Methodological Outline Of The RCT Component Of The Impact Of Debt Advice Research Project[1]

1. TITLE

Improvement in financial circumstances with debt advice provision: A randomized trial of efficacy, cost-effectiveness and cost-benefit

2. INTRODUCTION/BACKGROUND

The Impact of Debt Advice RCT is part of the broader Impact of Debt Advice Research Project, which is being conducted by the Legal Services Research Centre to establish the economic, social and health impact of advice interventions for people facing debt problems. A particular aim of the project is to establish the benefit of advice in monetary terms.

A number of studies have examined the social and demographic predictors and impact of debt problems (e.g. Balmer et al. 2005, Department for Work and Pensions & Department of Trade and Industry 2004, Jacoby 2003, Kempson 2002, Kempson, McKay and Willitts 2004, Nettleton and Burrows 2000 and Tedula and Young 2004). However, despite the Department of Trade and Industry (2001) recognising "the importance of ensuring that consumers who get into debt problems should have ready access to sound, high quality debt advice" there is little empirical evidence of the impact of debt advice.

A few studies in England and Wales have suggested that debt advice can improve health, family stability and recidivism rates among offenders. For example, an early evaluation of National Debtline indicated that people receiving advice felt their health and family relationships improved as a result (Gardner and Wells 2003). In addition, Plumpton and Bostock (2003) have reported that "a recent survey of Mental Health Care Managers in Northumberland found that engaging people with debt advice services was an effective way of ameliorating mental health problems." However, they also concluded that "there appear to be no studies in the U.K. using validated measures of health and well being which show the direct impact of debt advice on mental health." Also, Samuel (2001) has reported that re-offending rates are lower among probationers who are provided with debt counselling, though the re-offending rate of Samuel's sample was simply compared with the rate in general. More generally, the Consumer Credit Counselling Service (2002) has reported that one-quarter of their clients comment that using their services improved their quality of life.

Elsewhere, Elliehausen, Lundquist and Staten (2004) have reported that U.S. borrowers who received debt counselling improved their 'credit profile' over three years relative to a comparison sample of individuals with similar 'risk profiles and geographic residences'. However, Elliehausen, Lundquist and Staten themselves note the problem of selection bias within their study.

[1] Using the CONSORT checklist (Moher, Schulz and Altman, 2001)

More robust evidence exists of the benefit of representation before courts and tribunals. Seron, Van Ryzin, Frankel and Kovath (2001) reported that just 22% of represented tenants in a randomised experiment in New York City's Housing Court had final judgments against them, compared with 51% of tenants without legal representation. In England and Wales Watts (1987) reported that 4% of those represented in local authority possession proceedings had possession orders made against them, compared to 22% of those who were unrepresented. More generally, Genn and Genn (1989) have reported that those represented before tribunals obtain far better outcomes than those who are unrepresented. However, representation in a court or tribunal is very different in nature from the provision of advice, particularly initial advice.

Finally, Abbott and Hobby (e.g. 2000, 2002, 2003) have reported on a number of occasions that improvements in health can follow from the provision of benefits advice in health care settings. However, benefits advice is concerned solely with the entitlement to and obtaining of welfare benefits, the reported studies have been relatively small and they have not been able to generate effective comparison groups.

3. PARTICIPANTS

Study participants are to be obtained from a face-to-face survey of the experience of debt among adults attending thirteen Jobcentres in ten geographical areas: London, Birmingham, Manchester, Cardiff, Leicester, Milton Keynes, Nottingham, Portsmouth and South Tyneside. Respondents to the survey who indicate they are currently experiencing a debt problem about which they have not obtained formal advice will be contacted again by telephone (provided the requisite consent is obtained) to determine their eligibility for inclusion in the RCT in a combined eligibility screening and baseline data collection exercise.

In determining eligibility, a debt problem is defined as either:

(a) being behind in payments for a mortgage, rent, credit or store card, personal loan, purchase on credit, utility bill, maintenance, tax or fine and having a problem paying the money owed; or

(b) experiencing real difficulty managing debt.

All those reporting debt problems as defined will be eligible for inclusion in the RCT provided: (i) their problems are ongoing; (ii) they have not obtained advice about their problem from anybody other than a friend or relative; and (iii) they consent to participate in the study and being contacted for the purpose of follow-up interviews. Complete eligibility screening and baseline questions are set out at Appendix A.

Follow-up data will also be collected over the telephone, 20 weeks and 50 weeks after baseline data collection.

4. INTERVENTIONS

Participants with eligible debt problems will be randomized to receive either advice provided by National Debtline (Group 1) or no advice (Group 2). In the first instance, advice will be provided over the telephone, though advice provision will be allowed to develop with each individual participants requirements, and could include face-to-face advice or representation. National Debtline will initiate the fist advice session.

5. SPECIFIC OBJECTIVES / HYPOTHESES

The objective of this study is to determine whether the offer of advice to those experiencing debt problems has a positive impact on their financial and general circumstances, whether it is cost-effective (from the individual's perspective) and whether the benefits of advice warrant the expenditure of public/charitable monies on its provision.

In this study we will test the hypotheses that the offer of advice to those experiencing debt problems as defined in paragraph 3 above:

a) reduces the proportion of people with debt problems as so defined;

b) reduces the amount of money people owe / debt servicing payments;

c) reduces the proportion of people who experience repossession/eviction;

d) reduces the proportion of people who experience formal debt recovery procedures;

e) improves people's ability to understand their finances;

f) improves people's ability to manage their finances;

g) improves people's economic outlook (when debt problems will end);

h) improves family stability; and,

i) improves people's health.

We will also test the hypotheses that the offer of advice to those experiencing debt problems as defined in paragraph 3 above benefits individuals and the public to an extent that exceeds the offer and provision of advice.

6. OUTCOMES

The primary outcome with respect to the efficacy of debt advice will be the proportion of people with no debt problem 12 months after the offer of advice. Whether or not people have a debt problem will be determined in the same way as for eligibility (see paragraph 3 above).

Secondary outcome measures will be:

- proportion of people with debt problems after 5 months;
- level of personal debt;
- amount of monthly debt servicing payments;
- ease of living on income (index);
- outlook (when debt problems will end);
- knowledge of personal financial situation;
- continuity of ownership/possession of the family home;
- continuity of services (utilities: water, electricity, etc.);
- loss of property through debt enforcement processes;
- county court judgments;
- employment status;
- relationship stability (relationship status)
- relationship stability (suggestion of separation);
- anxiety (index); and,
- health (index).

7a. SAMPLE SIZE

Recent findings suggest that across the population as a whole the proportion of people facing debt problems who will cease to face them at the end of a twelve-month period is 0.37 (Kempson, E., McKay, S and Willitts, M. (2004)). Findings from the Legal Services Research Centre's first national survey of justiciable problems suggest that around half of those facing debt problems will obtain formal advice in dealing with them (Pleasence et al. (2004)). The study is designed to detect a doubling of the likelihood of those receiving formal advice ceasing to face debt problems at the end of a twelve-month period. The target sample size is 308 at its conclusion, as a two group continuity corrected χ^2 test with a 0.05 two-sided significance level will have 80% power to detect the difference between a Group 1 proportion, π_1, of 0.29 and a Group 2 proportion, π_2, of 0.45 (odds ratio of 2.0) when the sample size in each group is 154.

To ensure a sample size of 308 at the conclusion of the study we intend to collect baseline data from between 370 and 400 eligible people. This will allow for attrition of 10-15% at first and second follow-up stage. To limit attrition we will offer participants an incentive to complete all interviews. They will be offered £10 in vouchers for providing baseline data and £20 in vouchers for completing the two follow-up interviews.

7b. INTERIM ANALYSES AND STOPPING RULES

The trial is intended to establish both the short and medium term impact of debt advice, so no stopping rule is to be incorporated relating to interim analyses.

There is a risk of attrition rates being higher than anticipated. The rate will therefore be reviewed after the first follow-up data collection exercise. If the effective sample size is reduced to below 339 then the trial will be stopped.

The trial will not be stopped if the effective sample size is below 339 at the conclusion of baseline data collection, as allocation to groups is immediate upon interview conclusion and so the intervention will have been applied to all those in Group 1 prior to a determination of the effective sample size. However, baseline data collection and allocation will be monitored closely. In such an event, the trial will be stopped after first follow-up. Nevertheless, if it becomes clear that there is no reasonable prospect of obtaining an effective sample of 339 or more at the conclusion of baseline data collection – at any point when stopping the trial would make a substantial difference to the numbers of people allocated within the trial – then the trial will be stopped.

8. GENERATION OF RANDOM ALLCATION SEQUENCE

Advice will be offered to study participants according to a computer determined 0.5 probability allocation (simple randomisation) process on completion of baseline data collection. All participants will therefore have an equal probability of assignment to either group.

9. METHOD TO IMPLEMENT THE RANDOM ALLOCATION SEQUENCE

Participants allocated to Group 1 (intervention) will have their details passed to National Debtline following the collection of baseline data by the organisation conducting baseline interviews (provided the requisite consent is obtained). Details will be passed to National Debtline in batches on a weekly basis throughout the baseline data collection period. National Debtline will contact those in Group 1 direct to offer and provide advice, along with referral if considered necessary. Participants allocated to Group 2 (control) will not be contacted again throughout the duration of the study except for the purpose of follow-up data collection. All participants will remain on the same allocation throughout the study.

10. PEOPLE INVOLVED IN ALLOCATION AND IMPLEMENTATION

The allocation of participants will be computerised (as detailed in paragraph 8) and the process will be contained within the CATI script of baseline interviews (at the end of the interview) to enable consent issues to be dealt with on the same occasion. BMG International will conduct baseline interviews. The details of people allocated to Group 1 will be sent direct from BMG International to a designated recipient within National Debtline.

11. BLINDING

Allocation will be undertaken at the end of baseline data collection to enable consent issues to be dealt with. Baseline interviewers will therefore have knowledge of the allocation of participations, but at a point following data collection. No record of this will be kept by interviewers and no details of allocation will be provided to interviewers involved in follow-up data collection. National debtline will have full knowledge of allocation, but will have no contact with the control group. The researchers undertaking the study's analysis will have no contact with either group.

12a. STATISTICAL METHODS TO COMPARE GROUPS FOR PRIMARY OUTCOME.

The proportion of participants in the two groups who no longer have debt problems will be compared by using a Chi-squared test with continuity correction.

12b. METHODS FOR ADDITIONAL ANALYSIS

More extensive multivariate analyses of primary and secondary outcome measures will be conducted using logistic regression and a range of generalized linear models. Particular regard will be had to limiting the likelihood of Type 1 errors. Additional analysis will also involve cost-benefit and cost-effectiveness modelling.

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