher crime rates. These effects were measured before and after the introduction of JSA in 1996/7 and were seen to be persistent for two years. Machin and Marie discuss a crude cost-benefit approach that discounts the savings from benefit budget with the increased costs of crime and find a 22 to 28 per cent of the reduction in gross benefit expenditure was netted off by higher spending on crime.

Do sanction effects persist over time?

While there is consistent evidential support for sanction effects on unemployment exits and job entry, there is greater difference across the studies about how long the sanction effect lasts during an unemployment spell. Abbring *et al.*'s evidence tends to support the hypothesis that the sanction effect persists over time and suggests that sanctioned populations are more closely monitored during the sanction period and perhaps subsequently. Lalive *et al.* also find imposition

effects do not significantly drop over time, but do suggest that the effect of a warning decreases by 16 per cent after 30 days (if not followed by a sanction). On the other hand, Røed and Westlie (2007) find that 'most effects are short-lived ... After the sanction is completed (and the job-seeker again receives benefits)' only entry into an ALMP or to a repeat sanction remains at a higher level (p. 35). Svarer (2007) finds the highest effect during the first month post-sanction; these are still positive but lower at two months post-sanction. At three months the effect is no longer significant. Müller and Steiner (2008) also find that over longer durations the differences between the sanctioned and non-sanctioned narrow to insignificance. Similarly, other studies tend to see some weakening of sanction effects over time.

Arni *et al.* (2009) is the only study to look at persistence of effects after leaving unemployment, finding sustained reductions in earnings levels two years after exit alongside increased churning in employment.

Therefore, we see a clear problem of inconsistent findings in the literature on the persistence of effects over time. It may be that these differences spring from the methodological estimation of econometric models, with 'timing of effects' based models more likely to find sustained effects while other approaches find less persistence.

There is another potential problem interpreting differences by the timing of sanctions. The overall picture from the studies is that sanctions imposed early in the claim have more effect. Svarer (2007) finds that sanctions imposed 15 months or later in the unemployment spell produced no employment exit effects, and this is also found in the German studies of employment outcomes (Müller and Steiner, 2008; Hofmann, 2008). However, sanctions imposed early in the claim are more likely to be administrative sanctions (or disqualifications on the grounds of intentional job loss) and thus there is no clear causal link to changed job search behaviour.

How are different subgroups affected?

There is a common finding across a number of studies that macro-factors influence sanction outcomes. Hofmann's (2008) study, for example,

shows that sanction effects differ with regional unemployment rates. Women in low unemployment regions are likely to have regular employment outcomes following a sanction, and men in high unemployment regions respond more to early period sanctions than men in low unemployment regions. Hofmann concludes that 'the effectiveness of sanctions not only depends on the individual reaction but also ... on labour market conditions' (ibid., p. 21). Additionally, office or regime effects are found where different levels of sanctions operate due to differences in the organisational culture, in interpretation of policy and/or the operation of discretion (ibid.).

Svarer (2007) decomposes results by both sanction severity and gender and finds that for both men and women tougher sanctions are more effective among older, long-term and immigrant subgroups. Svarer also considers and differentiates by type of sanction, finding the risk of administrative sanctions to be highest during the first 15 weeks of the claim (often the result of missed meetings) and that while 'men tend to avoid meetings, women are more reluctant to accept job offers or attend job Interviews' (ibid., pp. 9–10).

Müller and Steiner (2008) find that aggregate sanction effects are mainly driven by the younger unemployed, and by younger women amongst a female sub-sample. Effects of sanctions on exits to 'low-quality' jobs (poor pay and conditions) are positive for women and negative for men. Additionally, they find sanction effects on exits out of the labour force (to inactivity) are stronger for older women; and for men when the sanction is imposed in the middle to late period of the claim.

Summary of findings on UB sanctions

Overall we find compelling and consistent evidence of short-term sanction effects, raising unemployment exit and job entry rates. However, there are far more mixed results concerning the sustainability of these effects over time, as well as regards impacts on different claimant subgroups. Furthermore, studies looking at longer-term effects demonstrate less favourable results: for example, the one study to look at longer-term post-unemployment effects finds

persistent negative impacts on earnings and job churning two years after exit. And Machin and Marie's study of sanction spill-over effects identifies an unfavourable impact on crime rates.

Despite some strong findings there are still many unknowns, for example in terms of sanction duration and severity. Indeed as Schneider reminds us, 'the exact behavioural effect of a sanction is still undetermined in an ongoing and vivid research debate' (2008, p. 43).

Evidence of evaluations of US welfare programmes

US welfare reforms that took place in the 1990s led to a large expansion in the use of sanctions which have become central to most state TANF programmes. Although influenced by federal rules,¹³ under the current block grant system states are left to make their own decisions regarding the shape, nature and implementation of their own sanctions policies, which has given rise to large variation in practice (Blank and Haskins, 2001). Sanctions may be partial (decreasing assistance by a fixed proportion or by removing the adult component of a family benefit¹⁴) or full ('full-family sanctions', which remove all entitlement) (Bryner and Martin, 2005), temporary or permanent (Bryner and Martin, 2005), in some cases having long-lasting consequences for future eligibility. The W-2 programme in Wisconsin, for example, has a 'strike' system: failure to participate in work requirements without good cause can lead to one strike and three such strikes to a lifetime ban (Wu *et al.*, 2004). These differences are highly important, because those states with severe sanction policies that remove significant proportions of sanctioned claimants (and their families) from their rolls have (potential for) considerable entitlement effects and also high corresponding effects on inflow, longer-term outcomes and spill-over effects.

Incidence and intermediate outcomes

Two pieces of information are important in order to contextualise evidence on impacts:

- 1 An estimate of sanction incidence (the number of claimants affected by sanctions); and

- 2 An understanding of intermediate outcomes (the proportion of claimants coming back into compliance following a sanction versus those exiting welfare rolls thus ending their spell on welfare).

Both are problematic. Figures on incidence, for example, range from 5 to 60 per cent of the caseload. This variation stems from differences in the way incidence is measured, the state or area the incidence applies to and the timing. While the GAO study, which found a nationwide sanction rate of 5 per cent, is widely interpreted as a low count of incidence¹⁵ (Wu *et al.*, 2004), cohort studies (e.g. Fein and Lee, 1999), by their nature, show a very high, although arguably more accurate, incidence (figures of 45 to 60 per cent have been reported [see Pavetti *et al.*, 2003, Table 3]). Few sources of evidence consider issues of compliance versus removal from the rolls. However, we can make some tentative judgements based on limited data; for example, Pavetti *et al.* (2003) suggest that approximately a third of claimants come into compliance after receiving a sanction (see also Fein and Lee, 1999 and Meyers *et al.*, 2006), while a further 17 to 51 per cent remain sanctioned (Meyers *et al.*, 2006) and between 7 and 33 per cent are removed from the rolls¹⁶ (Fein and Lee, 1999; Kalil *et al.*, 2002; Wu *et al.*, 2004; Schram *et al.*, 2008). Pavetti *et al.* suggest that the proportion of claimants ‘sanctioned-off’ caseloads may have risen over time (2003).

In a consideration of the relationship between sanctions and participation, the National Evaluation of Welfare-to-Work Strategies (NEWWS) found higher sanctioning rates ‘were not associated with higher participation rates’ (Blank and Haskins, 2001, p. 255). However, in a rare specification of the threat effect, Lee *et al.* (2004) estimate that claimants receiving a sanction warning were more than twice as likely (odds ratio = 2.09¹⁷) to be participating in job search and training as other claimants (*ibid.*, p. 390). This same study, unusually, also considers the reasons behind the sanction: approximately 60 per cent being imposed for failing to undertake one of the work-related activities or refusing a job offer, the remainder being for missing meetings or failing to provide verification papers (*ibid.*).

What aspects do these studies cover?

The theoretical approach discussed earlier also applies to employment-related conditionality for ‘welfare’ populations but with the obvious underlying point of how far lone parents are ‘available for work’ given their childcare responsibilities.¹⁸ With social assistance being an anti-poverty programme and with the obvious potential for child outcomes to be affected alongside the circumstances of the parent, evaluations have tended to look a little more prominently at longer-term effects. We rely on the excellent systematic review of ‘welfare reform’ by Grogger and Karoly (2005), which cites eight individual studies that have evidence of distinct impacts of sanctions. This evidence is supplemented by three studies identified during our literature search – Lee *et al.* (2004), Peck (2007) and Wu (2008).

Despite a greater focus on longer-term outcomes, the evidence from US welfare reform is, however, dominated by ‘entitlement’ effects, of measured falls in caseload and welfare exits in the main. The preoccupation with caseload decline stems from the political economy of US welfare reform and the incentives on states to reduce caseloads, federal grants being set at year one caseload levels (the bigger the caseload drop the greater the savings and potential to use grant aid for related, but other, purposes).

The studies extracted from Grogger and Karoly’s meta-analysis all applied econometric methods to observational (mostly administrative or national survey) data. Of the three additional studies, Peck (2007) uses propensity score matching (PSM) and experimental data from the NEWWS evaluation to compare treatment and control-group members who are high sanction risks; Wu (2008) uses event history analysis and longitudinal administrative data and Lee *et al.* (2004) control for potentially explanatory variables in their longitudinal survey and administrative dataset. In some cases (CEA, 1997 and 1999; Moffitt, 1999) the sample periods begin in the late 1970s and run through to the mid- to late 1990s – encompassing pre-PRWORA and waiver periods. Different state-level reforms introduced over this period and around PRWORA created a ‘natural experiment’ (Grogger and Karoly, 2005, p. 61) that

has been used by quantitative researchers to ‘isolate the causal impact of welfare reform on welfare related behaviour’ (ibid.). However, the same measurement issues discussed in reference to the econometric UB studies also apply here. It is worth noting that while observational econometric analysis is (technically) able to capture the effects of reform on welfare entry/take-up (by studying the entire ‘at risk’ population), the ‘bundled’ nature of reform makes isolating the effect of specific measures (e.g. sanctions) extremely difficult.¹⁹ A similar problem occurs for general threat effects, which may be so intrinsically tied to the conditions they accompany that it becomes near impossible to disentangle them.

Indeed, the majority of studies in this area do not attempt to separately estimate sanction effects, most looking at ‘bundles’ of reforms (Grogger and Karoly, 2005) rather than components. This distinguishes the ‘welfare’ literature from the unemployment benefits studies, where evaluation was specifically concerned with changes to sanction practice. In common with the unemployment literature, welfare reform evaluations tend to look only at imposition effects; rarely do they look at the timing of exits, or at the different destinations of sanctioned leavers. Neither do they distinguish between the effects of different types of sanction (behavioural or administrative), although one does contain basic descriptives (Lee *et al.*, 2004).

What (and who) do the studies cover?

All studies included here look at (current or past) welfare claimants – generally TANF or its predecessor ADFC, although some TANF programmes are referred to by their state names (e.g. Wisconsin’s W-2). Studies use a number of different sources:

- annual state-level administrative data (CEA, 1997; CEA, 1999; Moffitt, 1999);
- monthly state-level administrative data (Wu, 2008; Ziliak *et al.*, 2000 in Grogger and Karoly, 2005);

- changes in state-level administrative data (MaCurdy *et al.*, 2002; Mead, 2001; Rector and Youssef, 1999 [all in Grogger and Karoly, 2005]);
- longitudinal survey data combined with administrative records (Lee *et al.*, 2004);
- experimental data (Peck, 2007);
- state child protection system aggregate data (Paxson and Waldfogel, 2003).

What types of sanction, with what range of severity, are observed?

As introduced above, different US states have different sanction policies, as well as different grant values. Therefore the ‘severity’ of a sanction in both proportional and real (cash value) terms is highly variable (as is incidence or likelihood). These differences are evident in the studies reviewed here.

Although some (CEA, 1999; MaCurdy *et al.*, 2002; Mead, 2001; Rector and Youssef, 1999; Wu, 2008) attempt to break down impacts by sanction severity, there is often little consistency between studies in the classifications used (see Grogger and Karoly, 2005, p. 119), making comparison difficult. Just one study (Wu, 2008) considers the duration of sanctions.

What sanction effects are estimated?

Like the UB studies, all welfare evaluations consider imposition effects; just one, Lee *et al.* (2004), looks at threat effects, that is of sanctions ‘initiated but lifted prior to grant reduction’ (p. 377). Peck (2007) is unique in that the author compares sub-samples with different levels of sanction risk in treatment and control groups – coming closer to identifying a more general threat effect.

The lack of studies directly exploring *ex ante* effects means that much of the supposed impact on behaviour and other outcomes as a result of ‘threat’ or ‘anticipation’ will be part of undifferentiated effects in most evaluations. The absence of studies on threat effects thus joins the absence of studies on inflow or take-up effects as a weakness of US welfare evidence that ‘fails to separate the impact of grant reductions caused

by a sanction from the sanction as a tool to induce behavioral changes' (Lee *et al.*, 2004, p. 378).

Entitlement effects predominate in the studies identified, being the sole consideration of virtually all those included in the Grogger and Karoly meta-analysis (CEA, 1997; CEA, 1999; Moffitt, 1999; MaCurdy *et al.*, 2002; Mead, 2001; Rector and Youssef, 1999; Ziliak *et al.*, 2000); in this case all studies look specifically at 'caseload/welfare use' (Grogger and Karoly, 2005, p. 121).

The three other studies looked at both entitlement effects and a wider range of outcome measures. Wu (2008) tracked W-2 participant employment and earnings by merging a TANF dataset (CARES²⁰) with a UI one. Thus the study considers a small range of outcomes: welfare exit, employment and level of earnings in the quarter after exit.²¹ Lee *et al.* (2004) considered welfare, work and hardship outcomes – including participation in informal work and in job or vocational training programmes. And Peck (2007) used PSM to explore the impact of 'sanction risk' (that is the participant subset most likely to experience sanctions) on work, earnings and income.²²

Uniquely, Paxson and Waldfogel (2003) considered the impact of welfare policy (including sanctions) on child maltreatment outcomes. These authors used a variety of outcome measures including: out-of-home care, reports and substantiated cases of abuse and neglect.

What are the sanction effects on caseloads?

Caseload numbers have been used to gauge

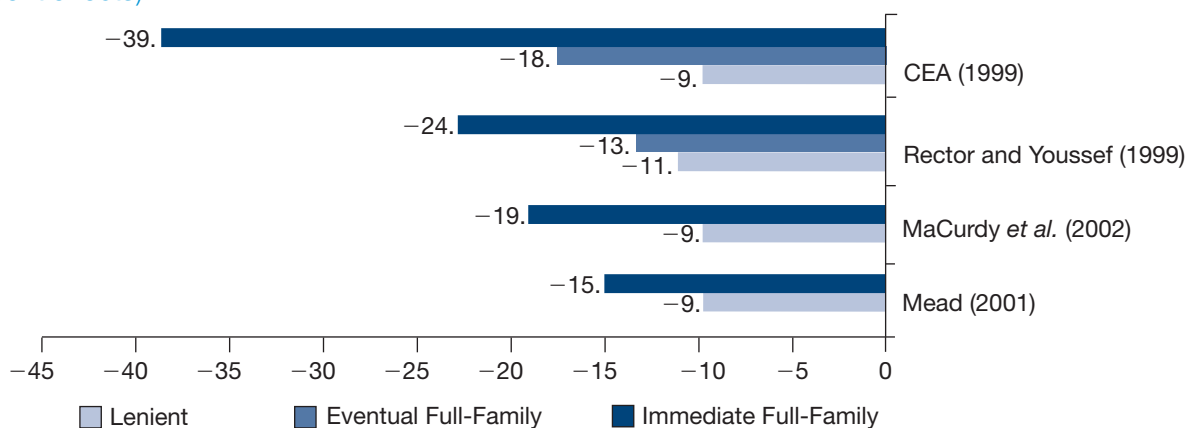
the overall success of welfare reform (Grogger and Karoly, 2005) but as we demonstrate in Chapter 2 these are mostly short-term and instrumental. Nevertheless, the most robust US evidence lies here.

Across virtually all studies looking specifically at welfare use and caseloads we find that more severe sanctions result in significant reductions. Indeed, the five studies separating impacts by sanction severity (CEA, 1999; MaCurdy *et al.*, 2002; Mead, 2001; Rector and Youssef, 1999; Wu, 2008) all found that the most severe sanctions (immediate full-family) produced the largest declines (see Figure 3) (an average of 25 per cent).²³

Where sanction effects are not broken down by severity (i.e. a simple sanctioned or not sanctioned binary), the results are far less clear. Findings from the early (pre-1996 reform) studies (CEA, 1997; Moffitt, 1999; Ziliak *et al.*, 2000) produce just one statistically significant finding between them, a 9.7 per cent caseload reduction (CEA, 1997), although all suggest a decrease in welfare use (Grogger and Karoly, 2005, pp. 116–17). Lee *et al.* (2004) found no evidence that sanctions, or the threat of sanctions ('initiated but lifted prior to grant loss' [p. 383]), were related to a statistically significant degree to being off welfare.

Likewise, impacts of partial (or more lenient) sanctions are more ambiguous. Figure 3 shows that only half the impact results of eventual full-family and lenient sanctions are significant, although all suggest a decline. Wu (2008), however, found that the smallest sanctions (<10 per cent) were significantly associated with lower welfare

Figure 3. Results from post-reform observational studies on the effects of sanctions on caseload/welfare use (per cent effects)²⁴



Source: Grogger and Karoly (2005): figure 5.5

exit rates. Furthermore that moderate sanctions (11–50 per cent) had little significant effect (p. 38).²⁵

What are the sanction effects on employment and earnings?

We must begin with a warning about the potential limitations of the impact studies addressing outcomes other than welfare use. While all studies (Lee *et al.*, 2004; Peck, 2007; Wu, 2008) adopt what we consider to be a robust methodology, all are subject to different problems. Peck, for example, is reliant on self-reported sanction data in her PSM study (2007). Lee *et al.* (2004) overcome the problem of self-report data by merging survey and administrative data but note that although a ‘comprehensive set of control variables is used’ (p. 381) the possibility of selection effects in unobserved differences between sanctioned and non-sanctioned recipients makes inferences of causality problematic. Wu (2008) experiences a similar problem being unable to control for ‘unobserved covariates and unmeasured variables’, and notes that findings may not be generalisable outside Wisconsin thanks to high sanction rates and heavy work orientation (pp. 41–2).

Perhaps a product of methodological differences and difficulties are the mixed results we find in terms of destinations of leavers. While findings from both Wu (2008) and Lee *et al.* (2004) indicate that sanctioned claimants are less likely to be employed, Peck (2007) suggests that being at a higher risk of sanctions induces greater work levels. Specifically Wu (2008) indicates that ‘compared to those not currently sanctioned, those currently sanctioned are 18 per cent more likely to leave welfare without work’ (p. 35; see table 1, column

1 – ‘current sanction’). However, the effect varies with the severity of the sanction, the largest impacts on the odds of leaving welfare without work being generated by full-family sanctions (see Table 1). Lee *et al.* (2004) report that those who experienced sanctions were 44 per cent less likely to be employed than those who did not, but also found higher rates of informal working among sanctioned claimants.²⁶ Sanction threats did not appear to have a significant effect on formal employment, but like imposed sanctions were related to an increased likelihood of informal work (odds ratio = 3.23) (*ibid.*). This is an important finding as it highlights a distinction in the type of work claimants with different sanction histories are likely to enter. Both studies are thus comparing sanctioned with non-sanctioned claimants while employing methods to control for selection effects. However, while Wu compares exits and breaks effects down according to sanction severity, Lee looks at participation in formal employment in quarters two and three of 2001, and participation in informal work.

Peck’s study is rather different in that it uses propensity score matching (PSM) to compare subgroups of those at high sanction risk and low sanction risk in treatment and control groups. Thus unlike Wu and Lee the author is not comparing sanctioned and non-sanctioned, but estimating the effect of a treatment (the NEWWS programme) on these two subgroups. Rather than describing the effect of being sanctioned on employment, Peck tells us that those belonging to the high sanction risk treatment subgroup are significantly more likely to be employed than their control counterparts. This is true across all five follow-up years (although

Table 1. Sanctions and the destinations of welfare leavers – odds ratios

	Current sanction	Low sanctions (<10%)	Medium sanctions (11%–50%)	High sanctions (51%–90%)	Full sanctions (>90%)
Likelihood of leaving welfare without work	1.18	0.65	0.92	1.23	2.13
Likelihood of leaving to lower earning job	1.01	0.68	0.95	0.95	1.47
Likelihood of leaving to higher earning job	0.74	0.7	0.6	0.81	0.97

Source: Wu (2008): tables 2 and 3

the effect was smaller in years four and five). However, it is interesting and important to note that those belonging to the low sanction risk treatment subgroup also significantly outperformed its control, but only in the first year: by year five the control group had overtaken the treatment group on this outcome measure.

Earnings impacts are subject to the same comparison difficulties. While Wu measured the likelihood of exits to 'higher-earning' and 'lower-earning' jobs alongside exits to worklessness in the first quarter after exit, Lee *et al.* (2004) considered mean earnings from formal employment in quarters 2 and 3 of 2001 for sanctioned, non-sanctioned and threat-of-sanction groups (thus corresponding directly with impacts on formal employment – reported above). Peck (2007) compares earnings of the two sanction risk groups with their control counterparts across the five follow-up years.

On the basis of the basic higher/lower-earning measure, Wu demonstrates the lesser likelihood claimants with a current sanction have of exiting to a higher earning job.²⁷ Breaking this down by sanction severity it is only full sanctions that are associated with increased exits to work – specifically to lower-paying jobs.²⁸

Similarly, Lee *et al.* (2004) identified an association with lower earnings; the authors found that 'those who were sanctioned, on average, earned about \$1,320 less in the last two quarters of the study period than those who were not sanctioned' (p. 390). The threat of sanctions (those initiated but lifted prior to grant reduction) did not impact significantly on earnings (*ibid.*).

Peck (2007) found the average earnings of the high sanction risk group to be significantly higher for recipients in the treatment group when compared to their control counterparts. This was true of all but one of the five follow-up years (the exception being year four with a non-significant impact). Again the low sanction risk subgroup 'outperformed' its control, but only in year one. Interestingly there was no corresponding increase in income for either group, the rise in earnings seemingly offset by the welfare grant reduction (see Peck, 2007, p. 268).

What are the sanction effects on hardship?

The relationship between sanctions and economic hardship is explored in just one impact study. Lee *et al.* (2004) considered claimants' experience of food, rent and general hardship.²⁹ Their study determined that whilst sanctioned claimants were more likely than others to experience food and general hardship (the effect on 'food hardship' being particularly pronounced), the threat of sanctions appeared to impact on rent hardship – a higher incidence of problems meeting housing payments.

What are the sanction effects on child welfare?

A number of studies have identified links between welfare receipt and child safety (Meyers *et al.*, 2006; Pavetti *et al.*, 2003). For example, Courtney *et al.* (2001) suggest that in Milwaukee more than half of TANF claimants had been investigated by Child Protective Services (discussed in Pavetti *et al.*, 2003). And in California 27 per cent of children belonging to families in receipt of welfare in 1990 had experienced a maltreatment report within five years (Needell *et al.*, 1999 in Pavetti *et al.*, 2003). However, there is little by way of impact evidence. Indeed just one study was identified, Paxson and Waldfogel (2003). This investigated the impact of sanctions, work requirements and time limits on child maltreatment and out-of-home care, and found, among many insignificant results, that a first full-family sanction under TANF raised reports of physical abuse, reports of neglect and substantiated cases of abuse (see Grogger and Karoly, 2005, p. 223). However, the reliability of results has been challenged by the absence of controls for some potentially influential policy variables.

Do effects vary by the timing and duration of sanctions?

Just one study (Wu, 2008) analysed exits according to the timing and duration of sanctions.³⁰ Findings indicate that (holding current sanction status constant) 'families that had been sanctioned at some point during the first welfare spell were significantly more likely to leave welfare without a job or to leave welfare with a lower-earnings job than families who

were not sanctioned' (ibid., p. 37). This effect was most pronounced for those sanctioned one to three months prior to exit, claimants being 63 per cent more likely to leave for a lower-earning job. Those sanctioned for two or more consecutive months were significantly more likely to leave without a job or to a lower-paying job than non-sanctioned claimants.³¹ No association was identified between 'longer spells of current sanctions and the likelihood of having a higher earnings job' (Wu, 2008, p. 40).

Summary of impact findings

Consolidating findings of UB and welfare evaluations, we can conclude that sanctions for employment-related conditions (full-family sanctions in the case of US welfare systems) have strong favourable impacts on benefit use and exits and generally unfavourable effects on longer-term outcomes. However, beyond this the evidence is far harder to reconcile; for example, while UB programmes tend to demonstrate positive impacts on employment (job entry), this is not the case for welfare studies.

In sum:

- There is compelling and consistent evidence of the short-term effects of sanctions in unemployment benefit (UB) systems – raising unemployment benefit exits and job entry.
- Evidence of sanction effects in the US welfare system is more mixed. While more severe sanctions (in particular, immediate full-family sanctions) significantly reduce welfare caseloads, evidence on employment and earnings is far less conclusive or favourable (two studies indicating a negative impact, the third a positive one).
- Recent evidence from a single study of unemployment benefit sanctions (Arni *et al.*, 2009) suggests earlier UB exits prompted by sanctions result in poorer quality employment (lower earnings and job instability).
- The one study exploring spill-over effects found that sanctions had unfavourable impacts on local crime rates (Machin and Marie, 2004).

- An exploration of earning expectations found no effects from imposed sanctions (Schneider, 2008).
- Studies offer a clear indication of the importance of macro-economic effects and also of implementation or organisational effects.
- Despite this evidence there are still a lot of unknowns, creating a need for more evidence on the duration of effects, difference by claimant subgroup and inflow and take-up effects as well as on a range of exit destinations and longer-term impacts, such as income, post-unemployment work sustainability and earnings.

4 Contextual studies of employment-related sanctions

The nature of the impact studies identified means that there are potential effects that are poorly covered by high-quality evidence. In this Chapter we relax the evidential quality requirement for studies in order to gain further insights into the effects of sanctions in theoretically important areas.

Claimant knowledge and awareness

It is an unspecified assumption for sanctions to operate effectively that claimants understand the behavioural conditions of entitlement and the penalties for breaching them. Additionally, (in theory) they must be aware when they have been sanctioned and know how to go about resolving or ‘curing’ these penalties. However, there is a body of evidence that suggests there are significant gaps in claimant knowledge as regards their responsibilities and the penalties for not meeting them (SSAC, 2006).

British evidence indicates that although most claimants are aware of sanctions and understand the principles behind them,³² they have little knowledge of the details of the sanctioning system (see Dorsett, 2008; Goodwin, 2008; Joyce and Whiting, 2006; Legard *et al.*, 1998; Peters and Joyce, 2006; Smith, 1998). For some claimants this means a disconnection between awareness of mandate and resulting sanctions (Dorsett, 2008) – often the result of insufficient communication and explanation by personal advisors or service providers (Vincent, 1998). Research conducted with sanctioned claimants shows that many did not know how the penalty could have been avoided,³³ and that knowledge of the specific ‘transgressions’ that led to sanctions was extremely poor (for example, very few claimants were able to name, unprompted, items such as ‘not actively seeking work’ and ‘leaving a job voluntarily

without good reason’ [Peters and Joyce, 2006]). As with the wider system, there are also very low levels of claimant awareness concerning the details of the sanction appeal process.

This lack of awareness sometimes continued even once a sanction had begun (Dorsett, 2008; Goodwin, 2008). Indeed, few claimants in Goodwin’s study knew how much their usual benefit payment should be. Those who already had deductions for social fund loans, previous non-payment of bills or another source of income sometimes went for long periods before becoming aware of the sanction (Dorsett 2008; Goodwin, 2008).

Qualitative interviews with jobcentre staff suggest that, although staff try to ensure rules are comprehensively conveyed to customers, the complexities and level of detail inherent in the system make this extremely difficult (Peters and Joyce, 2006). However, considerable individual and organisational differences were identified in the quantity and quality of information conveyed (verbally) to claimants; this also varied according to perceived claimant need (Joyce *et al.*, 2005).

US evidence tells a similar story, with low levels of understanding of the regime, little knowledge about how sanctions might be avoided and inconsistent relaying of information from staff (SSAC, 2006). Pavetti *et al.* (2003), for example, suggest that information about sanctions is inconsistently delivered – a result of non-attendance at orientation meetings and also variation in the quality of meetings. A study comparing self-reported sanction status with administrative data indicates that approximately a third of claimants are unaware of their sanction status (Lee *et al.*, 2004). Findings also demonstrate a strong association between a lack of knowledge of welfare rules (and therefore the sanctions system) and a greater likelihood of being off the welfare rolls (Lee *et al.*, 2004).

Administrative capacity and consistency

A further assumption of the theory is that sanctions operate according to eligibility rules that are applied rationally and without bias. However, evidence from the US finds persistent concerns of bias from race (Meyers *et al.*, 2006). Schram *et al.* (2009) used hypothetical vignettes (triangulated with administrative records) to explore whether racial biases and 'discrediting social markers' affected officials' decisions to impose sanctions (p. 398).³⁴ They found that case managers were more likely to recommend sanctions for black and Hispanic claimants than white claimants when they have evidence of prior sanctions. African Americans, research suggests, are at particularly heightened risk; specifically, a black woman with a prior sanction had a 0.97 predicted probability of being sanctioned compared with 0.75 for her white counterpart. Interestingly, the study also indicated that more experienced case workers were less likely to impose sanctions, but found no differences on the basis of the case workers' race. Explanations offered for such differential sanction rates: 'have included the possibility that racial stereotypes contribute to thinking that blacks are in need of more coercive and disciplinary approaches that will increase their motivation and change their behaviour so that they will be more willing to leave welfare for paid employment' (Schram *et al.*, 2008, p. 20).

Our earlier discussion of unemployment insurance found differences in the likelihood of sanctions, independent of caseload characteristics, between unemployment benefit offices in Switzerland, Germany and Norway, despite the fact that national-level rules were consistent across such offices and should have meant uniform treatment. Similar findings have also been reported in studies conducted in the UK, Australia and the US; for example, the CalWORKs evaluation found county of residence was strongly associated with sanction likelihood (Ong and Houston, 2005; see also Meyers *et al.*, 2006). And Fein and Lee (1999) found that even after controlling for differing caseload compositions sanction rates differed significantly between offices.³⁵

In a UK study of the New Deal for Young People, Bonjour *et al.* identified similar differences by locality and also by delivery: those living in areas with a more intensive gateway service³⁶ with private-sector-led delivery and living in the Employment Service Northern region being significantly more likely to be sanctioned than their counterparts (Bonjour *et al.*, 2001, p. 115). It is likely that differences reflect inconsistencies in policy interpretation and implementation (Marston and McDonald, 2007), local organisational culture and practices (discretion and willingness to sanction) as well as local barriers to compliance (e.g. lack of transport or childcare) (Meyers *et al.*, 2006; Ong and Houston, 2005).

Selection into sanctions: characteristics of sanctioned claimants

Information issues of claimant understanding and consistency issues of administration come together when considering who is being sanctioned. Reviews of UK (Peters and Joyce, 2006), US (Lee *et al.*, 2004 and Pavetti *et al.*, 2003) and Australian evidence (Eardley *et al.*, 2005), together with a number of studies specifically exploring differential rates of sanctioning by claimant characteristics, often report high-quality evidence, but not with any experimental or econometric controls.³⁷ However, generally high levels of consensus between the results of different studies (some using self-report and others administrative data) indicate a degree of reliability (Lee *et al.*, 2004).

Demographic characteristics

A number of demographic characteristics have been consistently associated in the literature with a greater likelihood of being sanctioned. In the US, ethnicity has proved particularly significant and has received considerable attention in sanction studies (Meyers *et al.*, 2006) (see Table 1). In particular, research has identified positive associations between being African American and being sanctioned (Cheng, 2009; Kalil *et al.*, 2002; Ong and Houston, 2005; Pavetti *et al.*, 2003); in one study rates are more than 25 per cent higher than for white claimants³⁸ (Meyers *et al.*, 2006). Similar patterns have also been identified in Australia,

with a higher incidence of ‘breaches’ amongst the indigenous population (Eardley *et al.*, 2005; Marston and McDonald, 2007).

In the UK the relationship between sanctions and ethnicity appears to be mediated by claimant knowledge of the sanction process – with white British survey respondents demonstrating a greater awareness of the system than other ethnic groups (Peters and Joyce, 2006); implying that language may play a role. While Australian research also identifies a link between language and sanction likelihood, US research suggests far more mixed results (Meyers *et al.*, 2006) with one study showing non-native English speakers to be at substantially lower risk of sanctions (Ong and Houston, 2005).

Age, or more specifically youth, is a factor associated with sanction likelihood evident across the literature. In the US sanctioned claimants have been shown to be, on average, two years younger than the non-sanctioned (Born *et al.*, 1999 and Koralek, 2000 in Pavetti *et al.*, 2003) with those aged under 24 being at particular risk (Hasenfeld *et al.*, 2002 in Pavetti *et al.*, 2003). Similarly, Peters and Joyce (2006) found a disproportionately large number of referrals and enforced sanctions amongst JSA claimants aged 16 to 24. Advisors interviewed as part of this mixed-methods study believed this to be a product of younger claimants’ more ‘relaxed’ attitude to sanctioning, thought to be the result of the financial safety net provided by their families (Peters and Joyce, 2006, p. 17).

Other demographic features identified in the (US) literature are primarily concerned with family type and formation. Belonging to a large family or household (Cherlin *et al.*, 2002; Lewis *et al.*, 2003 in Lee *et al.*, 2004; Mancuso and Lindler 2001), being single (i.e. never married and not living with a partner) (Edelhoc *et al.*, 2000; Kalil *et al.*, 2002; Lewis *et al.*, 2003 in Lee *et al.*, 2004; Westra and Routely, 2000 in Pavetti *et al.*, 2003) and being, or having been, a young parent³⁹ (Ong and Houston, 2005; Pavetti *et al.*, 2003) have all been associated with an increased likelihood of receiving a sanction. All are also characteristics that have traditionally been associated with longer claim durations (Pavetti *et al.*, 2003; Lee *et al.*, 2004).

Gender was identified as a significant factor only in the UK and Australian material (the US claimant population being comprised almost

exclusively of women), with female unemployment benefit claimants being less likely to be sanctioned than their male counterparts (Bonjour *et al.*, 2001; Eardley *et al.*, 2005; Joyce *et al.*, 2006).

Human capital deficits (employment experience, education and training)

Another important area in which sanctioned claimants appear to differ from their non-sanctioned counterparts is their level of employment and educational disadvantage. This is a factor that is explored in many of the US studies in this field, but is less prominent within the (smaller) UK literature.

US evidence suggests sanctioned claimants are more likely to have human capital deficits (characteristics that make them harder to employ) and to be long-term welfare recipients (Pavetti *et al.*, 2003).⁴⁰ A number of studies highlight a lack of work experience and job skills as well as disproportionately low education levels amongst sanctioned claimants (Bryner and Martin, 2005; Fein and Lee, 1999; Goldberg, 2002; Kalil *et al.*, 2002; Lee *et al.*, 2004; Meyers *et al.*, 2006; Wu *et al.*, 2004); for example, a study by GAO (2000) shows that while between 30 and 45 per cent of all TANF recipients had no General Educational Development (GED) or high school diploma this was the case for between 44 to 54 per cent of sanctioned claimants (see Pavetti *et al.*, 2003 and Lee *et al.*, 2004). These differences remained even when researchers controlled for variables such as motivation, attitudes and expectations about work.

UK evidence suggests such differences in education, qualifications and job skills are less prominent among JSA claimants in general (Peters and Joyce, 2006) but identify a difference in the qualification levels of sanctioned and non-sanctioned New Deal customers (68 per cent and 77 per cent respectively reported having qualifications) (p. 18). Similarly, Bonjour *et al.* (2001) found New Deal participants with basic skills needs (literacy or numeracy problems) faced a higher sanction risk.

Other barriers to work

As well as demographic and human capital barriers to employment, some researchers have considered the role more practical and logistic barriers play in selection into sanctions. The US

literature identifies three key practical barriers to work as being associated with higher likelihood of sanctions: problems with transportation (limited or no access to a car and/or not holding a driver's licence⁴¹) (Cherlin *et al.*, 2002; Fein and Lee, 1999; Goldberg, 2002; Kalil *et al.*, 2002 in Pavetti *et al.*, 2003), communication (i.e. not having a home telephone) (Cherlin *et al.*, 2002 in Lee *et al.*, 2004; Oggins and Fleming, 2001) and childcare (which

demonstrates a far less certain association) (see Pavetti *et al.*, 2003 and Meyers *et al.*, 2006).

These practical barriers are less evident in studies conducted outside the US, although Peters and Joyce (2006) highlight the higher proportion of sanctioned claimants who reported experiencing transport difficulties (p. 19). More frequently identified were problems with housing (unstable arrangements and

Table 2. Characteristics identified as more prevalent among sanctioned than non-sanctioned benefit recipients

Study	Location	Characteristics					Barriers to employment		
		African American	Never married	Large family/ household	Young	Young mother	Human capital deficits	Practical	Personal and family challenges
US – TANF									
*Born <i>et al.</i> (1999)	Maryland	ns		X	X	X	X		
Cherlin <i>et al.</i> (2001)	Boston, Chicago and San Antonio			ns			X	X	X
*Edelhoch <i>et al.</i> (2000)	South Carolina	X		X	ns		X		
Fein and Lee (1999)	Delaware	X			X		X	X	
Hasenfeld (2002)	California	ns		X	X		X	X	X
Kalil <i>et al.</i> (2002)	Michigan	X	X				X	X	X
Koralek (2000)	South Carolina	X	X		X		X		
*Mancuso and Lindler (2001)	California			X			X	X	X
*Westra and Routely (2000)	California	X	X				X		
UK									
Bonjour <i>et al.</i> (2001)	UK				X				X (Basic skills) -X (Lower likelihood among disabled claimants)
Goodwin (2008)	UK								X
Peters and Joyce (2006)	UK				X		X (New Deal)		X

* Studies comparing sanctioned and non-sanctioned leavers
 ns – Variables included but not statistically significant
 Source: (adapted from) Pavetti *et al.* (2003): tables 4 and 5

homelessness) and chaotic lifestyles leading to difficulties communicating with the benefits agency and thus to missed appointments (see, for example, Eardley *et al.*, 2005).

Personal and family challenges

'Research is more limited on less easily observed personal and family problems that may contribute to sanction risk by creating barriers to participation' (Meyers *et al.*, 2006, p. 20). However, there is one factor which appears consistently in studies of sanction selection effects conducted in the US and the UK. Health problems and disability (the claimant's own, and those of a dependent) have been identified as a significant factor in a number of US studies comparing sanctioned and non-sanctioned claimants (Cherlin *et al.*, 2001; Meyers *et al.*, 2006). For example, the GAO (2002) found that more than 50 per cent of welfare leavers reporting a health condition had been sanctioned for non-compliance compared with 39 per cent of leavers without health problems. In the UK, a survey conducted with unemployed benefit claimants identified a higher incidence of learning difficulties among those who had been sanctioned (15 per cent compared with 11 per cent of non-sanctioned claimants); this difference was even more pronounced among New Deal participants (22 per cent compared to 14 per cent) (Peters and Joyce, 2006). Similarly, Goodwin (2008) found that those lone parents who had incurred and continued to live with sanctions had higher levels of ill-health (as did their children). Evidence, however, does not always associate disability with a higher sanction risk. For example, Bonjour *et al.* (2001) found that New Deal participants with a registered disability were approximately half as likely to be sanctioned as those with no disability.

A number of other personal barriers have also been associated with higher sanction likelihood, in particular substance misuse (Cherlin *et al.*, 2001; Eardley *et al.*, 2005; Hasenfeld *et al.*, 2002 and Mancuso and Lindler, 2001, in Pavetti *et al.*, 2003), and domestic violence (Lee *et al.*, 2004; Polit *et al.*, 2001). Claimants with multiple barriers of this type were found to be particularly vulnerable to sanctions (Eardley *et al.*, 2005; Polit *et al.*, 2001).

Outcomes for sanctioned claimants

We now turn to look at descriptive accounts of sanction effects; those studies not meeting the methodological inclusion criteria of true impact studies. Consideration of more descriptive studies of outcomes experienced by sanctioned families allow us to draw on a range of evidence from the UK, USA and Australia. The US welfare evidence is presented separately, reflecting differences in both sanction systems and predominant research methodologies.

Personal outcomes

Just as the incidence of sanctions varies across the claimant population, benefit recipients are differently affected by the imposition of penalties (Eardley *et al.*, 2005). Indeed, much of the literature highlights the diversity of claimant experience (Dorsett, 2008), and the greater likelihood of some claimants, such as those with dependents (Employment Service, 2000; Molloy and Ritchie, 2000; O'Connor *et al.*, 1999; Vincent, 1998) and/or no other source of income (e.g. savings, family/partner support) (Eardley *et al.*, 2005; Molloy and Ritchie, 2000; Vincent, 1998), to 'suffer' as a result of being sanctioned (Employment Service, 1999; O'Connor *et al.*, 1999).

Hardship was commonly reported by sanctioned claimants (Peters and Joyce, 2006); often this resulted in difficulties meeting household expenses, especially housing costs (Dorsett, 2008; Joyce *et al.*, 2006; Joyce and Whiting, 2006) and utility bills (Employment Service, 2000; Joyce and Whiting, 2006; O'Connor *et al.*, 1999; Vincent, 1998). In the most severe cases respondents had had utilities disconnected and experienced food hardship (Vincent, 1998). Australian research suggests that between 10 and 20 per cent of claimants lose their accommodation or have to move into cheaper housing as a result of sanctions (Eardley *et al.*, 2005).

As well as financial and material difficulties, qualitative research indicates that sanctions can impact on claimants' emotional well-being (Peters and Joyce, 2006). Feelings of anger, humiliation, depression and anxiety experienced on the imposition of a sanction (Dorsett, 2008; Joyce *et al.*, 2006; Molloy and Ritchie, 2000;

Vincent, 1998) were, in some cases, sustained over a longer period, impacting negatively on claimants' motivation and self-esteem. Moreover, material hardship induced by sanctions created or worsened experiences of stress and anxiety (Goodwin, 2008). While reports of frustration and bitterness were common across claimant groups, the feeling of having been cheated was particularly prevalent among those who had regularly paid National Insurance contributions (Vincent, 1998).

In some cases such emotional impacts led to the worsening of existing health conditions (Dorsett, 2008; Goodwin, 2008; Peters and Joyce, 2006), and even to the development of new health problems (for example, some claimants sought medication for depression following a sanction [Joyce *et al.*, 2006]). An Australian survey indicates that sanctions prompted some claimants (13 per cent) to increase potentially harmful behaviours, such as drinking or drug use (see Eardley *et al.*, 2005).

Coping strategies

Often, the hardship created or threatened by sanctions led claimants to seek means of mitigating such negative outcomes. Family and friends proved regular providers of practical, material and financial support, alleviating the more serious impacts of sanctions (Eardley *et al.*, 2005; Joyce *et al.*, 2006; O'Connor *et al.*, 1999; Peters and Joyce, 2006). Those with savings would often use them to meet living expenses, while others would borrow money from both informal and formal sources. While some claimants applied for a crisis loan or hardship fund money (Joyce *et al.*, 2006), low awareness levels of these types of support and fear of debt meant that this was fairly uncommon (Dorsett, 2008); sanctioned claimants were more likely to seek loans from informal sources, usually relatives or friends (Goodwin, 2008; Joyce *et al.*, 2006; O'Connor *et al.*, 1999; Peters and Joyce, 2006). Claimants recognised that although borrowing money alleviated immediate needs, it also prolonged the effect of the sanction.

Borrowing from or relying on family and friends for support was reported to have negative impacts on their well-being: in some cases those supporting claimants were left struggling financially themselves (Joyce *et al.*, 2006; Peters and Joyce,

2006). This placed additional strain on personal relationships and added to the emotional impact of the sanction (Dorsett, 2008; Peters and Joyce, 2006). A third of respondents involved in an Australian study reported experiencing relationship stress and serious household arguments as a result of the sanction (see Eardley *et al.*, 2005, p. xii).

Some claimants engaged in informal work (e.g. busking) or agency work to see them through the sanction period (O'Connor *et al.*, 1999). It was rare for this work to be declared to jobcentre staff (Joyce *et al.*, 2006). O'Connor *et al.* (1999) also found that some claimants sold possessions to mitigate hardship. Others adopted a strategy of reduced spending; this included cutting back on social activities (Joyce *et al.*, 2006; Peters and Joyce, 2006), treats for children (Joyce and Whiting, 2006) and even household necessities such as food (Dorsett, 2008; Employment Service, 2000; Goodwin, 2008).

A small proportion of respondents felt that the hardship caused by sanctions created a situation where crime became the only response – stealing either food or saleable goods (Eardley *et al.*, 2005; Employment Service, 1999; Joyce *et al.*, 2006; O'Connor *et al.*, 1999; Vincent, 1998). In Eardley *et al.*'s study of sanctioned claimants in Australia 'about one in six reported jumping trains or avoiding paying fares as a result' (2005, p. xii).

The relationship with advisors

One potential unfavourable 'side-effect' of sanction referral is the damage it may cause to the relationship between a claimant and their caseworker – eroding any trust built over time. Despite this hypothesis empirical evidence from the UK suggests that, at least under the current system,⁴² claimants generally separate the imposition of a sanction from the actions of their advisors (Joyce *et al.*, 2006; Peters and Joyce, 2006). However, in some cases resentment may surface if claimants feel they have been coerced by their personal advisor (Dorsett, 2008).

Employment-related and compliance outcomes

Our theoretical discussion of sanction effects pointed to actual changes in behaviour due to sanctions and posited these as 'intermediate

outcomes'. Such behaviour is rarely detailed in administrative data and requires more qualitative approaches. In some such studies claimants, staff and providers were asked directly whether they felt behaviour (participation, compliance and job search) had been influenced by the (prospect of) sanctions.

Findings from UK studies exploring intermediate outcomes indicate that:

- Sanction warnings could lead to contact being re-established with providers (Dorsett, 2008).
- The threat of sanctions 'was instrumental in encouraging and ultimately persuading customers' to comply (Joyce *et al.*, 2005, p. 45); it meant that some demotivated claimants participated in treatments they may previously have refused (or not volunteered for) (Dorsett, 2008; Joyce *et al.*, 2005) and 'from which they derived considerable benefit' (Hasluck, 2002, p. 16).
- Some claimants felt that without compulsion they would not have participated in activities – mandation providing necessary stimulus (Eardley *et al.*, 2005; Employment Service, 2000; Joyce *et al.*, 2005; Molloy and Ritchie, 2000). This was true even of some seemingly very resistant customers (Joyce *et al.*, 2005).
- There was some evidence that claimants 'stepped-up' job search efforts in order to avoid a sanction (Employment Service, 1999; Legard and Ritchie, 1999) or following the penalty (Peters and Joyce, 2006; Vincent, 1998).
- A small proportion of lone parent claimants in receipt of Income Support believed that sanctions may have had a positive effect on their job-search behaviours.

But:

- Not all claimants were motivated to comply with conditions as a result of sanctions; there were those who would have attended without sanctions and those who were unwilling to comply regardless of the consequences

(this group comprised those with an alternative income, those avoiding contact with the jobcentre and those whose health ruled out attendance) (Dorsett, 2008).

- For longer-term, more intensive treatments (such as the New Deal options), there were difficulties sustaining compliance among some claimants – in some cases this led to multiple sanctions being accrued (Joyce *et al.*, 2006).
- Where claimants received multiple sanctions and were resolved not to attend, 'there was a clear sense that no form of sanction would work to influence their behaviour, particularly as the coping mechanisms employed enabled the customer to manage financially without their benefit' (Joyce *et al.*, 2006, p. 57).
- While sanctions could promote attendance they had little impact on engagement; treatments (e.g. Work-focused Interviews [WFIs]) were thought to do little for those who only participated simply to avoid a sanction (Dorsett, 2008).
- Most of those referred felt subsequent attendance did little to alter views about work and those who felt pressurised sometimes developed more negative views about work (Dorsett, 2008) and hostility or resistance towards services (Joyce *et al.*, 2005).
- Staff queried the value in teaching or supporting the unwilling and uncommitted – those who may disrupt other more motivated clients (Joyce *et al.*, 2005).
- Some sanctioned claimants had become cautious of entering employment for fear they should find the job unsuitable, and be penalised (Vincent, 1998; Peters and Joyce, 2006).
- Sanctions led some claimants to sign off (Dorsett, 2008; Joyce *et al.*, 2005), or to move onto another benefit (Joyce *et al.*, 2006), for example from an unemployment benefit to an inactive one (Joyce *et al.*, 2005).

The relationship between hardship and behaviour

A small number of scholars have taken this exploration further and attempted to unpack the relationship between different types of sanction outcome, in particular the effect of hardship on subsequent behaviour. Such research indicates that those claimants experiencing (severe) economic difficulties as a result of a sanction were the most likely to be compliant in future dealings with the benefit system (in this case the New Deal), whereas those ‘cushioned’ by the support of family and friends (usually co-resident parents) could better continue in non-compliance (O’Connor *et al.*, 1999).

However, Bryson *et al.*’s study (2000) shows that, despite higher levels of compliance among sanctioned claimants, those who experienced hardship as a direct result of benefit loss were less likely to be employed by the time of the follow-up survey (see Table 3).

US welfare studies

We know more of the situation of sanctioned welfare recipients in the US, although the body of evidence is still small when compared with other components of welfare reform. Most of this evidence comes from studies that employ cross-sectional and correlation methods – and are therefore unable to establish a causal relationship. This means that it is not possible to say categorically whether it is the sanction that is responsible for the outcome or the family’s characteristics, which also led the family to be sanctioned (Pavetti *et al.*, 2003).

Evidence reveals that sanctioned families face considerable disadvantage on a number of fronts (Bryner and Martin, 2005; Meyers *et al.*, 2006); they are less likely to be employed than other welfare leavers (Pavetti and Bloom, 2001) and have lower earnings (Born *et al.*, 1999).⁴³ They are also more likely to experience utility hardship (21 per cent of sanctioned claimants had their utilities disconnected compared to 9 per cent of non-sanctioned) and more likely to expect hardship in the future (50 per cent of sanctioned compared with 25 per cent of non-sanctioned) (Kalil *et al.*, 2002 in Lee *et al.*, 2004 and Pavetti *et al.*, 2003). Kalil *et al.*’s (2002) study found that a third of sanctioned claimants were engaged in hardship activities⁴⁴ compared with 14 per cent of non-sanctioned (Kalil *et al.*, 2002 in Pavetti *et al.*, 2003).

The experience of hardship appears particularly pronounced when directly comparing individuals who left welfare involuntarily (i.e. were sanctioned off) with those who chose to leave the rolls (Born *et al.*, 1999; Meyers *et al.*, 2006). For example, Lindhorst *et al.* (2000) show that those leaving involuntarily were more likely to go without meals, to have their utilities cut off and to have unmet medical needs (in Lee *et al.*, 2004). Furthermore, those who had been sanctioned for leaving work were more likely to experience financial strain and unstable housing (*ibid.*).

The material hardship sanctions create for claimants leads to difficulties meeting living expenses, and also a number of strategies to ‘manage’ the loss of income and material disadvantage (Meyers *et al.*, 2006; Pavetti *et al.*, 2003). Cherlin *et al.* (2001), for instance, found that sanctioned families were five times more likely

Table 3. Employment rates by sanction status and hardship experience

	No stops/reductions (%)	Benefit stopped/reduced, hardship (%)	Benefit stopped/reduced, no hardship (%)
Paid work	42 (30)	18 (13)	31 (17)
Unemployed	40	67	53
Other	18	15	16
Weighted base	1980	356	132
Unweighted base	1858	343	152

Note: Figures in brackets are full-time employment rates
Source: Bryson *et al.*, 2000, p. 169

than non-sanctioned to borrow money to pay bills. Edelhoch *et al.* (2000) reported that approximately a quarter received income from someone outside the home and Fraker *et al.* (1997) found that 65 per cent of sanctioned clients received support from their parents (in Pavetti *et al.*, 2003). As well as family and friends, sanctioned claimants were also more likely to seek additional support from government assistance programmes, such as food stamps and Medicaid (Meyers *et al.*, 2006). Hardship also led to higher rates of use of emergency services, such as food pantries (25 per cent of sanctioned claimants compared to 19 per cent of non-sanctioned), emergency clothing (25 per cent compared to 15 per cent) and homeless shelters (Cherlin *et al.*, 2001 and Kalil *et al.*, 2002 in Pavetti *et al.*, 2003).

However, there is evidence that unfavourable outcomes may not persist over time. Edelhoch *et al.* (2000), for example, found that almost 50 per cent of sanctioned individuals were employed two years after their case was closed; this compares with only 20 per cent at the point the case was closed. Similarly, Mancuso and Lindler's (2001) qualitative study showed that sanctioned families' resources and general family stability improved over time (Pavetti *et al.*, 2003). It is also important to note, though, that recidivism is more common amongst sanctioned recipients (Meyers *et al.*, 2006). Born *et al.* (1999) found that twice as many sanctioned claimants (35 per cent) returned within 90 days as non-sanctioned (18 per cent); for a quarter of sanctioned families this return occurred within 30 days (p. iv).

Summary of contextual findings

Exploration of contextual evidence offers pertinent insights to be considered alongside impact findings. Of particular relevance are those elements that undermine the way in which sanctions are designed to operate. Low levels of claimant awareness and bias in their implementation mean that sanctions are not operating equitably and cannot meet their own objectives in terms of preventing or punishing (deliberate) non-compliance. However, also within this body of contextual evidence are qualitative studies that suggest the perception of sanctioned claimants as being wilfully non-compliant underlying punitive

sanctions is predominantly a false one; rather, the breaching of conditions appears to be a product of personal, social and practical circumstances. Such evidence relating to the characteristics of the sanctioned populations and the unfavourable outcomes they often encounter also raises important questions about whether sanctions merely compound existing inequalities and create further barriers to work for some claimants.

5 Sanctions and other types of conditionality

Conditionality and sanctions have not been confined to unemployment and social assistance benefits, although this is certainly where they are most visible. Some important developments have taken place in the UK in which other kinds of benefit payment have been made conditional on certain behaviours (Deacon, 2004; Dwyer, 2004; Griggs and Bennett, 2009; Stanley, 2005). These developments, although regularly proposed, have not always been easy to put into effect. Indeed, they have often been met with considerable resistance from both within and beyond Parliament. Perhaps the best example lies in the repeated attempts the government has made to introduce housing benefit sanctions for those guilty of anti-social behaviour. This has been proposed in numerous iterations, often demonstrating a compromise, and most being met with varying degrees of dissent; it now operates as a pilot programme affecting those claimants who have previously been evicted for anti-social behaviour and refuse to take up available support.⁴⁵ Despite such set-backs:

The initial enthusiasm with which the Prime Minister and many of his inner circle embraced ... [such] proposals does, however, illustrate ... the extent to which a principle of conditionality now informs much social security policy.

(Dwyer, 2004, p. 272)

While the UK has taken steps towards increasing conditionality outside the social security benefit system, the US has gone even further. These include explicit objectives to reduce non-marital births (the family cap), with financial incentives for those states successfully reducing extra-marital births. There are strictly enforced rules regarding paternity establishment, in some states as a precondition of benefit receipt (Stanley, 2005). Otherwise the largest use of conditionality across the world has been in the development of

CCTs in developing countries where entitlement to child-related benefits is conditional on up-take of immunisation, regular health checks and on school enrolment and attendance.

The following chapter brings together some of the main messages from studies exploring the impact of non-work-related sanctions and conditionality, organised on the basis of policy type. Evidence varies a great deal in terms of both quality and coverage, but we have prioritised studies employing more robust methods, including the results of experimental and observational evidence where available. While the following is not intended to provide a comprehensive account of all we know about conditionality and sanctions outside work(-related) programmes, it is able to offer a good indication of the potential of sanctioned-backed conditionality in different policy fields.

US studies on immunisation and health

Parental responsibility requirements refer specifically to mandatory benefit conditions for children's health care. There are two such programmes included in Grogger and Karoly's meta-analysis, the PPI (Maryland), which required families with infants to prove that their children had received preventative health care (this included immunisation), and the PIP (Georgia), which required parents to regularly verify their children's immunisation status. While the first of these programmes showed no significant treatment-control differences, children in PIP's treatment group were significantly more likely to be up to date with their vaccinations (for example, four years after being randomly assigned, 87.5 per cent of treatment children had up-to-date polio vaccines compared with 80.1 per cent of the control). One reason posited for the difference between these two programmes is the larger sanction imposed as part of the PIP programme (equivalent to the non-vaccinated child's grant). It is also possible that the simpler objectives

and narrower requirements contributed to PIP's success (Grogger and Karoly, 2005, p. 222).

US evidence on school attendance

Wisconsin's Learnfare programme, implemented in 1988, can be described as a 'conditional cash penalty' in that it penalises teenage welfare recipients (or their parents) for not meeting school attendance targets (specifically, a maximum of two unexcused absences in a given month) (Dee, 2009). The results of Dee's evaluation (2009) show a positive impact on school enrolment (a 3.7 per cent increase, effect size = 0.08) and attendance (4.5 per cent, effect size = 0.10). Effects were larger among subgroups at risk of dropping out of school. Results suggest that well-designed financial incentives (or sanctions) can be an effective mechanism for improving the school attendance of at-risk students at scale.

The similar Learning, Earning and Parenting (LEAP) programme was also experimentally evaluated. This programme required pregnant and parenting teenagers who had not graduated or attained a General Educational Development (GED) qualification to attend school (or its equivalent). Those who attended were awarded with an additional welfare payment (\$62) while those who failed to attend faced a sanction (\$62) from their monthly grant (typically around \$274). Participants were also assigned caseworkers who provided support and advice, assisting with barriers to participation such as childcare and transport. Evaluators found impacts were largely dependent on whether participants were attending school or a GED programme when they entered LEAP (see Auspos *et al.*, 1999). Those who had been attending school showed favourable educational outcomes (66 per cent of the treatment group completed school, gained a GED or were still at school compared with 57 per cent of the control group [Long *et al.*, 1996]) and employment impacts (four years after enrolment 65 per cent treatment and 60 per cent control were employed [Bos and Fellerath, 1997]). Favourable impacts on earnings noted during the first two years did not extend into the second – with control-group members 'catching up' with their treatment counterparts. Those in the treatment group who had not been enrolled

in school at baseline showed less favourable outcomes when compared with their control group counterparts. These treatment-group members experienced a high sanction rate, and those with multiple sanctions reported resulting hardship for themselves and their families, in some cases lacking essentials such as food and medicines (Bos and Fellerath, 1997 in Auspos *et al.*, 1999).

US studies exploring the impact of the family cap

A small body of US evaluations (experimental and observational) have considered the effect of the 'family cap' (a grant reduction imposed when new babies are born to families receiving welfare) on childbearing. Among these are two experiments, AWWDP and FDP, designed explicitly to test the impact of the family cap (see Grogger and Karoly, 2005, pp. 189–90). The first of these, AWWDP, which took place in Arkansas, found no significant effect on childbearing, use of family planning services or birth control. The second study, which evaluated the New Jersey Family Development Program (FDP), found a statistically significant 3.2 percentage point decline in additional births (a 9.2 per cent drop) as well as a 4 percentage point increase in the use of family planning services, but no corresponding effects on abortion rates. However, methodological problems with both studies, such as small samples (less than 100 claimants in the AWWDP), difficulties with randomisation (FDP) and low awareness of policies among recipients (AWWDP and FDP), suggest caution should be exercised if generalising from these findings (Grogger and Karoly, 2005).

Four observational studies also explored the impact of the family cap on childbearing (Horvath-Rose and Peters, 2002; Kaushal and Kaestner, 2001; Kearney, 2002; Levine, 2002 – all in Grogger and Karoly, 2005). Of these studies only two, Levine (2002) and Horvath-Rose and Peters (2002), identified a statistically significant impact on childbearing. Yet these two results work in different directions, Levine showing a 5.3 per cent increase in births and Horvath-Rose and Peters a 7.6 per cent decline (among unmarried teenagers). Reconciling these results is rendered particularly difficult by the methodological problems experienced by both

the experimental and the observational studies. This leads Klerman (see Grogger and Karoly, 2005, p. 197) to conclude that family caps have had no significant effect on childbearing.

UK evidence on community sentences

In 2001 the UK introduced the community sentence sanction policy. Under this scheme, offenders failing to meet the conditions of community punishment orders, community rehabilitation orders and combination orders were subject to social security benefit sanctions. The aim of the programme was to promote compliance and a wider sense of responsibility to society by linking behaviour to the receipt of minimum income benefits (Knight *et al.*, 2003).

The policy was implemented in four pilot areas and evaluated using mixed methods with the aim of assessing its impact on compliance and issues relevant to a national roll-out. By comparing the change in compliance rates among claimants and non-claimants, the evaluation was able to identify a small, but positive, effect on compliance (a 1.8 per cent increase) (Knight *et al.*, 2003). Interviews with offenders, however, suggest that they did not believe the policy to have had a significant effect on their behaviour. Although some were encouraged to pay extra attention to the conditions of community orders, effectiveness was hampered by low awareness levels, and had less influence where offenders faced greater barriers to compliance (e.g. substance misuse). Sanctions did, however, create financial difficulties for all those affected and led some claimants who were already re-offending to increase criminal activity. There was little evidence of an effect on labour market behaviour (*ibid.*).

Interestingly, despite demonstrating small positive impacts, the pilots were terminated in 2009 following an evaluation that 'showed the pilots did not provide sufficient value for money to justify their continuation' (Hansard, 2009).

Conditional Cash Transfers

There are now a number of CCT schemes operating in Latin America (where they originated) and further afield. Two of the most significant are the Bolsa Familia⁴⁶ (or 'family stipend'), which has been operating in Brazil since 2003 (ILO, 2009b,

p. 42) and Oportunidades (formally Progresá) the first CCT, launched in Mexico in 1997, now the Mexican Government's principal anti-poverty programme. Although each individual scheme has both its own objectives and set of conditions attached to receipt, all broadly aim to address poverty (the benefit is targeted at low-income households) and opportunity through 'human development' requirements. Thus conditions are often attached to school attendance and health-related behaviours such as immunisation, and (for mothers) pre-natal care. If these conditions are not met then the grant is stopped (approximately 7 per cent of Bolsa Familia families have had their benefits stopped while compliance is investigated).

While evaluations of these schemes do not consider the specific effects of grant loss, wider assessments focusing on the receipt of cash transfers demonstrate generally positive impacts. Evaluations of Oportunidades and Bolsa Familia, for example, show a significant effect on school enrolment and attendance rates (ILO, 2009b, p. 43). Child health has also improved, with evidence that Oportunidades has had an impact on nutrition, child growth and prevented stunting; similar positive impacts were found in an evaluation of the Colombian Familias en Acción programme. Moreover, CCTs also appear to have had a positive effect on adult health.

Favourable impacts can also be identified in terms of their anti-poverty objectives. Average consumption has risen, with evaluations indicating that the bulk of increased spending has been on food and providing for children's health, education and other needs. Furthermore, concerns that the grant would create a work disincentive have not been realised; indeed programmes like Bolsa Familia appear to have had a positive impact on labour market participation rates (ILO, 2009b, p. 44).

However, the long-term benefits and broader outcomes of CCTs have been questioned. The recent World Bank review by Fiszbein and Schady (2009) looked at evidence from a large pool of CCT programmes where children's school attendance is required of parents receiving cash transfers. The clear result from these programmes was that school attendance rates rose as participants complied with the conditional nature of the

programme, but there was no measurable increase in educational attainment. There are a number of reasons asserted to explain this result, although none are conclusive. Overall it seems children from poor families (many of whom resided in rural areas with poor-quality schools) suffer many educational disadvantages – school attendance is just one of them. CCT programmes also had demonstrable positive impacts on income and poverty, but these effects may have been greater had the cash transfer been unconditional. The UK Department for International Development (DIFD) has argued that ‘cash transfers do not need to be made conditional on school attendance to impact on children’s education’ (2005, p. 14, cited in ILO, 2009a, p. 51). Furthermore, suspending or terminating a family’s grant for failing to meet conditions will do little to assist those in the greatest need, particularly children, who cannot be held accountable for their parent’s transgressions.

Substance misuse programmes

Systematic reviews of evidence in this area tend to show the potential benefits of incentives and of treatment monitoring; however, there is little evidence on the relationship between programmes for substance users and benefit sanctions. Part of the CalWORKs evaluation does, however, begin to explore the association. Overall, researchers found sanction rates among claimants referred to substance support services were low (4.7 per cent after the referral) (although they were far higher over the whole four-year evaluation period – 27 per cent). Those claimants dropping out of supportive services were more likely to be sanctioned than ‘completers’ – thus the same factors appeared to contribute to non-compliance across different programmes. Completers had, to a large extent, attained and sustained abstinence, and services appeared to have positive effects on claimants’ attitudes towards work, as well as their ability to undertake it. According to providers, 60 per cent of claimants had a positive change in their capacity to look for, find and retain work – this appeared to be associated to the amount of time the claimant had been receiving services (and very significantly, whether the programme had been completed) (California Institute for Mental Health, 2005).

These findings take on greater significance given the changes set out in the Welfare Reform Act 2009. Schedule 3 details requirements for problem drug and alcohol users to make and follow a rehabilitation plan and in some instances to undergo drug testing (OPSI, 2009). Sanctions of up to 26 weeks can apply to those claimants failing to attend interviews or substance-related assessments, answer questions about drug use or follow the mandatory rehabilitation plan (without good cause). What little evidence can be applied to these proposals raises concern about the lack of incentives and focus on mandation backed by punitive measures.

6 Justifications for sanction-backed conditionality

The majority of commentary on benefit sanctions and conditionality has focused on underlying issues of moral philosophy – largely, contractualism, communitarianism and paternalism. Critics have questioned the viability and fairness of such approaches, often from a social rights perspective. Our review comes from a different perspective, a rigorous analysis of the evidence base on the outcomes of sanctions in practice, based on a clear set of theoretical a priori assumptions on the nature and basis of such evidence. To extend our review into the realms of moral philosophy would be difficult and inappropriate, and therefore this section of the review considers only those justifications of sanctions that can be informed by evidence. This means we focus on whether measurable aims of policies couched in terms such as ‘equity, effectiveness and efficiency’ have supporting evidence to underlie them.

Equity-based justifications?

One justification for sanctions is that the underlying mandatory approach they enforce widens opportunity for claimants. When compared with voluntary programmes (in which sanctions would be a non sequitur), mandation ensures that people cannot select themselves out, and thus those who are furthest from the labour market, facing multiple barriers to work (those who are more likely to opt out [Hasluck and Green, 2007]), are ensured access to programmes. As a quid pro quo, this ensures that the most advantaged jobseekers do not disproportionately capture available services (Deacon, 1997). There are, however, several preconditions that must be in place to ensure that such equality of opportunity is enforced and complemented by sanctions.

The first condition is that the approach does not alter the probability of being on the underlying programme, or, if it does, that this operates equally across the profile of claimants. The effect

of sanctions on inflow or take-up has been under-considered to date, but the combination of reduced take-up and increased exits to being neither ‘in work’ nor on benefit has been observed consistently over several wider studies and commentaries, which clearly suggests a fall in take-up. US welfare reform, including the rise in full-family sanctions, has led to growing numbers of what Rebecca Blank calls the ‘disconnected’ (those not in work or on welfare) (see Blank, 2007). In the UK, exits from unemployment to ‘unknown destinations’ are high both in unemployment and incapacity benefit programmes (Blyth, 2006). Considering the introduction of JSA, Petrongolo observes ‘those who left the unemployment register without finding a job might have in general become detached from the labor market’ (2009, p. 26).

A second condition, which ensures equality of opportunity, requires that programme (and treatment) quality is consistent, in both implementation and delivery. This is known to be problematic, for example, as voluntary programmes become mandatory the number of claimants expands and the relationship between provider and client changes. Mandatory programmes with sanctions also require high levels of information quality (Griggs and Bennett, 2009; SSAC, 2006) and, to date, this has been consistently absent from UK benefit programmes with sanctions (see our discussion in Chapter 4).

A third condition is that sanction risk is equal across the claimant population. Again, the evidence reviewed in Chapter 4 suggests that those for whom there is the greatest concern about equality of opportunity (the lowest skilled and those furthest from the labour market) are disproportionately selected into sanctions.

It is thus wise to conclude that while sanctions may enforce increased coverage of treatment, this should not in itself be seen as equality of opportunity for such treatments. Studies to date have concentrated too narrowly on crude treatment

rates of caseloads and on narrow and short-term definitions of exit. Widening the approach to include the impacts of sanctions on take-up and of exits to reduced (or consistent) opportunity sets would provide a fuller profile of inequality.

Policies that promote sanctions linked to labour market participation also tend to see entry into the labour market as an outcome that increases equality of opportunity. It is certainly the case that changing someone's behaviour from 'not seeking' to 'seeking' work will raise their probability of finding work from zero. However, once this is done, there is a significant group for whom the probabilities of employment are not significantly above zero. This group gets bigger the wider the conditional rules of being 'available for work' are set (for example, the movement of lone parents with older children to JSA). There is also a group for whom the probabilities of work are reasonable, but the employment profile they face when in work is very poor (jobs being low paid and the work conditions substandard) – which means a higher likelihood of cycling between work and unemployment. Sanctions that result in lower earnings in work, in decreased employment persistence and increased churning in employment (such as those seen in Arni *et al.*'s study) are more difficult to argue for in terms of increasing opportunity.

Lowering sanction incidence to encourage better job matching and delayed labour market entry (associated with human capital development and ultimately to better job matches, higher wages and work retention [Ochel, 2004]) requires better consideration and the development of a broader evidence base. The general message is that equality of opportunity could, in theory, be increased by sanctions, but this relies on the absence of selection effects and the ability to equate enforced participation with a higher level of outcome. The evidence to support both these necessary conditions is extremely weak indeed.

We now begin to consider equity in a different form – looking to issues that justify sanctions in terms of **equality of outcomes**. It should first be noted that the evidence base for such justifications is extremely small. Evidence largely concentrates on employment impacts and 'entitlement' effects and thus on short-term impacts. However, it could be argued that the things that really matter for

longer-term outcomes (in terms of self-sufficiency, freedom from poverty, child outcomes, etc.) concern the quality of employment and sustained income gains (the areas with very little robust evidence). Indeed, what evidence there is of longer-term sanction impacts is largely unfavourable – sanctions lowering the likelihood of sustainable employment and incomes over time (Arni *et al.*, 2009). Furthermore, favourable short-term effects may offset longer-term impacts; US welfare reform evidence shows any employment gains as a result of mandatory work requirements that were not accompanied by gains in income actually had a detrimental impact on children's educational outcomes in high school, suggesting the potential to limit opportunity in the next generation (see Grogger and Karoly, 2005, pp. 215–16).

Greater income equality or decreased incidence of poverty as outcomes from sanction-enforced benefit conditionality would certainly arise from higher employment rates if such employment was sustained and provided incomes that were above the poverty line. The problem is that much of the evidence base does not concern itself with the 'losers', either those exiting to destinations other than formal employment or those who are deterred from take-up. Part of the evidential problem is that those not in the system cannot appear in the results; if one of the effects of the programme is to lower enrolment then there are huge difficulties taking this into account. In effect, the evidence base overstates gains without the ability to fully account for losses.

This is accompanied by a general failure to decompose treatment effects. While we know something of 'aggregate' and 'average' gains, some of this evidence, particularly of earnings gains over time, is very difficult to interpret. The recent study by Arni *et al.* (2009) that starts to unpack the effects of sanctions on different outcomes is a significant addition to the literature. It also helps us to understand why so many of the US studies see the control group catching up with the treatment group over time: the short-term gains from treatments are mainly through reduced periods on benefits rather than improved wage levels. Much of why sanctions 'work' is to encourage an earlier return to employment, but at the expense of the quality of the job match. This has clear anticipated outcomes

for longer-term issues of equality of outcome that are borne out in the discussion above.

And what of those who are not seen to be in employment? Blank's US-based work on the 'disconnected' indicates that 75 per cent of this group are living below the poverty line (Blank, 2007). The growth of the 'disconnected' can be considered a by-product of welfare reform such as sanctions and time limits (and thus of entitlement effects) – forcing premature exit and deterring entry; this is the other side of the caseload reduction coin. How far such 'exclusion' can be considered as furthering equality of opportunity is an empirical question of huge importance and requires urgent specification and measurement. How far have profiles of poverty gaps and poverty intensity changed alongside poverty headcounts and how far have the 'disconnected' been part of changing profiles of such poverty measures?

Efficiency-based justifications

It is argued that sanction-backed conditionality is efficient; that these approaches are best able to use available resources to maximise positive outcomes by ensuring claimants are better informed and realistic about opportunities (managing, in most cases lowering, expectations and reservation wages), that job search is 'optimised' and that moral hazard type risks are minimised.

One suggestion is that sanctions can help conditional approaches solve a variety of 'information problems' at the individual level. For instance, the evaluation of work-focused interviews for lone parents clearly showed that providing information on employment opportunities and the various government initiatives available to assist a move into work improved take-up of the New Deal for Lone Parents and increased employment (Knight *et al.*, 2006). Of course, such potential gains to aggregate efficiency must be put against the underlying problem of system complexity and the poor quality of programme information. And, as an alternative, it may be more efficient to provide and communicate clear information about simple and transparent programmes. But within a complex system there are potential gains to be made through investment in mandatory methods to

ensure that improved information is shared across the widest profile of programme participants.

There are other, more difficult, issues concerning imperfect claimant knowledge – for instance, in the argument for making CCTs conditional on school enrolment and attendance is the perception that parental investment in children's human capital is too low, as parents are unaware of the returns to education (Fiszbein and Schady, 2009). Put more simply, parents (themselves poorly educated) may not sufficiently value investment in the education of their children (Attanasio and Kaufmann, 2009; Jensen, 2006, for instance); therefore making this 'investment' a precondition of benefit efficiently corrects the problem this informational deficit creates. However, given what evaluative evidence tells us about the long-term returns on school attendance it is difficult to establish how far the parents in CCT programmes were actually making poor decisions about their children's education.

It is thus possible to overstate information problems and understate the rational reaction to short-term risks – especially in terms of risk appreciation and risk aversion of low-skilled claimants. Information may be part of the problem but encouraging risk-averse individuals to make behavioural changes that are posited in short-term returns (being 'better off in work' at the job entry point of transition) may not be sufficiently robust in the face of evidence of smaller medium- to long-term gains from employment, income security and job opportunities. If sanctions are only enforcing short-term outcomes, it makes little sense to talk of the longer term – of earnings progression, of sustained employment and better returns to human capital – unless such longer-term returns are transparent and realisable. This point takes us back to the problems discussed under equality of opportunity and outcome above.

A second way of appreciating efficiency is in the micro-economics of job search. Here sanctions will, in theory, reduce employment expectations and reservation wages and thus increase efficiency in job search and a quicker return to employment (as outlined in Chapter 2). However, there are obvious problems in applying this logic to all unemployed.

If sanctions are imposed too early the gains will be minimal since many claimants will be short-term unemployed and thus likely to leave benefits quickly without intervention (moreover experimental evidence shows imposing conditions too early has a negative impact on earnings [Klepinger *et al.*, 2002]). Delaying treatments until an elapsed period of unemployment has occurred has typically been the British approach, the Restart evaluation being hugely influential in supporting more intense monitoring of, and job search advice to, the long-term unemployed (Dolton and O'Neill, 1996). The role of sanctions in such approaches and the trade-offs between early returns to employment versus longer periods to achieve a better job match have not featured in the vast majority of studies in this field. There is evidence that sanction effects (both threat and imposition) can reduce periods on benefit (and thus caseloads) and increase job entry – this is a compelling evidence base. However, the measures are only short-term and very narrow measures of efficiency.

Arni *et al.* (2009) is the only study to question this approach by looking at other measures – finding negative effects of sanctions on earnings and job sustainability. More crucially, these authors also address the underlying question of efficiency in the approach to short-term returns to employment:

There is a trade-off between the positive effect of finding a job sooner rather than collecting unemployment benefits for a longer period of time, and the negative effect of finding a less well-paid job with a shorter duration. Using our estimation results we are able to quantify this trade-off. We show that over a period of two years following the exit from unemployment, the net effect of benefit sanctions is negative. For sanctioned workers, the loss in earnings is in the order of two months whereas the gain from shorter unemployment duration is about one month. (Ibid., p. 33)

If these findings can be replicated in other studies then sanctions cannot be seen as efficient in such terms.

A specific argument for efficiency arises from the design of unemployment insurance and the perceived requirement to have sanctions

(disqualifications) for those who leave work voluntarily, are dismissed for misconduct or leave without good reason. Insurance theory suggests that the risk event must not be subject to individual choice. This may have made sense in 1948 when jobs were long-term, unionised and unemployment levels were low and benefits represented a significant proportion of average wages. But all these underlying facts are now false, and we are left in 2010 with a theoretical justification that may well not be efficient.

The facts underlying the reason for each job exit take huge amounts of administrative time to establish and impose a burden on staff that could well be more efficiently placed in job advice and brokering. Unemployment is increasingly a repeated event for that segment of the workforce who are low paid and low-skilled. These are not the people it is sensible to suspend from unemployment benefits for long periods as they have recent job experience and could well be re-employed in better positions to reduce the risk of future job loss. The perceived 'financial incentives' of benefits have also fallen, with current JSA replacement rates of around just 40 per cent (Griggs and Bennett, 2009) (although passported benefits for housing etc. are potentially of more concern).⁴⁷ And for those who have long work records and a contribution record, disqualification for those who 'choose' voluntary redundancy is a perverted interpretation of choice for the majority. They have paid into a system that provides them with no coverage. Voluntary redundancy is one of a very limited set of options that are best seen as decisions based on 'when' and not 'if' their job ends, together with the different value of financial packages that will accompany such different versions of redundancy. In the early 1980s, the Conservative Government suspended the operation of the moral hazard rule for voluntary redundancy in the face of large-scale restructuring of the economy. Its operation now, while we are still feeling the effects of the deepest recession since the 1920s, is questionable for the same reasons – scale, consequences, efficiency and effectiveness. There is a clear evidential challenge to prove the efficiency and effectiveness of the continued operation of the moral hazard rules for sanctions of JSA as they currently stand.

There is one key measure of efficiency where sanctions appear attractive. They are a relatively cheap way of reducing caseload and thus of reducing programme expenditure. Combined with the highly valued increase in immediate employment rates for those claiming unemployment benefits, this appreciation of efficiency dominates the evidence base. The evidence is compelling: if policy-makers want short-term savings to programme budgets with a sellable 'outcome' attached then ratcheting up conditionality and applying more sanctions will 'save money' and have measurable outcomes. In much of the literature there is reference to the assumption that 'behaviour-based interventions' are 'more (cost-) effective than traditional service delivery and ... deliver wider social benefits' (Stanley, 2005, p. 38). However, a more considered appraisal of efficiency would have to take several additional issues into account.

First, savings in one programme does not mean that all such savings can be claimed across the whole government profile of expenditure. There will be consequential spending on other programmes – for example, working tax credits. There will also be additional tax revenue to consider – taxes paid by those in work. Second, there is the issue of displaced spending that results in other programmes – more unforeseen or unintended spending outcomes. Some of these may result from spill-over effects or externalities. For example, Machin and Marie's (2004) study of crime rates points to one area where a direct causal relationship to sanction rates has been found and estimates made that 22–28 per cent of overall benefit savings from the introduction of JSA could be netted off due to spending on policing and criminal justice. Similarly, evidence from the US of higher hospital admissions for children of sanctioned claimants points to other areas (in this case acute and primary health services) where displaced spending may occur.

However, in the main policy-makers can calculate savings and gains from increased rates of employment based on fairly simple assumptions using tax benefit models. The difficulty lies in making such estimates dynamic and reflective of the risk of repeated unemployment, and, as more sanctions and conditionality operate on people

with health problems and with young children who are entering part-time work with poor prospects of sustained employment, the more those conditions will be problematic. Raising the employment rate assumption depends on knowing what the combined effect of inflows and outflows are. These are largely unknown and probably over-optimistic given that the policy objective is to achieve a statically specified 80 per cent employment rate irrespective of turnover. (Achieving an 80 per cent rate with high turnover may need a combined inflow and persistent employed assumption of 85 or 90 per cent as being in work or achieving job entry – an unheard of and internationally unseen level.)

There are other longer-term effects that may lead to a general worsening of employment conditions for a significant section of the population. When tight conditionality and high sanction rates operate on a large scale and affect the majority of claimants there is a danger that these will impact the informal economy. Indeed, van den Berg *et al.* (2004) point out that sanctions may well change the relative value of informal versus formal job search. For those exiting the benefit system or choosing not to enter then the attractiveness of the grey economy is increased. Sanctions are seen by some as ways of preventing such activity on benefits, weeding out those who are feckless or who are claiming fraudulently (for instance, see Rayner *et al.*, 2000, p. 7). However, the relationship between the informal economy and benefits is a complex one. In countries with little formal social assistance, low productive informal work is the norm as a way of achieving a subsistence income. The self-employed who work for low wages, often below the minimum wage level, are the UK equivalent of such a workforce. It is rare for such economic activity to provide lifetime employment and even rarer for it to get to a level where it can be formalised and taxed appropriately. If the benefit system is changed to stimulate the provision of such services then there is a considerable likelihood of significant 'general equilibrium' effects at the lower end of the labour market. Such outcomes are the unseen side of US welfare and public benefit reform since the late 1980s.

A final problem comes in establishing the overall position of efficiency to include those who 'disconnect'. Those who are off the programme

rolls and therefore not in receipt of welfare benefits can potentially be considered an unequivocal saving to the public purse. Indeed, if they really disconnect then they will additionally be absent from government survey responses, and may well not vote, which raises interesting questions about how far this group are recognised in policy.

Effectiveness-based justifications: optimal models for changing behaviour

'The primary purpose of ... welfare conditionality is not to determine entitlement or to establish need, but to change behaviour' (Deacon, 2004, p. 912). Sanctions are designed to encourage compliance with, or participation in, activities deemed to be in the best interests of claimants (and ultimately for society [Bastagli, 2008]). They can discourage certain behaviours or punish those who behave inappropriately (Griggs and Bennett, 2009; Halpern *et al.*, 2004; Lister, 2008) and to help 'determine motivation, shape action and thereby determine character' (Field, 1999). Approaches that promote such 'behavioural engineering' have found growing legitimacy and acceptance within active labour market programmes, and have been the subject of government-sponsored research (see, for example, Halpern *et al.*, 2004).

There is a prerequisite for sanctions intended to alter behaviour, 'the application of sanctions must be efficient, transparent and fair if it is to be linked to changes in the behaviour and attitude of clients' (WorkDirections, 2008, p. 3). Research discussed in Chapter 4 shows clear evidence of consistent and widespread problems with information and claimant awareness and this means that sanctions rarely function as intended, either in terms of threat (claimants being motivated by fear of a potential sanction) or as a punishment for wilful non-compliance (*ibid.*). A considerable proportion of claimants do not fully understand how sanctions are incurred and very few make an active choice not to meet the conditions of receipt (Goodwin, 2008). Many are not aware that they have been sanctioned even after the reduction has been made. Substantial time-lags exist between the infraction (act of non-compliance) and the punishment (the imposition of the sanction), and

can limit claimants' perception of cause and effect (*ibid.*). Such disassociation of breach and sanction can serve only to dilute the message and potentially reduce more favourable impacts.

Additionally, descriptive statistics suggest that only a minority of the sanctions imposed reflect a clear behavioural aspect. The majority of sanctions (55 per cent in the UK) are retrospective moral hazard sanctions (i.e. claimants are deemed to be responsible for their own unemployment, either because they left work voluntarily or because they were sacked for misconduct). There is also a high prevalence of administrative sanctions, imposed for 'infractions' such as failure to provide necessary paperwork (see Chapter 2).

There are also wider questions about the effectiveness of sanctions compared with rewards (sticks versus carrots). The psychological literature (a summary of which is included in Appendix 3) strongly suggests that rewards produce better longer-term behavioural outcomes than negative punishments – this includes higher levels of take-up and improved attitudes to work. Evidence on outcomes suggests that although some claimants are motivated to comply or participate as a result of sanctions, this has a positive effect on motivation and behaviour in only a minority of cases (see Chapter 4).

The evidence of what works when attempting to promote and reinforce desired behaviour, as an alternative to benefit sanctions, is very limited indeed apart from the use of positive financial incentives, which are based on rewarding work of a particular type and duration. A Job Preparation Premium was part of one trial in the Pathways to Work programme, but no separate impact study was conducted; however, qualitative research with receipts indicated modest positive outcomes (Nice *et al.*, 2008). Of course, the move to positive reward-based incentives as an alternative to sanctions alters the polarity in potential selection effects – with the ability of the most job-ready and educated to capture them, a real problem for the design and implementation of such an approach. How could rewards be targeted towards those who are currently at risk of sanctions and what difference would such an approach make? These are questions that future research needs to address.

7 Conclusion and recommendations

This review was commissioned to explore the evidence base for benefit sanctions and to critically assess their justifications. The latter is the more difficult task because the mismatch between evidence and policy justifications (particularly those cosseted in moral philosophy) is stark. These justifications are often presented as certainty, but the evidence base is very mixed – with significant evidence gaps around longer-term impacts (on earnings for example) and a comparatively large body of evidence on more narrowly defined short-term entitlement effects such as exits from unemployment. The presence of strong selection effects and administrative inconsistency all contribute to reservations about the broader effectiveness (and therefore true justifiability) of sanction-based systems (Bryner and Martin, 2005).

Thus this report brings into focus the gulf between the rhetoric of welfare reform and the evidence of the effects of sanctions. The gulf is not just one of evidence; it comes about by also trying to cross a divide in approaches that seek to prevent poverty and promote opportunity in the UK with the reality of the effects of sanction-backed conditional benefits. It is easy to see why the evidence of effects in terms of reduced programme caseloads led to such a strongly fought debate in the US. This debate was, however, dominated by those who were opposed to state social assistance for people who are out of work on any scale. Unemployment assistance in the US (state-based schemes in the main) was retracted in the late 1980s and early 1990s, leaving two-thirds of unemployed people with no system to help them. Lone parents in particular were targeted for reduced provision, and the large evidence base from those reforms has dominated our thinking about the problem – without adequate reflection on the quality and coverage of evidence and without a systematic appreciation of what effects to expect or how to measure them.

The UK, on the other hand, has committed itself to reducing and, ultimately, eliminating child

poverty, as well as to evidence-based policy-making. These policy commitments suggest a different approach to policy on sanctions as an element of ‘welfare reform’, one that better balances evidence of their efficacy and effects alongside more rhetorical approaches based on the moral philosophy of rights and responsibilities.

However, to date there is little indication that this more balanced approach is being achieved; indeed, policy-makers continue to justify the extension of sanctions (and sanction-backed conditionality) on moral philosophy grounds while taking an ambivalent attitude to the evidence. This ambivalence can be identified in policy (green and white) papers, with evidence being marginalised by discussion of principles and what can be expected of claimants in return for benefits.

This is true of both major political parties. Indeed Conservative welfare reform proposals are strikingly (if not openly) similar to those of New Labour, as is the language, being heavily contractualist, for example ‘the payment of unemployment benefit by the state is an entitlement which is earned, not owed. In return for proper support and intervention for those who require it, the taxpayer should expect people to follow through on their side of the bargain’ (Conservative Party, 2009, p. 12). The 2009 Conservative policy paper, *Get Britain Working*, also suggests further extensions of conditionality, backed by sanctions, including a mandatory community work scheme for the long-term unemployed (Conservative Party, 2009). The preceding policy green paper, *Work for Welfare*, highlights the Conservatives’ overt intention to employ conditionality and sanctions to lower caseloads ‘we expect to see an initial, one-off reduction in the number of out of work benefit claimants as a result of the introduction of tougher sanctions⁴⁸ and conditions’ (Conservative Party, 2008, p. 40).

Recent proposals for reform have openly discussed the need for a new approach to

sanctions (Gregg, 2008). These proposals are problematic in that they are based on the premise that sanction policies will work because all information on them will be perfect so that universal understanding and compliance are finally optimisable. New forms of sanction that penalise time rather than income will be considered. But this does not address current problems with implementation or with the equivocal impacts we have detailed in this review. The evidence suggests sanctions have relatively little to do with changing behaviour in practice, the majority punishing those who left work for ‘unacceptable’ reasons, or who failed to meet administrative requirements. Few act to remedy ‘bad behaviour’. That means that the future emphasis must be on the deterrent effect of sanctions. However, this is precisely the area where we have the least evidence and what does exist is largely inconclusive on current approaches; there is nothing on which to base the proposed future system. Likewise, given what we know regarding current problems with information and communication (discussed in Chapter 4), it is difficult to envisage a system without these (seemingly inherent) difficulties.

But it is wrong to be dismissive of such proposals because they are an ‘alternative’ to the approach advocated by those pushing for further ratcheting up of sanctions. Perhaps this is where our evidence review can make the more important impact, because the arguments for such radically increased conditionality and sanction severity cannot be reconciled with emerging evidence that sanctions do not operate in people’s best interests in the longer term.

The review leads us to make a number of recommendations:

- 1 To replicate the Arni *et al.* (2009) study in the UK – to test the effect of sanctions on earnings and sustainability of work;
- 2 To put in place better, more wide-ranging cost-benefit studies of conditionality and sanctions that look at displacement and externalities;
- 3 To look more closely at the (potential) spill-over effects of conditionality and sanctions on the grey economy and informal work;
- 4 To ensure longitudinal datasets can capture the changes to conditionality that have already been put in place, for example by ensuring that the next wave of the Millennium Birth Cohort Survey has a module of questions to assess the effects of the new benefit conditions on lone parents according to their youngest child’s age.

Administrative sanctions: primarily relate to issues of eligibility and to the claim for benefit such as responding to letters, attending interviews, giving information and other behaviour that is primarily to do with the process of the claim and continued entitlement.

Compliance costs: the costs (time, money and psychological) that are imposed by social security regulations and statutory authorities, 'costs which would not be incurred in the absence of [benefit] payments' (Bennett *et al.*, 2009, p. 7).

Entitlement effects: impacts arising directly from lower entitlement to benefits produced by sanctions as seen from the perspective of the benefit system (i.e. welfare use and spending). For example, there are lower caseloads if fewer people claim, resulting in savings to the benefit budget.

Imposition effects: impacts arising directly from the enforcement of benefit sanctions.

Moral hazard: arises when a party (individual or group) behaves less cautiously as a direct result of being insured against a particular risk resulting in the other party having to share the negative consequences of this behaviour, for example failing to guard against job loss, because of the existence of the benefit 'safety-net'.

Propensity score matching (PSM): corrects for selection biases when using observational data by pairing individuals in the treatment group with those in the control group on the basis of their propensity score. This is essentially a single number that represents a combination of characteristics (Peck, 2007).

Reservation wage: the wage level sought by the jobseeker.

Threat effects: impacts arising from the threat of sanctions, either from the issuing of a specific sanction warning or from the presence of sanctions in the benefit system (general threat effects).

Treatment effects: impacts arising from the changed behaviour or the changed circumstances of claimants caused by sanctions. They may be short-term effects, such as employment rates, or longer-term effects, such as child outcomes and spill-over effects.

Notes

- 1 See Job Seeker Compliance Data, December Quarter 2008, <http://www.workplace.gov.au/workplace/Publications/ResearchStats/Participation+and+compliance+data/> (accessed November 2009).
- 2 In some cases policy-makers may want to fudge the difference between entitlement effects and treatment effects because it is convenient to do so (e.g. the assumption that all welfare exits are to employment). But it is a crucial distinction.
- 3 The costs (time, money and psychological) that are imposed by social security regulations and statutory authorities, 'costs which would not be incurred in the absence of [benefit] payments' (Bennett *et al.*, 2009, p. 7).
- 4 Another common diversion technique is to offer prospective claimants a financial incentive (i.e. a lump-sum payment) not to pursue their claim. Acceptance of the incentive results in a fixed period of benefit disqualification (Blank and Schmidt, 2001).
- 5 Previously known as food stamps.
- 6 Retired or unable to work as a result of illness or caring responsibilities.
- 7 GB data from DWP Tabulation Tool, Australian data from Job Seeker Compliance Data – December Quarter 2008 – see note 1.
- 8 Research evidence has shown that unemployment benefits impact negatively on the intensity of job search and raise the reservation wage (Burgess and Garrett, 2005).
- 9 An innovative quasi-experimental study by McVicar demonstrates the importance of monitoring search activity: suspension of monitoring led to lower exit rates from registered unemployment, increased claim duration and lower rates of job entry for unemployed men (see McVicar, 2008).
- 10 Or more specifically the exit rate from unemployment benefits (individuals may remain unemployed, but unregistered).
- 11 Card *et al.* (2007) have recently shown the crucial importance of understanding exit destinations and the difference between being observed to exit a programme and entering a desired destination.
- 12 Lalive *et al.* (2002) found sanction warnings to increase the 'job-finding' rate by 25 per cent, with an additional 20 per cent increase as a result of subsequent imposed sanctions.
- 13 'Federal law requires states to impose at least a pro rata (partial) benefit reduction on families that do not satisfy work and child support compliance requirements. The sanction must remain in place until the family complies with the requirement' (Blank and Haskins, 2001, p. 246).
- 14 For example, in the Los Angeles Jobs-First GAIN programme a sanction 'entailed dropping the recipient (but not the recipient's children) from the grant' (Freedman *et al.*, 2000, p. ES-9). Part of Delaware's A Better Chance (ABC) sanction policy involves dropping a dependent teen's portion of the grant when they do not meet school attendance requirements, or if out of school, work and training requirements (Fein and Lee, 1999).
- 15 'In states that impose only a partial sanction, this methodology provides a meaningful measure of the prevalence of sanctions. However, for states that impose immediate or gradual full-family sanctions, it does not' (Pavetti *et al.*, 2003, p. 9). The under-recording of full-family sanctions occurred because these were only counted during the first month they were imposed, and not on an ongoing basis – therefore missing their cumulative effect.

- 16 Figures only apply to those claimants who are subject to a work requirement (Pavetti *et al.*, 2003).
- 17 Only marginally statistically significant ($p < .10$)
- 18 It is important to note that the US term 'single parent' is primarily concerned with marital status and not co-residence as in the UK sense of 'lone parent'. Many US single parents have co-resident and/or involved partners.
- 19 Studies looking at the effect of reform on take-up have demonstrated that around half the overall decline in caseloads occurred as a result of reform 'entry-effects' (Grogger and Karoly, 2005, p. 59).
- 20 Client Assistance for Re-employment and Economic Support.
- 21 'A welfare exit was defined as a welfare recipient not receiving cash benefits for two consecutive months, and participants were defined as working if they had earnings of more than \$300 per quarter (an average of \$100 per month) reported in the UI wage records. Having a lower-earnings job was defined as monthly earnings between \$100 and \$1200, a level close to the 2003 poverty line for an adult with two children (\$14,824 annually). Having a higher earnings job was defined as monthly earnings over \$1200' (Wu, 2008, p. 32).
- 22 In her study Peck (2007) breaks down the household incomes of claimant subgroups into assistance and earnings.
- 23 It is unclear from the presentation of results how much of the impact lies in the administration of sanctions in the states under investigation; we know from other sources that in some states sanctioned claimants are removed from 'active' administrative databases using a particular closure code, while in others they remain on the rolls. Also, 'it is important to note that the prominence of above-average pre-reform caseload declines in the areas where these evaluations took place may have resulted in an overstatement of sanctions effects' (Grogger and Karoly, 2005, p. 132).
- 24 Results from Wu (2008) are not included in the graph due to significant differences in terms of both the classifications of sanction severity and the unit of impact measurement used.
- 25 Moderate sanctions do have a significant association with one type of exit – that is they reduce the likelihood that claimants will leave welfare for a higher earning job (defined as one that pays enough to lift the family above the poverty line), odds ratio = 0.60 (Wu, 2008, p. 38).
- 26 Lee *et al.* (2004) define informal work as employment activities likely to be missing from UI records, for example babysitting, performing odd jobs, housekeeping and selling crafts (p. 379).
- 27 More than \$1200 a month.
- 28 Between \$100 and \$1200 a month.
- 29 Hardship data is self-reported.
- 30 'This analysis included four categories based on the time since the first spell of sanctions ended: 1–3 months, 4–6 months, 7–12 months, and over one year' (Wu, 2008, p. 37).
- 31 This reflects the results of Fein and Lee's (1999) study which found that 'the effect of sanctions on welfare exit increased exponentially with each additional month the sanctions continued' (p. iv).
- 32 Some 18 per cent of participants in Peters and Joyce's 2006 study had little or no understanding of the rules associated with claiming JSA; 32% reported that they had not been told anything about the possibility of sanctions (see also Legard *et al.*, 1998).

- 33 Some 6 per cent of claimants did not know why they had been sanctioned and 18 per cent gave reasons that were not in the list of sanctionable breaches (Peters and Joyce, 2006, p. 3).
- 34 'Respondents were presented with realistic rule-violation vignettes in which key client characteristics were randomly assigned' (Schram *et al.*, 2009, p. 399). Each vignette included a 'claimant name'; this was white, black or Hispanic sounding. Case managers were then asked whether they would impose a sanction.
- 35 'Holding cross-county differences in individual- and family-level characteristics constant, county of residence remained clearly associated with large differences in the likelihood that an adult was sanctioned. In comparison to recipients in San Diego, adults in the other three counties were between 8% and 15% less likely to have been sanctioned' (Ong and Houston, 2005, p. 5).
- 36 Defined as: a large number of interviews per claimant, a short period of time between first and second gateway interviews and a large number of referrals to adjudication (Bonjour *et al.*, 2001).
- 37 The study by Bonjour *et al.* (2001) does include a multivariate analysis of sanctions (using bivariate logistic regression), which controls for 'the impact of other relevant factors' (p. 112).
- 38 Other studies (see Born *et al.*, 1999, for example) suggest that white women are more likely to be sanctioned; this may of course reflect the local context with regional political differences playing a role (Schram *et al.*, 2008). There may also be differences in likelihood over time. Schram *et al.* (2008) found that during the first few months of the benefit spell white clients were more likely to be sanctioned, but that as the spell expanded, black claimants faced a significantly higher likelihood. Additionally the visibility of minority groups in the community may also be significant, with smaller disparities found in areas with a large minority presence.
- 39 Age at childbearing also appears to be a significant factor (although just one study, Born *et al.* [1999] looks specifically at this issue), with 53 per cent of sanctioned claimants having become a mother aged 20 or younger, compared to 45 per cent of non-sanctioned mothers (Pavetti *et al.*, 2003).
- 40 'Edelhoich *et al.* (2000) found that sanctioned clients are twice as likely to have received cash assistance for 60 months or longer' (Pavetti *et al.*, 2003).
- 41 The proportion of claimants with access to a car was considerably lower among sanctioned than non-sanctioned claimants (19% compared with 35%) (Cherlin *et al.*, 2001 in Pavetti *et al.*, 2003, p. 14).
- 42 Studies exploring the effect of sanctions on this pivotal relationship conducted shortly after the introduction of the caseworker model indicate an erosion of trust following a sanction referral.
- 43 'Sanctioned adults are less likely (31.1%) than non-sanctioned adults (56.1%) to work in the quarter in which their welfare cases closed and, among those who did work during this period, mean quarterly earnings are significantly lower among those who were sanctioned (\$1,741.57) than among those who were not (\$2,344.41)' (Born *et al.*, 1999, p. iii).
- 44 Hardship activities include: '(1) pawning or selling personal possessions, (2) taking food or items from stores without paying for them, (3) searching in trash cans or begging, (4) engaging in any illegal activity, and (5) selling or trading food stamps' (Pavetti *et al.*, 2003, p. Notes).
- 45 An evaluation of housing benefit sanctions for anti-social behaviour is currently underway by Professor John Flint for the Department of Work and Pensions.
- 46 This is generally considered to be the largest CCT programme. 'In 2008, it covered around 11.35 million families (estimated to number

47 million people), corresponding to about one quarter of Brazil's population' (ILO, 2009b, p. 42).

47 The net replacement rate for a single person on benefit previously earning the average wage in their first month of unemployment. This compares to 69 per cent in the Netherlands (source: the OECD tax benefit calculator).

48 The document contains a proposal to introduce a three-month sanction for the second infraction increasing to up to three years for the third (Conservative Party, 2008, p. 52).

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Appendix I: The systematic review

A systematic search using a carefully devised search combination was conducted of the following bibliographic databases:

- ASSIA: Applied Social Sciences Index and Abstracts
- CSA Illumina
- International Bibliography of Social Sciences
- PAIS International
- PolicyFile
- Social Services Abstracts
- Sociological Abstracts
- Web of Knowledge
- Web of Science (includes Social Science Citation Index/Science Citation Index Expanded/Arts & Humanities Citation Index/Conference Proceedings Citation Index-Science)
- Worldwide Political Science Abstracts

Each database was searched individually with the following limits:

Time period – 1986–2009
Language – English only

Using a modified search technique (necessitated by individual search engines), a systematic search was conducted on the following databases:

- Cochrane
- Campbell

- Google Scholar
- Social Science Research Network e-library
- Social Policy Digest
- Source OECD
- Department for Work and Pensions (this involved ‘hand’ searching DWP electronic publications lists and requesting selected reports from the DWP archives).

The total number of citation hits resulting from the search was 3728. Following an initial ‘filtering’ of results 1157 of these citations (with abstracts) were downloaded into Endnote; 216 duplicates were deleted, leaving 941 citations to check for relevancy against predetermined inclusion and exclusion criteria.

The following bounds were set for the scope of studies included in the review:

- Published since 1986;
- Available in English;
- Concerned with sanctions operating as part of conditional benefit systems.

In addition policy evaluations and experiments were only selected for inclusion if they satisfied the following criteria:

- 1 Measure impacts, (cost-) effectiveness or outcomes.
- 2 Include a comprehensive and examinable methodology.

Experimental, quasi-experimental and econometric observational studies were prioritised as true ‘impact’ studies. While other less robust evidence, including complementary qualitative research meeting inclusion criteria, was saved and stored separately. This evidence proved particularly important when considering unintended (negative) outcomes for claimants.

Although the inclusion criteria guaranteed some degree of quality, additional checks were made to ensure the robustness of impact results. Each impact evaluation's methodology was checked for comprehensiveness.

Evaluations were excluded from the review if they:

- a) fell outside the scope of the study;
- b) offered insufficient methodological detail;
- c) incorrectly used statistical techniques;
- d) satisfied all inclusion criteria but were not publicly available by August 2009.

Because our search contained no restrictions on the basis of outcome measure, it was designed to identify evaluations considering any outcome: expected, unexpected, positive or negative.

We excluded 871 articles after reviewing the full text description of the study; researchers were contacted for clarification as needed. Some 70 of the remaining studies were short-listed for inclusion as impact studies; these were reviewed and discussed by two researchers in order to assess their suitability for inclusion.

Formal literature search techniques were supplemented with personal contacts, cross-referencing and hand searching. Hand searches were conducted for relevant government publications produced between 1986 and 1997 (and therefore not easily available online) in the Bodleian and LSE libraries.

Note on limitations: although fully systematic in its approach this review was limited by a number of factors arising from its very tight time restrictions; for example, it was not possible to register the title with Campbell, to have the protocol peer-reviewed or to have two members of staff review all search results. However, thorough searches and a two-researcher revision of the document short-list ensured that the review was not compromised by these restrictions.

Appendix 2: Impact result tables

Table 4. Impacts of sanctions on unemployment benefits

Study	Country	Programme	Data and method	Types of impact	Levels of impact
Abbring <i>et al.</i> , 2005	Netherlands	UI	Inflow sample of UI admin data	Imposition effects only Unemployment duration Job entry	36%–98% increased job entry for four industrial sample groups
Arni <i>et al.</i> , 2009	Switzerland	UI	Swiss register data – multivariate mixed proportional hazard model	Threat and imposition effects (warnings and enforcement) First and second order Unemployment durations, post-unemployment employment stability, exits from the labour market and earnings	Both sanction warnings and impositions increased the unemployment exit and employment entry rates. Sanction warnings had no impact on post-unemployment, employment stability, but reduced earnings. Imposed sanctions had an unfavourable impact on post-unemployment, employment stability and on earnings. The net impact of sanctions on post-unemployment income was unfavourable – ‘over a period of two years after leaving unemployment workers who got a benefit sanction imposed face a net income loss equivalent to 30 days of full pay’ (p. 1).
Jensen <i>et al.</i> , 2003	Denmark	Uninsured young people UI	Youth Unemployment Programme data	Imposition effects only Entry into work, education and ALMP	There is evidence of a sanction effect after month 9 – a notable increase in the baseline hazard for the treatment group (see p. 312).
Hofmann, 2008	(West) Germany	UI	Inflow sample for financial year 2001–2; 18–55-year-olds Matching approach, with a difference-in-difference robustness check	Imposition effects only Re-employment probability In employment, other employment or out of the labour force	‘For both women and men a sanction during stratum one or two raises the number of months of “regular employment” during the twelve month period after the stratum considered (women: 0.66, 0.85, men: 0.60, 0.80)’ (p. 20). Results show a significant effect on other employment for women sanctioned during stratum two (0.6) and a negative effect for men sanctioned during stratum one significant at a 10%-level (-0.11, -0.15). Men sanctioned in stratum two or three are more likely to be out of the labour force.

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Machin and Marie, 2004	UK	UI and UA (JSA)	Quasi-experimental – induced by the introduction of JSA	Imposition effects only Spill-over, crime rates	‘Benefit sanctions in JSA shifted people off the benefits system and raised crime’ (p. 16). Results indicate that benefit sanctions may have a social cost to society resulting from higher crime.
Müller and Steiner, 2008	Germany	UI and UA	2001–2 unemployment admin data followed for 48 months	Imposition effects only Unemployment duration Exits to employment and education	Re-employment rates rise significantly following the imposition of a sanction. Dropping following the completion of the sanction, but not back to pre-sanction levels. Effects diminish the later sanctions come in the spell of unemployment. If imposed after 15 months or later they have no effect on re-employment probability. Analysis over the longer term suggests that claimants sanctioned within three months of the claim maintained a higher re-employment probability than their control group for as much as 24 months. <i>Conclusion:</i> (Limited use of) sanctions can prove an effective tool in activation programmes if imposed in the early stages of a claimant’s unemployment spell.
Lalive et al., 2002	Switzerland	UI	UI – inflow sample over 6 months – observed for 14 months thereafter. Data includes sanction warnings.	Threat and imposition effects	A sanction warning increases the job finding rate by 25%, with an additional 19.8% increase on the imposition of a sanction. The effect of a sanction warning decreases by 16% after 30 days but effects of imposed sanctions do not significantly drop over time. <i>Conclusion:</i> Increasing the strictness of the sanction policy by one standard deviation will reduce the duration of unemployment by about one week.
Røed and Westlie, 2007	Norway	UI	Merged administrative register data. All new unemployment spells, 1993–2001	Imposition effects Unemployment duration Transition rates into employment, other benefits, ALMPs and education	‘A sanction causes an immediate rise in the job hazard of 80%, a rise in the ALMP hazard of 22%, and a rise in the education hazard as large as 200%’ (p. 35). Only the benefit shift falls – reduced during an imposed sanction by 34%. Effects appear to be short-lived.

Schneider, 2008	Germany	UB	Survey data – winter 2005/6 UB recipients and UA recipients – randomly selected from admin records. Survey response from gross sample – 73%, but effective response was lower – only 39% of gross sample responded with 46% refusals. Effective bias in survey towards better qualified, native Germans (East Germans especially), women and large households. 'Overall an overrepresentation of persons with relatively high social integration and chances of employment' (p. 18).	Imposition effects only Effect of sanctions on reservation wages	<i>Conclusion:</i> 'If benefit sanctions increase exit rates out of benefit receipt they might do it because they stimulate effective job search, and not because they reduce reservation wages. Since reservation wages are at the utter bottom of the wage distribution' (p. 44).
Svarer, 2007	Denmark	UI	Administrative data – insured unemployed sample aged over 25	Threat and imposition effects Exit rate from unemployment	Results show a causal increase of 50% in job-finding rates – this effect increases with severity of sanctions, but decreases over time. There are heterogeneous effects by gender – men showed significant evidence of <i>ex ante</i> effects. On imposition exit rates increased by 98% for women and by 55% for men. More severe sanctions are linked to higher exit rates. Effects are short-lived (no longer significant three months following imposition).
Van den Berg <i>et al.</i> , 2004	Rotterdam, Netherlands	UI	Micro-duration data – database consists of all individuals who started collecting welfare benefits in 1994 in Rotterdam. Observation period ended October 1996.	Imposition effects only Exit destination ('usually employment') Leaving the municipality Getting married Exiting for unknown reasons	Exits increase by 140% after the sanction was imposed. This effect persisted beyond the sanction period. Harsher sanctions did not result in stronger effects. <i>Conclusion:</i> 'The imposition of sanctions substantially increases the individual transition rate from welfare to work' (p. 211).

Table 5. Impacts of sanctions in US welfare studies

Source	Locality	Programme/ target group	Data, method, time period	Types of impact	Levels of Impact
Grogger and Karoly, 2005	US – multiple	All welfare recipients – TANF or predecessor data	All observational – administrative/survey data, econometric	Imposition effects only, all welfare use/caseload decline	–
Council of Economic Advisors, 1997		Annual state-level admin data	1976–96		Sanctions for non-compliance reduced welfare use/ caseload by 9.7% (statistically significant)
Council of Economic Advisors, 1999		Annual state-level admin data	1976–98		Lenient sanctions for non-compliance reduced welfare use/ caseload by 9.7% (statistically significant); eventual full-family sanctions reduced welfare use/caseload by 18.1% (statistically significant); and immediate full-family sanctions reduced welfare use/caseload by 39.4% (statistically significant)
Moffitt, 1999		Annual state-level admin data	1977–95		Sanctions for non-compliance reduced welfare use/caseload by 2% (not significant)
Ziliak <i>et al.</i> , 2000		Monthly state-level admin data	1987–96		Sanctions for non-compliance reduced welfare use/ caseload by a negligible amount (not significant)
Rector and Youssef, 1999		State-level admin data (changes)	1997–98		Lenient sanctions for non-compliance reduced welfare use/ caseload by 11.3% (not significant); eventual full-family sanctions reduced welfare use/caseload by 13.7% (statistically significant); and immediate full-family sanctions reduced welfare use/caseload by 24.8% (statistically significant)
MaCurdy <i>et al.</i> , 2002		State-level admin data (changes)	1996–99		Eventual full-family sanctions for non-compliance reduced welfare use/ caseload by 9.1% (not significant); and immediate full-family sanctions reduced welfare use/caseload by 19.6% (statistically significant)
Mead, 2001		State-level admin data (changes)	1994–98		Eventual full-family sanctions for non-compliance reduced welfare use/caseload by 9.9% and immediate full-family sanctions reduced welfare use/caseload by 15.9% (both statistically significant)

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Lee et al., 2004	US – Illinois	TANF recipients – 1998 (current and former)	Observational – longitudinal administrative and survey data 1999–2001 Full set of control variables	Threat and imposition effects Outcomes considered: – welfare exits; – entry to formal work; – informal work; – participation in job training activities; – earnings; – hardship.	<p><i>Exits:</i> There is no evidence that sanctions or threat of sanctions (initiated but lifted prior to grant loss) are associated with lower reliance on welfare.</p> <p><i>Employment:</i> The sanctioned are less likely to work in the formal sector than the non-sanctioned; but are more likely to be involved in informal work or participating in other work activities (such as job training). Sanctions reduce the likelihood of being employed by 44% (statistically significant) (after controlling for confounding variables). Sanction threats have no significant relationship with formal employment outcomes.</p> <p><i>Earnings:</i> Sanctioned claimants have lower earnings than the non-sanctioned.</p> <p>The threat of sanctions had no significant impact on earnings. Sanctioned claimants earned significantly less than the non-sanctioned – on average \$1320 in the last two quarters of the study.</p> <p>'Sanctions are negatively associated with formal employment and earnings and positively associated with informal work [and] job training activities' (p. 394).</p> <p><i>Hardship:</i> Sanctioned claimants are more likely to experience food hardship (three times higher) and overall hardship than non-sanctioned (self-reported).</p> <p>The threat of sanctions is associated with greater risk of rent hardship.</p> <p><i>Conclusion:</i> Although sanctions and threat of sanctions may induce a behavioural change – i.e. more informal work and work-related activities – these occur outside the formal sector. <i>Questions</i> have been raised around the validity of causality.</p>
Peck, 2007	US – multiple	Welfare recipients – participants in the NEWWS programme	Experimental data from the NEWWS evaluation Propensity score matching	Uses PSM to compare treatment impacts for sanction-risk subgroups. Outcomes considered: employment and income (broken down into earnings and assistance) five-year follow-up.	<p><i>Earnings:</i> Those with higher sanction propensities had 'markedly higher earnings in Years 1 through 3 compared with their control group counterparts' (p. 268). Earnings impacts for the high sanction risk group were sustained, for the low risk group effects were positive and significant in year one only.</p> <p><i>Employment:</i> The high propensity treatment group showed significantly higher employment rates than its control over all five years although the gap declines in years four and five. The low propensity group also showed initial employment gains in year one, but these were not sustained – the treatment group had overtaken by year five.</p> <p><i>Income:</i> Net impacts on income were essentially zero whatever the propensity to be sanctioned.</p> <p><i>Conclusion:</i> Overall it appears that those with a higher propensity to be sanctioned are more likely to show sustained favourable impacts on employment and earnings (although the effect on income is negligible). <i>Questions</i> have been raised around the validity of causality</p>

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<p>Wu, 2008</p>	<p>US – Wisconsin</p>	<p>TANF recipients – sanctioned and non-sanctioned</p>	<p>Observational – administrative dataset (13,171 participants) 1997–2003 Event history analysis Full set of control variables</p>	<p>Imposition effects only Considered welfare exits: – exits without work to go to; – exits to 'lower paid jobs'; – exits to 'higher paid jobs'.</p>	<p><i>Welfare use/exits:</i> Families with small sanction reductions (<10%) were significantly less likely to leave welfare than non-sanctioned families. Full sanctions or an increase in duration increase the likelihood of leaving welfare. <i>Employment/earnings:</i> Sanctions (current) were associated with increased likelihood of leaving welfare without work (18% higher) and a lower likelihood entering a higher earnings job. The probably of leaving welfare without work within the first year was significantly higher for those with a current sanction (40%) than those without (34%). Previous sanction experience was significantly associated with an increased probability of leaving welfare without a job or with a low-earnings job, suggesting sanctions have lagged effects. The risk of leaving welfare without a job or to a lower-earnings job increased with the severity and duration of the sanctions. The likelihood of leaving to a lower-paying job was nearly 50% higher for those who received full sanctions than the non-sanctioned. Families with large sanctions (51%–90%) were around 23% more likely to exit without a job (than non-sanctioned) and those with full sanctions (>90%) were twice as likely to leave without employment. <i>Questions</i> have been raised around the validity of causality.</p>
<p>Grogger and Karoly, 2005 Paxson and Waldfogel, 2003</p>	<p>US – all states except Maryland and the District of Columbia</p>	<p>Children in the state protection/foster care system</p>	<p>Observational administrative data (difference-in-difference) 1990–8</p>	<p>Imposition effects only Child well-being/maltreatment/out-of-home care</p>	<p><i>Child maltreatment:</i> Statistically significant coefficients indicate that full-family sanctions raise reports of physical abuse, of neglect and substantiated cases of abuse. <i>Limitations</i> re: reliability and generalisability.</p>

Appendix 3: Evidence from social-psychological literature

Throughout the twentieth century a number of theories have attempted to provide a theoretical framework within which behaviour, motivation and the role of incentives can be properly understood. Within behavioural science incentives fall into the class of *positive reinforcers* where the shape of behaviour is determined by the promise of some positively viewed good such as money (a reward) (Ferster *et al.*, 1975). Positive reinforcement co-exists alongside *negative reinforcement* through which a desired behaviour is affected through the removal of a good. In both instances the goal is the limitation or elimination of undesirable behaviour. Incentives can promote positive reinforcement by rewarding desirable behaviour as well as extinguishing negative alternative behaviours. Negative reinforcement operates by severing the perceived link between behaviour and the outcomes it has been associated with (Gambrill, 1977). However, while negative reinforcement, via punishment, is often viewed as advantageous by policy-makers, its more immediate effects can mask the longer-term benefits of incentives since it can often take longer for positive behaviour to emerge (Bandura, 1977). A review of nine relevant systematic reviews available through the Cochrane Central Register of Controlled Trials found that incentives can be a powerful tool in prompting the take-up of interventions, but that they have mixed success in persuading participants to remain engaged throughout a programme's treatment cycle (Volmink and Garner, 2000).

Since 2006 the use of incentives (rewards) in promoting long-term employment outcomes has been of increasing interest in the UK. Evaluations of quasi-experimental programmes designed to test incentives on young adults' engagement in activities leading to employment showed some improvements across a range of measures, including 'positive attitudes towards employment'; and take-up of 'personal development activities' (Tanner *et al.*, 2009). While limited numbers

of controlled trials exist which have tested incentives, evidence from systematic reviews indicate a positive role in helping vulnerable groups with impaired or distorted decision-making abilities. This is of interest within a wider context of working with vulnerable groups or those difficult to engage such as young people with mental health difficulties. Work on the use of mentoring as a tool to improve social and educational outcomes suggests that there are advantages to using positive reinforcement as a way of incentivising young people to engage more constructively with their peers (Wood, 2009).

Additional evidence across a wide range of intervention studies in other research areas suggests that incentives can have a meaningful role for those engaged in programmes where these are of a time-limited nature. This is the case with smoking-cessation interventions during pregnancy where a return to postpartum smoking falls outside the trials' scope (Cahill and Perera, 2008; Lumley *et al.*, 2009). Therefore, while some studies find evidence that incentives are more effective as part of a comprehensive package of support (Knapp Werner *et al.*, 2007), this finding is tempered by the possibility of confounding variables as other 'components' of a study design potentially contribute to positive outcomes (Lagarde *et al.*, 2009).

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