

# **A Literature Review of the Use of Random Assignment Methodology in Evaluations of US Social Policy Programmes**

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# **A Literature Review of the Use of Random Assignment Methodology in Evaluations of US Social Policy Programmes**

**A study carried out on behalf of the  
Department for Work and Pensions**

**By**

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Centre for Research in Social Policy**

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## **DISCLAIMER**

Views expressed in this report are not necessarily those of the Department for Work and Pensions or any other government department.

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## **GLOSSARY**

JTPA	Job Training and Partnership Act – see Exhibit A
MDRC	Manpower Demonstration Research Corporation
SRDC	Social Research and Demonstration Corporation
SSP	Self Sufficiency Project
STETS	Structured Training and Employment Transitional Services – see Exhibit E



## SUMMARY

### INTRODUCTION

The (then) Department of Social Security commissioned CRSP to undertake a brief review of the use of social experiments in evaluations of social security, welfare-to-work, education and training and other relevant social policies. The review focuses on potential difficulties with implementing and operating random assignment and the strategies and options for overcoming them. It was commissioned in the context of the extension for New Deal for Disabled People.

Social experiments provide the estimate of the impact of a programme, the difference between what happens and what would have happened in the absence of the programme. They involve the random assignment of individuals to at least one treatment group and a control group. The advantages and disadvantages of social experiments (Section 1.1) and their uses are summarised (Section 1.1.1).

### PRESENTATION OF RANDOM ASSIGNMENT TO STAFF AND CLIENTS

Frequently mentioned concerns with the use of random assignment and possible remedies are as follows:

**Random assignment is perceived as unethical** (Section 2.1). People's concerns can be allayed by addressing the issue early, requiring informed consent from programme applicants, adjusting the research design (e.g. reducing the embargo period and/or the proportion assigned to controls), or providing controls with information about similar services. Under certain circumstances random assignment appears to be ethical, such as when the study is a demonstration project, or, if an existing programme, there is excess demand for programme slots. It can also be argued that when the effectiveness of the programme is unknown it would be unethical not to use random assignment to test its effectiveness.

**Gaining the co-operation of organisations** (Section 2.2). Available options include arguing that the organisation's mission will be advanced, it will have an opportunity to influence policy development, train staff and through contact with the evaluation team it will learn something about evaluation methodologies.

**Problems recruiting programme applicants** (Section 2.3). Broadly, voluntary programme may recruit door-to-door or using outreach (Section 2.3.2). Options to increase participation rates include assigning more people to the treatment group, providing staff with training in marketing, offering financial and other inducements (Section 2.3.3).

**The extra cost and burden of administering random assignment** (Section 2.4). Options include paying compensation, integrating the random assignment process as closely as possible to the organisation's application process, and decreasing the numbers assigned to the control group.

**Perceived difficulties in handling complaints from applicants, especially controls** (Section 2.5). Possible options are providing detailed information to applicants on the experiment during an orientation session, providing staff with training and a public relations package, and informing controls of their status by letter.

### THE RANDOM ASSIGNMENT PROCESS

Alternative models of random assignment are outlined – basic, for a voluntary programme and when there is an assessment stage prior to randomisation (Section 3.1). Voluntary models include an orientation stage. These can be conducted face-to-face, by telephone or in

groups. At these sessions the applicants' informed consent to take part can be obtained and the baseline questionnaire administered (Section 4.1).

Conceivably, random assignment can occur at a number of points in the application process (Section 3.3.1). In general, if the focus of policy is on participation rather than the eligible population, then random assignment should take place as late in the application process as is possible. Although administration and other considerations also influence when randomisation occurs.

The actual randomisation procedure (Section 3.4) can be done centrally or on-site (Section 3.3.2). The former gives the evaluators greater confidence that the randomisation was done properly, but may be prone to (clerical) errors. The latter allows an immediate response on the outcome of the randomisation to be given to applicants, but there is a greater risk of staff manipulation of the outcome.

### **MAINTAINING THE INTEGRITY OF THE RANDOM ASSIGNMENT**

The integrity of the random assignment must be maintained for the duration of the study (Section 5.1). This includes maintaining contact with members of the treatment and control groups if follow-up surveys are to be conducted (Section 5.2).

### **LEGAL ISSUES**

The review found few examples of where a social experiment had resulted in court action (Section 6).

### **CONCLUSIONS**

Chapter 7 draws some general conclusions (Section 7.1), notes that there are no technical reasons why random assignment cannot be used in the U.K. (Section 7.2) and highlights the need to combine experimental with non-experimental methods in any evaluation (Section 7.3).

# 1 INTRODUCTION

Random assignment has been extensively used in the U.S. to evaluate policy initiatives (Greenberg and Shroder, 1997). In late 2000, the (then) Department of Social Security (now Department for Work & Pensions) commissioned the Centre for Research in Social Policy to review information on the conduct of social experiments. The review highlights problems which have been encountered in carrying out random assignment, and outlines possible solutions to these problems which have been adopted in previous experiments. It was intended to inform the design of the evaluation of the extension of the New Deal for Disabled People (NDDP).

The review considers previous uses of random assignment, notably US experience with welfare-to-work and education and training programmes. The advantages and disadvantages of random assignment are briefly considered in Section 1.1, but this review is not intended to be an exploration of whether or not random assignment is the best means to achieve an impact assessment.

The research entailed a review of selected literature and discussions with key informants. It was conducted during December 2000 and early January 2001.

The focus of the review is on evaluations in which individuals are randomly assigned to at least one treatment group and to a control group – often known as a ‘random control trial’. For convenience, this design will be referred to in this paper as a social experiment or study. Little mention is made of cluster random assignment in this paper. In a ‘cluster random assignment’ a community or neighbourhood can be the unit of analysis and the entire area is randomly assigned. There are very few examples of cluster random assignment in welfare-to-work or job training evaluations (Riccio, 2000), mainly because it is a very expensive form of evaluation.

## 1.1 Social Experiments

Random assignment evaluations can address the questions: ‘*What works better and in what sense? For whom? And at what cost?*’ (Boruch, 1997, p. 11). Social experiments provide an estimate of the impact of a programme, the difference between what happened and what would have happened in the absence of the programme (for example, the number of

additional people who are working as a result of the programme.) Impacts can be distinguished from programme outcomes (for example, the total number of programme participants who are working). Social experiments are characterised by one or more treatment groups that receive the experimental treatment (or programme) and a control group that is excluded from the treatment. The control group is a counterfactual or benchmark against which to assess the programme's outcomes. Individuals are assigned to the groups '*...on basis of a random event such that each individual has a specific probability of being assigned to each group.*' (Orr, 1999, p. 10).

If conducted properly, random assignment will give well-matched group samples at the point of entry to a programme.

*'The fact that, at the point of random assignment, the treatment and control groups do not differ systematically in any way except eligibility for the experimental treatment means that any subsequent systematic difference in outcomes can be confidently attributed to the program.'*

(Orr, 1999, p.42)

It is the randomisation process that makes the methodology so powerful. An alternative is to match the group samples using key characteristics and to control statistically for differences in the analysis. However, matches in a non-randomised comparison are never perfect, and unobserved but influential variables mean that there may be systematic biases that distort the comparison.

The difference in the services provided to the treatment and control groups is critical. It is this 'service differential' that is assessed. The control group is unlikely to be a 'no service' group, as members will retain access to non-experimental services. This difference in service, as well as the non-experimental services received by both groups, has to be documented and included in the evaluation.

The existence of non-experimental services affects the research question that can be answered. For example, with an on the job training intervention, a social experiment can address the question: 'Does the on the job training achieve more than the services already available?' It cannot answer the question: 'Is on the job training better than no services at all?' (See Exhibit A, Appendix A).

The advantages of random assignment are (Boruch, 1997, Orr, 1999):

- It provides an unbiased estimate of the mean impact of the programme, because the treatment and control groups do not differ systematically and any difference in outcomes can be attributed to the intervention received by the treatment group – that is, it avoids selection bias.
- Statistically, it is possible for the evaluators to state their degree of confidence that the estimate is the ‘true’ measure of the impact of the experimental treatment.
- The estimates of (net) impact are internally valid, (that is, the estimates for the sampled population are unbiased).

Its limitations are (Björklund and Regnér, 1996; Friedlander et al., 1997; Heckman and Smith, 1996; Orr, 1999):

- It provides estimates of impacts on individuals, and not on wider social and community groups like neighbourhoods. (However, in principle whole communities or neighbourhoods can be randomly assigned, with some receiving a community-wide initiative and others acting as the control group, although it is rarely practical to do this.)
- It cannot be used where it is not possible to distinguish the service received by an individual.
- Randomisation bias, which arises when the random assignment itself affects the behaviour of potential participants. Some people may choose not to participate in the programme because it is being evaluated using random assignment. They may have ethical objections or, because whether they will receive the treatment is uncertain, they may be risk adverse. This can alter the composition of the eligible population included in the study and thereby limit the extent to which the experiment’s findings can be generalised to the target population.
- A related point is that ‘Hawthorne effects’ may occur, in which participants in the evaluated programme alter their behaviour - for instance, they might increase their job-search activity - because they are taking part in an experiment, rather than because of the programme itself.
- ‘Disruption bias’ occurs when the social experiment itself affects the behaviour of programme administrators, for example, if staff attempted to manipulate the random

assignment, or provide the treatment group with services over and above those initially proposed for the programme.

- ‘Substitution bias’ arises when controls are given additional help or assistance by staff to find alternative, but similar, services to that received by the treatment group.
- Displacement effects happen when, for instance, new jobs are gained by the treatment group at the expense of the control group. In such circumstances, the programme has only a distributional effect in the allocation of benefits between members of the groups.
- Whilst a properly conducted social experiment will provide an unbiased estimate of the average programme effect per participant, it cannot supply unbiased estimates of the distribution of effects across participants. The average estimates do not distinguish between situations where most people gained the average amount and those where a few benefited greatly and most made small or no gains.

In addition, the evaluation team conducting the experiment must take steps to externally validate the study as the sample might not be representative of the in-scope population, or the economic and social milieu may have changed since the experiment commenced.

However, many of the above limitations also apply to non-experimental designs. Björklund and Regnér (1996), whilst being aware of the limitations of the methodology, argue that social experiments:

*‘... are feasible and can provide useful information about the effects of labour market policy in Europe as well as in the United States.’ (p. 89).*

### ***1.1.1 Uses of social experiments***

As already mentioned, most social experiments use individuals as the unit of analysis, rather than institutions or communities (Boruch, 1997; Greenberg et al., 1999). Social experiments have been conducted in the U.S. of negative income taxes, low income housing assistance, co-insurance rates, welfare-to-work initiatives, job training, and case management (Greenberg and Shroder, 1997; Greenberg et al., 1999). As a result, most social experiments involve individuals or households that are economically disadvantaged; the middle and upper classes have not been the subject of many social experiments (Greenberg et al., 1999). They also tend to be evaluations of demonstration projects rather than of on-going or existing

programmes. It follows that random assignment evaluations of national programmes are rare in the U.S. (Personal communication).

There have been few social experiments in which the target population is people with impairments or ill-health. However, social experiments have been used to assess the effectiveness of treatments for mental illness (Boruch, 1997). Greenberg and Shroder (1997), in their digest of social experiments, identify four completed projects aimed at people with mental health problems:

- Training in Community Living;
- Job Path;
- Structured Training and Employment Transitional Services Demonstration (STETS); and
- Transitional Employment Training Demonstration.

They also identify one on-going welfare-to-work project aimed at people with disabilities: Project NetWork.

In the U.K., random assignment has been used in several labour market evaluations over the last ten years (Personal communication). The evaluations have included those of Restart Interviews, 13 week reviews, and Jobplan, as well as aspects of some New Deal programmes.

Most random assignment evaluations of U.S. welfare-to-work and job training programmes have been of the entire programme, that is, they evaluate a collection of services rather than individual services (Riccio, 2000). Such evaluations assess the impact of programmes as ‘black boxes’; they do not explore their ‘inside’ workings. They cannot assess which of the individual programme components are effective and which are not. Even if more than one treatment group is used in order to test specific interventions, each treatment group is still a ‘black box’ albeit more narrowly defined. So studies like that of California’s Greater Avenues for Independence Program (GAIN) can establish the positive effects of the programme on earnings and its variation across six counties, but are less able to explain the observed variation (Riccio, 2000). However, implementation, process studies and non-experimental analysis can illuminate and inform the interpretation of the experimental results.

*'The distinctive effects of any particular activity within a treatment stream cannot be deduced simply by comparing the outcomes of the two treatment groups.'*  
(Riccio, 2000, p.8).

## **1.2 Structure of the Report**

Possible staff and public objections to random assignment and options for addressing their concerns are outlined in Chapter 2. The process of conducting a social experiment is considered in Chapter 3. Chapter 4 discusses baseline survey questionnaires. Methods for maintaining the integrity of the random assignment are outlined in Chapter 5. Chapter 6 briefly discusses known court cases involving random assignment. Some conclusions are drawn in Chapter 7.

Throughout the review there are repeated references to a small number of random assignment studies. To avoid repeating details about these projects, some basic information is given on each in Appendix A. The programmes are:

- Exhibit A: National JTPA study.
- Exhibit B: The New Hope Project.
- Exhibit C: Project NetWork.
- Exhibit D: The Self-Sufficiency project.
- Exhibit E: Structured Training and Employment Transitional Services



## 2 PRESENTATION OF RANDOM ASSIGNMENT TO KEY PLAYERS

Ensuring the integrity and validity of social experiments requires that staff support and understand the methodology and the aims of the study. The American experience shows that resources, time and effort have to be devoted to gaining acceptance for random assignment (Doolittle and Traeger, 1990; Gueron, 1999; Orr, 1999; personal communications), as:

*'No one has ever welcomed random assignment.'*

(Gueron, 1999, p. 1)

Staff concerns in particular should be taken seriously, as their co-operation is a prerequisite for a successful evaluation (Björklund and Regnér, 1996).

The main objections that can be raised to random assignment are:

- concerns about the ethics of randomly deciding whether or not someone should receive the experimental treatment;
- fears that the social experiment has nothing to offer the organisation providing the experimental service;
- doubts that enough applicants will enrol in the programme, because a set proportion have to be assigned to the control group;
- worries that any performance targets (that may be linked to financial payments) may not be met as a consequence of lower enrolment in the programme;
- objections to the extra administrative burden and cost of a social experiment; and
- expectations that complaints from some of those assigned to the control group and, more generally, from community/pressure groups and the media will be difficult to handle.

Options for addressing these issues are outlined in the following sections.

### 2.1 Addressing Ethical Concerns

It can be assumed that whilst 'subjects' continue to take part in an experiment, their perceived benefits of participation equal or exceed the perceived costs of withdrawal. However, this does not mean that no harm is done to particular individuals (Gramlich and Orr, 1975). This means the onus is on the sponsors and evaluators to minimise the risk of personal loss in the design and implementation of the evaluation.

Both staff and clients may believe that random assignment violates ethical standards (Gueron, 1999; Orr 1999). It can be argued that the denial of a programme, and hence service benefits, to controls is unethical. Staff may also be concerned about those who would benefit greatly from the service being harmed by being assigned to the control group. For instance, Björklund and Regnér (1996) report that an evaluation of a training programme for the unemployed or those threatened by unemployment was strongly opposed by local staff because they believed that some of those that would benefit most from training would not be allowed to participate.

Objections to random assignment on ethical grounds can be firmly held. Failure to address ethical concerns can undermine the legitimacy of a study amongst key stakeholders (notably managers and frontline staff and clients), and even lead to its cancellation (Doolittle and Traeger, 1990; Gueron, 1999; Orr, 1999).

Strategies for addressing ethical concerns are outlined in Section 2.1.1. The circumstances under which random assignment can be ethically justified are presented in Section 2.1.2.

### ***2.1.1 Strategies for addressing ethical concerns***

The American literature suggests the following options for dealing with staff concerns about the ethics of random assignment:

- Those proposing the random assignment should themselves raise and address possible ethical issues early in discussions with staff (Doolittle and Traeger, 1990). Such discussions should be thorough and frank (Orr, 1999). Separate discussions may be held with managers and front-line staff.

*‘The objective here is as much to allow staff to work through their feelings about denial of services to controls, which can be quite strong and emotional, as to convince them intellectually of the ethical acceptability of the study.’*

(Orr, 1999, p. 144)

- Explain the need for the study – in the National JTPA Study (Exhibit A), MDRC staff had long and detailed discussions with staff on the case for the study and how it would improve the existing programme. The researchers also highlighted that the study sought to measure impacts rather than outcomes and outlined the merits of random assignment compared to other evaluation designs (Doolittle and Traeger, 1990).

- A voluntary programme ensures that applicants are given the right to refuse to participate. This usually involved them signing a consent form. Information given on the consent form or in accompanying material usually covers (Boruch, 1997):
  - a description of the research, including its content and timescale;
  - the potential benefits of participation;
  - the potential risks or discomforts;
  - alternative procedures available to individuals;
  - the degree of confidentiality;
  - contact names for further information and any complaints; and
  - the voluntary nature of taking part in the study.
- An example of the consent materials and form used for Project NetWork is given in Table 2.1<sup>1</sup>. Boruch (1997) maintains that these Project NetWork documents met all of the above requirements, except that they do not cover the potential risk or discomfort of participation.
- US studies have obtained informed consent from vulnerable client groups. In the STETS study (Exhibit E), informed consent was obtained from applicants with learning difficulties or from their legal representatives. The study pre-tested the informed consent forms, counsellors at referral agencies were informed about what to tell potential applicants, and parents and guardians were encouraged to accompany applicants to their initial referral meeting. If a referral counsellor felt that an individual was unable to give informed consent, the programme application interview was scheduled for when a parent or guardian could be present (MDRC, 1982).
- Informed consent forms can also be called participation agreements (Orr et al., 2000). The individual agrees to take part in the study, acknowledges that whether they receive the experimental treatment is determined randomly, agrees to complete the baseline survey and participate in follow-up surveys, and, if appropriate, gives permission for the research team to access relevant administrative records. A related option is informing participants of their right to withdraw from the experiment at any time (Gramlich and Orr,

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1975). The informed consent form can be designed in co-operation with staff representatives (Doolittle and Traeger, 1990). It should be printed on three-part carbon paper: one copy each for the applicant, the service provider and the research team. The informed consent form should be made available in community languages. In the National JTPA Study, the consent form was available in English and 10 other community languages (Doolittle and Traeger, 1990).

- The information accompanying the consent form can be given face-to-face, in a group, or over the telephone (see Section 3.1.2). When it is given over the telephone, the signed form has to be mailed to staff. (One of the Project NetWork sites (Fort Worth – Exhibit C) found that the consent form often did not arrive and that following up missing forms was also often unsuccessful (Kulik and Bell, 1992)).
- Establish a steering group or advisory group that can represent participants' interests. Such a group may reduce, if not eliminate, any mistrust (Boruch, 1997). It follows that (potential) applicants must be told about the group and how to contact it. Project NetWork established a Technical Advisory Panel of experts to advise on the evaluation (Rupp et al., 1994).
- Provide sufficient flexibility in the research design to minimise potential problems. For example:
  - Exempt certain groups from the random assignment – the National JTPA Study excluded the homeless, people on probationary orders and referred to the programme by the court and people with severe impairments (Doolittle and Traeger, 1990).
  - A related adaptation is to allow staff to exempt a certain proportion of applicants from random assignment – 'discretionary exemptions' – where they are judged to be in dire need of the programme. Orr (1999) argues that if such exemptions are small (one to two per cent) then the effect on overall estimates of impact is negligible. In the National JTPA Study, each site was allowed discretionary exemptions of about one per cent of the target number of random assignments (Doolittle and Traeger, 1990). The exempt group is excluded from the impact evaluation.
  - Shorten the period during which controls are excluded from the programme, even if the follow-up period for collating outcome data is longer. The original embargo period for the National JTPA Study was shortened from 30 months to 18 months. At the end of the embargo period, sites could serve controls as if they re-applied on

their own initiative (Doolittle and Traeger, 1990). Previous research had suggested that few would re-apply so that the service difference between treatment and control groups effectively continued for the full 30 months. However, if few controls re-apply, then this is not a major concession to staff.

- Adjust the random assignment ratio so that fewer applicants are assigned to the control group and, hence, denied the service. However, for a total sample of a given size, equally sized treatment and control groups give the most precise estimates of impacts (Orr, 1999). This complicates the analysis, especially if the ratio varies between sites (see Orr, 1999). The reduction in precision can be corrected by increasing the size of the total sample, but this may, in turn, lead to recruitment problems, increase the administrative burden on staff and increase the cost of collecting follow-up information.
- Allow staff to provide members of the control group with a list of non-programme services. This may limit more pro-active advice given by staff, and avoids staff turning people away empty handed. Staff in the National JTPA Study handed out lists of other local providers (but were not allowed to arrange referrals) (Doolittle and Traeger, 1990). The content of the list and accompanying letter was agreed between the site and MDRC. However, the extent to which the list leads members of the control group to use services that they would not otherwise use biases the impact estimates (that is, substitution bias). For this reason Orr (1999) does not recommend it. If it is argued that controls might then be withdrawn from the study, then Orr (1999) prefers that they be informed about a single source of help that they would be most likely to use even without the additional information. Although not ideal, it is a widespread practice (Personal communication).
- Provide members of the control group with an alternative service. For example, in the Norwegian Training Experiment controls were promised that they would receive 'ordinary services' from local employment offices (Björklund and Regnér, 1996). In the U.K. evaluation of Restart, members of the control group were given a Restart interview if they requested it, that is, they received the same treatment as the treatment group (Björklund and Regnér, 1996; personal communication). Most commentators recommend against the option of giving the control group a 'minimum' substitute service. It was rejected in the National JTPA Study because developing the service would have diverted scarce resources. Moreover, some members of the treatment group might only receive the minimum service and this

would reduce the service differential between the treatment and control groups (Doolittle and Traeger, 1990). In sum, provision of alternative services will bias the estimate of impact (substitution bias), although not necessarily invalidate the study's findings (Björklund and Regnér, 1996).

### **2.1.2 Justifying random assignment**

#### ***When is denying services ethically justified?***

Experience from the U.S. suggests that those proposing a social experiment need to be able to demonstrate that it does not violate ethical standards. There are various criteria for judging whether an experiment is ethical. Boruch (1997), for example, discusses both the standards developed in the U.S. by the Federal Judicial Center and those used by Institutional Review Boards. The former suggest the following questions to test the ethical status of a proposed evaluation:

- *Is there need for improvement?*
- *Is the effectiveness of proposed improvements uncertain?*
- *Will a randomized experiment yield more defensible evidence than alternatives?*
- *Will the results be used?*
- *Will the rights of participants be protected?*

(Boruch, 1997, p.13)

Affirmative answers to each question imply that there is a case for random assignment.

Principal investigators are required to present and justify the ethical nature of research designs to independent Institutional Review Boards. In the US social experiments funded by the Federal Government are examined by a relevant board. The boards are guided by federal regulation. In the U.K. context this could be a professional body or, say, a University's Ethics Committee.

In reviewing designs and following Federal Regulations, Institutional Review Boards consider the following questions (Boruch, 1997):

- Are the risks to those taking part in the treatment minimised? This can be achieved by adjusting the random assignment ratio so if, say, controls were believed to be at risk, fewer people were assigned to the control group. In addition, members of the treatment group may be given the right to withdraw from the treatment at any time if the treatment is discomforting.

- Are the risks reasonable relative to the anticipated benefits of the programme? If the programme is ineffective, members of the treatment group will have wasted their time and effort. If, on the other hand, the programme works, then controls will have been denied access to the treatment. Thus uncertainty about the impact of a programme is necessary to justify experimentation.
- Is the selection of individuals equitable in terms of different members of the eligible population? In general, random assignment should meet this criterion, unless certain groups are exempt for unjustifiable reasons. The determination of eligibility for a programme prior to random assignment is to ensure that the study is focused on the target population.
- Is informed consent given by the applicant or a legally authorised representative?
- Are data monitored to detect possible problems that might endanger the safety of participants?
- Are privacy and confidentiality assured?

Gueron (1999), who is a leading U.S. evaluator of welfare-to-work and job training programmes, outlines the following six criteria for social experiments. They should:

- *'not deny people access to services to which they are entitled,*
- *not reduce service levels,*
- *address important unanswered questions,*
- *include adequate procedures to inform program participants and assure data confidentiality,*
- *be used only if there is no less intrusive way to answer the questions adequately,*
- *have a high probability of producing results that will be used.'* (p.3)

These overlap and effectively summarise the key criteria outline above. Nevertheless, Gueron's first two criteria raise the issue of denying programme services to controls. Social experiments always involve denying a service to a group of people, in order to create a service differential. The issue, therefore, is under what circumstances is the denial of a service justified.

Whilst social experiments must not be conducted when policymakers know the programme will be harmful to the treatment group, it should not be assumed that programme services are always beneficial; they may have zero, negative or positive impacts. Indeed, a key justification for a study is often to measure the unknown impact of a programme.

Accordingly, the denial of a service is not necessarily a disbenefit. In the U.S. programmes have not always increased earnings or improved the employment rates of members of the treatment group (Orr, 1999). This is notwithstanding that social experiments involving labour market interventions are undertaken because it is anticipated that they will have a beneficial impact.

The ‘experimental context’ influences whether the denial of services to controls is ethically justified. Two types of context can be identified: demonstration projects and on-going programmes.

### ***Demonstration projects***

Demonstration projects are special programmes, usually providing an additional service to a sub-group of the potentially eligible population. They are usually implemented with the specific intention of learning whether a proposed policy is effective. It is easier to justify random assignment in demonstration projects because:

- The denial of services to the control group leaves them in the same position as if there was no demonstration project. Any non-experimental services will still be available to controls. The experiment provides extra services to the treatment group and does not reduce services to the control group. Moreover, random assignment provides an arguably ‘fairer’ method than some other mechanisms staff might use, such as first-come first-served, for rationing services provided by the demonstration, especially as resources for the treatment will be scarce.
- Testing a proposed programme experimentally through a demonstration project is more ethical and effective than providing the services to the entire population as the programme might be harmful or constitute an ineffective use of resources.

### ***On-going programmes***

It is harder to justify the denial of services to members of the control group where there is an on-going or existing programme because some eligible applicants are being denied, for research purposes, access to services that would otherwise be available to them. The harm done by the random assignment has to be weighed against gains in knowledge about the efficacy of the programme. Orr (1999) points out that to date policymakers and researchers have decided not to deny entitlement solely for research purposes.



However, sometimes resources are limited in an on-going programme. Under these circumstances, random assignment may be a reasonable means of rationing services. Moreover, denial of services might be justified if compensation is paid or if there is excess demand for a programme.

### Compensation

Individuals in the control group can be compensated for their loss of a service and for the cost of applying (such as transport and childcare). The problem with paying compensation is that a small amount would undervalue the service (Doolittle and Traeger, 1990) and a large sum might change the nature of the service differential and so undermine the social experiment. The contrast between the treatment and control groups would no longer be just additional service/no extra service. Controls might use their compensation to gain access to non-experimental services that they otherwise would not have used and, as a result, obtain, say, paid employment. This would reduce the estimate of the net impact of the programme. On the other hand, some controls might use the compensation to allow them to defer their entry into the labour market and this would lead to an overestimate of the net impact of the programme. The effect of compensation on controls can be uncertain. If so, it makes the interpretation of impact estimates problematic.

The amount required to pay compensation may also be beyond the resources of the organisations involved in the experiment (Doolittle and Traeger, 1990).

Compensation need not simply involve a financial payment to controls. Payments can be made to both members of treatment and control groups for completing survey questionnaires (Personal communication). Alternatively, respondents completing questionnaires can be entered into prize/lottery draws. In addition, it can be argued that reducing the time period over which controls are embargoed from the programme is a form of compensation.

By definition, incentives to take part in a mandatory programme are not required. However, inducements may be needed if people are to be encouraged to complete survey questionnaires and qualitative interviews.

### Excess demand

Random assignment need not result in a reduction in the total number of people served. Resources for programmes are often scarce and demand for an experimental service can exceed its supply. Under these circumstances random assignment results in only a reallocation of services amongst the eligible population. Indeed, random assignment may be a more ethical means of dealing with excess demand for services than other methods used by staff, for example, first-come first-served or discretion.

This need not necessarily be the case if programme resources can be spread more thinly amongst the eligible population, as this would be a way of addressing the excess demand (Boruch, 1997). However, doing this may result in a degradation in the quality of services and lead to an overall fall in programme effectiveness and efficiency.

This may mean that there is a conflict between two research goals: obtaining internally valid estimates of net impacts and having a strictly representative sample of delivery sites. A lesson drawn from the National JTPA Study (Exhibit A) was that priorities must be set quickly so that planning can proceed. In this instance, the Department of Labor prioritised valid estimates of impacts over a statistically representative sample of sites (Doolittle and Traeger, 1990)<sup>2</sup>.

### Other ethical arguments

There are several other arguments that might help convince staff and clients of the legitimacy and ethical acceptability of an experiment.

First, regardless of whether the social experiment is of a demonstration project or of an on-going programme the knowledge gained should be of benefit to service providers, the client group, policymakers and taxpayers. Often those objecting to random assignment are told that it provides a powerful and effective test of a programme (Gueron, 1999; Orr, 1999). Indeed, it would be unethical not to use the strongest methodology available to test the effectiveness of the programme (Boruch, 1997; Orr, 1999). A non-experimental design risks revealing misleading and erroneous findings, and, it can be argued, it would be a disservice to the client

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<sup>2</sup> Doolittle and Traeger (1990) also conclude that had the Department prioritised having generalisable findings, then major adjustments to the design would have been necessary.

group and taxpayers to let an ineffective programme continue. In contrast, an experimental design provides the opportunity for an agency/provider to learn about what does work. If the programme is effective, the study provides evidence for its continued funding (Orr, 1999).

Secondly, whether the programme would help or harm people is sometimes uncertain. If so, the evaluation will resolve this (Boruch, 1997; Gueron, 1999; Riccio, 2000).

Thirdly, the use of social experimentation has been endorsed as the best method for measuring impacts. For example, the widespread use of random assignment in welfare-to-work evaluations in the U.S. could be highlighted or it can be argued that random assignment has been successfully used in other fields, notably medicine (Personal communication).

Finally, it is arguably more ethical to use random assignment for testing part, rather than the entirety, of the services delivered by an organisation. For instance, using random assignment and denying access to all employment services and programmes is less ethical than denying access to one particular scheme.

## **2.2 Gaining the Co-operation of Organisations**

Gueron (1999) outlines guidelines for MDRC staff seeking to enlist sites in social experiments (see Appendix B). The co-operation of the service organisations is required so that staff administer the programme in the 'normal way', both non co-operative and over-enthusiastic behaviour can undermine an experiment. Possible steps include:

- Convincing the organisation the study will advance its mission and provide reliable answers about the impact of the programme (Gueron, 1985 and 1999). This may involve highlighting the merits of random assignment over non-experiment methods (c.f. Section 1.2). By establishing what works and for whom, the organisation will be better placed to serve and enhance the well-being of its client group.
- Pointing out that the organisation has the opportunity to lead and be at the forefront of programme evaluation (Gueron, 1985).
- Agreeing to provide the organisation with summary information on the study's results (Doolittle and Traeger, 1990).
- Highlighting that contact with the research team gives staff the opportunity to acquire and study methods of data collection (Doolittle and Traeger, 1990).

- Offering senior staff the opportunity to participate in conferences/workshops that will be attended by senior policymakers (Doolittle and Traeger, 1990).
- Showing that random assignment has been successfully implemented elsewhere (Gueron, 1985).
- Include in the evaluation team people who staff will respect (Orr, 1999) – for example, the National JTPA Study included former directors of local training programmes.
- The sponsor(s) can help emphasize the importance of the study by playing a prominent role in initial discussions (Orr, 1999).
- Staff training and procedural manuals can outline the application process and random assignment procedure and discuss frequently asked questions. A sample script for dealing with client queries is often provided. This has the added advantage of ensuring that staff do not refer controls to alternative services (but see below) (Personal communication). A video explaining the study may also be used by staff, possibly until they are comfortable in explaining it themselves (Doolittle and Traeger, 1990).

### **2.3 Recruitment and Participation Issues**

As already mentioned, staff may have concerns about the possible adverse effects of random assignment on the recruitment and retention of programme applicants. Whilst staff can be concerned about the feasibility and practicality of recruiting extra participants, programme expansion may also lead to randomisation bias. The additional recruitment needed to generate a control group may mean that programmes accept applicants who they would not ordinarily take on (Doolittle and Treager, 1990; Heckman and Smith, 1996). This is a particular problem for programmes with a training component, as it might mean that lower standards for admittance are set for the programme, and this could diminish the performance of the programme (Heckman and Smith, 1996).

Moreover, recruitment problems may lead to an insufficient sample size. This, in turn, can reduce the statistical power of the evaluation (Boruch, 1997). Failure to estimate correctly the size of the target population and participation rates has been a feature of U.S. employment and training programmes (Op. cit.). Boruch notes:

*‘... a common finding in field tests of programs is that the target population is smaller or more difficult to reach than was anticipated.’ (p.89)*

If recruitment strategies are unsuccessful and sample sizes are too low, it might be necessary to terminate the experiment.

A consequence of recruitment problems is that social experiments can take longer to complete than originally envisaged.

### ***2.3.1 Possible recruitment problems***

Recruitment of applicants is potentially more problematic for voluntary programmes than for mandatory ones. So, for example, the New Hope Study, which is a voluntary welfare-to-work programme (Exhibit B), found that recruitment was one of the major challenges of the project. Recruitment was more difficult and took longer than anticipated and involved virtually all members of staff. The study experienced difficulties in finding effective ways to promote the programme and explaining its eligibility rules. The major problems were (Brock et al., 1997):

- The New Hope offer was too complex for some people to understand.
- Many potential applicants were suffering from ‘information overload’ – there was too much information in the public domain about various schemes.
- Some people were sceptical about the offer, the programme benefits were generous, and some thought there had to be a ‘catch’.
- The geographical boundaries of the study complicated the promotion of the programme – both mailing lists and the reception areas for radio and TV stations covered areas that were outside the programme boundaries.
- Some staff believed the use of random assignment deterred some applicants.

Some of these problems arose from it being a demonstration project and from the geographical targeting of the programme (Brock et al., 1997).

The National JTPA Study and the STETS study (Exhibits A and E) also encountered problems in recruiting applicants. In the latter study, some people were believed not to have participated because they (or their parents/guardians) were reluctant to give up places in other programmes, such as sheltered workshops, because there was no guarantee that they would be assigned to the treatment group (Riccio and Price, 1984).

### 2.3.2 *Recruitment strategies*

There are two basic recruitment strategies for social experiments of voluntary programmes:

- Conduct a door-to-door survey of (a sample) of households and identify a sample of people who meet the programme's eligibility criteria. Then randomly assign members to treatment and control groups, and contact members of the treatment group and invite them to take part in the programme. This method allows a take-up rate for the programme to be estimated. However, it is a relatively expensive method, especially as information will be collated on ineligible respondents and on those that whilst meeting programme entry requirements subsequently refuse to participate further. It also involves random assignment of people before they have had the opportunity to say whether they wish to participate in the programme.
- Conduct outreach work in the community and randomly assign those who volunteer and meet the eligibility criteria. This involves mailings, public notices in retail and community locations and in the mass media, news coverage in the media, networking with and presentations to relevant community groups, and close working with any referral agencies. This method allows individuals to take the decision about whether to proceed with the random assignment. The outreach can be combined with building (community) support for the programme and raising general awareness of it. However, it is harder to measure take-up rates as not everyone in the eligible population will hear about the programme. It can also be ineffective if the target population is geographically dispersed as information about the programme will have to compete against other sources. Moreover, it encourages people to come forward and apply who are then found to be ineligible.

The New Hope Study (Brock et al., 1997) opted for the outreach approach. However, although this became a lengthy and multifaceted strategy (see below), Brock et al. (1997) conclude that it was no more complicated and costly than a door-to-door recruitment process.

#### ***Outreach methods – e.g. New Hope***

The New Hope Study utilised the following outreach methods over the course of the recruitment period (Op cit.):

- *The 'start-up' phase of the recruitment (July-October 1994):* targeted mailings of potential eligible applicants living in the study's neighbourhoods; staff presentations to

various public and private community organisations (mainly churches, social service agencies and businesses); and use of local media to announce the implementation of the programme and to place advertisements. Leaflets and posters were produced in English, Spanish, Hmong and Loa. Early written materials described the project's eligibility rules and scheme components. Later these were simplified, with the help of a specialist marketing team, to emphasise the benefits of the programme. Staff got other community organisations to distribute written materials. Initially, staff contacted senior and middle managers in organisations, but later realised that front-line officers and receptionists had more contact with potential applicants and produced more applicants for the programme.

- *The 'review and retool' phase (November 1994 to March 1995)*: recruitment was slowed down so that staff could train in other skills related to delivering the programme and to reassess the recruitment strategy. A project co-ordinator was appointed to develop new recruitment strategies and assist with outreach with community organisations. The geographic area covered by the study was widened.
- *The 'rapid enrolment' phase (April-August 1995)*: temporary staff were hired as community outreach workers to conduct door-to-door canvassing; programme participants were offered gift vouchers (valued at \$5) if they recruited another eligible applicant; and a satellite office opened in the second neighbourhood<sup>3</sup>.
- The 'wind-down' (September-December 1995): the temporary workers were laid off and the gift voucher scheme was discontinued.

There was also a 24-hour telephone hotline service. During office hours callers could speak to a member of staff; at other time a recorded message provided basic information on the programme.

Single outreach contacts did not produce large numbers of referrals, but repeated contacts produced a small but steady flow of applicants. Brock et al. (1997) conclude:

*'Of all the recruitment strategies used, targeted mailings and community outreach yielded the best results. New Hope staff learned that people pay attention to different modes of communication; that many of them need to hear about the program repeatedly – and have it explained to them in person – before they will attend an orientation or submit an application; and that they are likely to enrol at a time and place that is most convenient for them.'*

(pp. 101-102)

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<sup>3</sup> Improvements in the weather during early spring are also attributed with increasing the enrolment of applicants.

### ***Mailings – for example Project NetWork***

Mailings which reached approximately 150,000 existing disabled claimants were also used to recruit 60 per cent of the participants in Project NetWork (Exhibit C) (Kornfeld and Rupp, 2000). These mailings included a reply postcard addressed to the local Social Security Administration office that could be used by those wanting more information about the study. The mailings were done quarterly in five batches, each covering one-fifth of the existing caseload (Rupp et al., 1996). One month after each quarterly mailing there was a follow-up mailing to those claiming disability benefits for two to five years and those aged 15-18 years. Whilst the initial mailings generated a substantial response (Leiter et al., 1997), the follow-up mailings recruited only two per cent of the volunteers.

Although effective, the use of mailings led to delays. First, backlogs of returned postcards developed at some sites, and, as a consequence, the timing of the mailings had to be rescheduled or the follow up mailings discontinued. This meant that some people waited between two weeks and three months before they had an informational interview<sup>4</sup>. Accordingly, some potential applicants lost interest in the programme. Some mailings were cancelled or scaled down when it was clear that recruitment targets would be met. Secondly, undelivered and returned letters as well as completed reply postcards had to be manually checked against a master mailing list at each site before those targeted for follow-up could be identified. Leiter et al. (1997) conclude that smaller but more frequent mailings would have been more efficient and manageable.

### ***2.3.3 Options for increasing participation rates***

Possible options to ensure reasonable take-up and retention rates are:

- Conduct a ‘pipeline study’ (Boruch, 1997). Prior to establishing the programme and experiment, investigate the likely number of applicants and exits. The study could explore the feasibility of recruitment, and the reasons why people might choose to participate, not participate or withdraw early from the programme. In addition, actual pipeline data – covering the initial search for applicants, through determination of

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<sup>4</sup> It also meant that some clients who had entered the programme did not get the support they needed (Leiter et al., 1997).



eligibility to enrolment – gives valuable information on the selection of the sample for random assignment.

- The original treatment group/control group ratio can be adjusted upward to allow more people to receive the treatment (Doolittle and Traeger, 1990; Orr, 1999). Any adjustment can be permanent or temporary in response to fluctuating application numbers. (Increasing the random assignment ratio can also help allay staffs ethical concerns (Section 2.1.1) and ease the administrative burden of conducting a social experiment (Section 2.4).)
- Provide specialist staff training. Doolittle and Traeger (1990) report that in the National JTPA Study local sites were given advice on recruitment and retention of applicants. This advice included: target sub-groups in the eligible population; sell the programme's benefits, not its features; avoid unnecessary steps in the application process; adopt the viewpoint of potential applicants with respect to reception facilities, customer care, etc.
- Extend the period over which recruitment takes place (Boruch, 1997).
- Intensify the recruitment effort; whether door-to-door or outreach. This will require additional resources that either have to be provided from outside the programme or diverted from other programme activities - for example, service delivery or monitoring. There is little literature on how to balance competing needs when extra resources are required to increase enrolment rates on programmes (Boruch, 1997).
- Ensure that those explaining the programme to potential applicants are well informed about the study, can empathise with applicants and are seen as possessing credibility (Boruch, 1997). For instance, University undergraduates would probably be unsuitable for enrolling applicants in a welfare benefit study.
- Offer financial and other inducements for taking part. Notwithstanding that the effectiveness of the treatment is uncertain, its receipt may be sufficient to guarantee people's continued involvement in the study. If not, the offer of a payment to treatment and control groups may be necessary. The payment can be viewed as compensation for the inconvenience of, and/or expenses incurred in, providing information to the researchers. It may take the form of a fee, gift or entry into a prize draw for those completing interviews and/or payment for travel and childcare costs. Whilst it might be presumed that payment would increase participation rates, people's reasons for not participating may be complex and unaffected by the offer. Those on higher incomes may also be less influenced by the promise of a payment. Indeed, the effect of payments on

people's decisions to participate in field experiments is unknown (Boruch, 1997). If a payment is made, the same amount should be paid to members of the treatment and control groups. This ensures that the groups are treated the same, except for the experimental intervention.

- Reduce the burden on participants by:
  - Reducing the length and number of interviews. Wherever possible, collect data from administrative records.
  - Ensuring that the information given to potential applicants is in accordance with minimum ethical standards.
  - Reducing the period of the embargo period placed on controls.
  - Following good practice in customer care.
- Relax the eligibility criteria so that more people can take part in the programme. This may, however, confound the analysis. For example, if those newly admitted do not benefit from the treatment then little has been gained, or if they do gain, but in a different way than the 'old' eligibles, then the increase in sample size might not lead to improvements in statistical power. Boruch (1997) recommends that this option be used cautiously, or not at all.
- Provide feedback on the progress of clients to agencies that had referred them to the programme (Riccio and Price, 1984).

A related issue is that any shortfall in numbers may adversely affect performance target, in some cases, organisational funding that depends on outcomes. The solution is to vary financial arrangements so that the organisation does not incur a financial loss (Doolittle and Traeger, 1990).

#### **2.4 The Extra Cost and Burden that Conducting Random Assignment Places on Staff**

Some additional cost and burden arising from administering random assignment is inevitable. Staff involvement is required to implement the random assignment and to ensure that the treatment/control status of individuals is maintained throughout the post-random assignment follow-up period. The STETS study, for example, generated extra paperwork that caused some staff not to refer clients to the programme. Possible remedies include:

- Paying compensation to local offices for agreeing to participate in the study. This has been done in many welfare-to-work evaluations (Gueron, 1995). In the National JTPA Study, local agencies were offered an average of \$170,000 as compensation (Orr, 1999).
- Integrating the random assignment process and collection of baseline data into the organisation's application process as closely as possible. This means that staff do not have to make radical changes to their usual practices, which they might otherwise resist. In addition, it minimises changes to the composition of the applicant population and hence the risk of randomisation bias. For example, requiring applicants to make an extra visit to the office to conduct the random assignment could effectively screen out those with childcare and transportation problems (Orr, 1999). Generally, random assignment adds four stages to an organisation's application process (Orr,1999):
  - informing people of the random assignment and, in the case of a voluntary programme, obtaining their consent;
  - collecting baseline data;
  - conducting the random assignment; and
  - informing applicants of the outcome.

Integration need not increase the number of staff contacts with applicants nor alter the order of steps in the application process. If desired, the baseline questionnaire and informed consent form can be included in the organisation's application pack. Different models of random assignment are discussed in Chapter 3.

- Reducing the proportion of applicants assigned to the control group may lessen the burden on staff as fewer controls have to be recruited (but see Section 2.1.1).
- Conducting the random assignment early in the application process so those applicants who eventually become controls are processed through fewer administrative stages. (The point at which the random assignment should be conducted is discussed in Section 3.3.1)

In the U.S. those proposing random assignment can point to the successful implementation of other social experiments and so demonstrate their feasibility. There are fewer UK examples, but case studies of random assignment here and overseas could help counter reservations about their practicability.

## 2.5 Dealing with Complaints

Failure to have procedures to deal with applicants' complaints can lead to contamination of the control group. Control group contamination occurs when controls receive services that they would not otherwise have received. It arises when staff refer controls to other sources of information or services. Such contamination will bias the estimates of the impact of the programme. Minimising control group contamination requires staff to understand and accept that there are limits on the information they can give to controls.

Most people understand lotteries, and in particular that their assignment does not reflect their personal characteristics (Personal communication; MDRC, 1982) - controls are not, for instance, denied services on grounds of their race (Personal communication). Possible mechanisms for minimising complaints from applicants and others are as follows:

- Hold a pre-random assignment orientation session (Section 3.1.2). It is important that people understand that receipt of the treatment is a lottery, that programme resources are scarce and not everyone can have the service on demand, and that random assignment is a fair method of allocation.
- A related point is that front-line staff must have the skills and expertise that will enable them to establish rapport with applicants. This is particularly important when dealing with vulnerable client groups, such as those with alcohol problems or with a mental illness (Boruch, 1997).
- To minimise misinformation, a public relations/press package can be produced and researchers can be involved in briefing local groups and the press (Doolittle and Traeger, 1990). Local offices could tailor the package and might have discretion on how actively it was promoted within their areas.
- Agree beforehand what staff are going to tell aggrieved applicants – as already mentioned this may involve preparation and use of a script (Personal communication). It is especially important to include guidance to staff about handling applicants threatening suicide or violence.
- For mandatory programmes establish a complaints procedure for aggrieved clients (Gueron, 1999). Such procedures may not be needed for a voluntary programme, as those wishing not to participate can opt out, notably when informed consent is sought.
- Inform applicants of their random assignment by letter, rather than face-to-face or by telephone. This negates the need for staff to respond to requests for help from controls

after assignment (Orr, 1999). Examples of the letters that may be sent to (treatment and) control group members are given in Appendix C.

- Minimise the burden on the applicants of providing information to the researchers by ensuring that any surveys are as infrequent as possible and of a short duration and maximising the use of administrative records. However, tagging and collating the necessary case papers may increase the administrative burden placed on staff.

### ***2.5.1 Reactions to being assigned to the control group***

There appears to be relatively few systematic studies of the effect on people of being assigned to the control group. Mark and Cook (1984) identify four possible reactions to the ‘inequity’ between groups caused by randomisation:

- Imitation of the treatment – members of the less desirable group (for instance, the control group) find out about the treatment and gain access to the treatment. As already mentioned above, this reduces the difference between the groups and affects the measure of the programme’s impact;
- Compensatory equalisation of treatments – programme administrators may provide similar services to those in the less desirable groups as compensation;
- Compensatory rivalry – members of the control group once aware of the treatment may become competitive, and alter their behaviour so as not to be out-performed by individuals in the treatment group (s); and
- Resentful demoralisation of participant’s receiving less desirable treatments – control group members may become demoralised, and again, this will affect the size of the effect between groups.

Welther and Ross (1982) report on a study to investigate how people react to being denied a ‘treatment’ because of being assigned to a control group, in particular does it alter their attitude or behaviour. The study involved 48 first-year undergraduate students being randomly assigned to one of three groups. Each group was required to complete an arithmetic and proofreading exercise. One group was told that an experimental group would receive an additional financial reward for each page of the exercises completed without error (the missing–reward group). Another group was informed that the experimental group would receive a punishment (a 17 volt shock) for each error (the missing–punishment group). The

third group was not told anything about the hypothetical experiment group (the comparison group). On all tests the missing–punishment group scored the highest and the missing-reward group the lowest. The authors conclude that individuals’ behaviour is affected if they know that they are in a control group. The missing-punishment group made more effort in the tests because they thought they had received better treatment relative to the experimental group. Whilst the missing-reward appeared to feel that they had been denied the extra reward, and so made more errors than the comparison group. Welther and Ross (1982) recommend that participants, where possible, are not informed of the various experimental conditions. However, they recognise that participants may discover the other experimental conditions, or there may be ethical reasons why participants should be informed of all experimental conditions.

Lam et al., (1994) report on a US social experiment evaluating various treatments for homeless substance abusers. Qualitative research with members of the control group show that there was a wide variation in the extent to which the men were affected by their random assignment. Some rationalised the outcome of the random assignment; they believed they must not have been eligible; or said they did not really wish to participate in a long-term treatment programme; or decided it was ‘the Lord’s will’. Others, however, appeared to understand the randomisation process, and were angry at their control group assignment. The level of resentment against the random assignment amongst the control group was less than feared by the staff of the service providers involved in the study. In part this may have been because the control group did receive payments for interviews connected with the study; that is, they did receive something from their participation. Indeed, the randomisation process had a greater emotional impact on service providers than on control group members.

Solutions include minimising the information given to control group members about the treatment (see discussion above), monitoring the implementation of the treatment, and checking whether post test characteristics of the control group differ significantly from its baseline (Mark and Cook, 1984). Major changes would indicate that the control group had reacted in one of the ways listed above. Data for the control group can also be compared with other sources as a check.

### 3 THE RANDOM ASSIGNMENT PROCESS

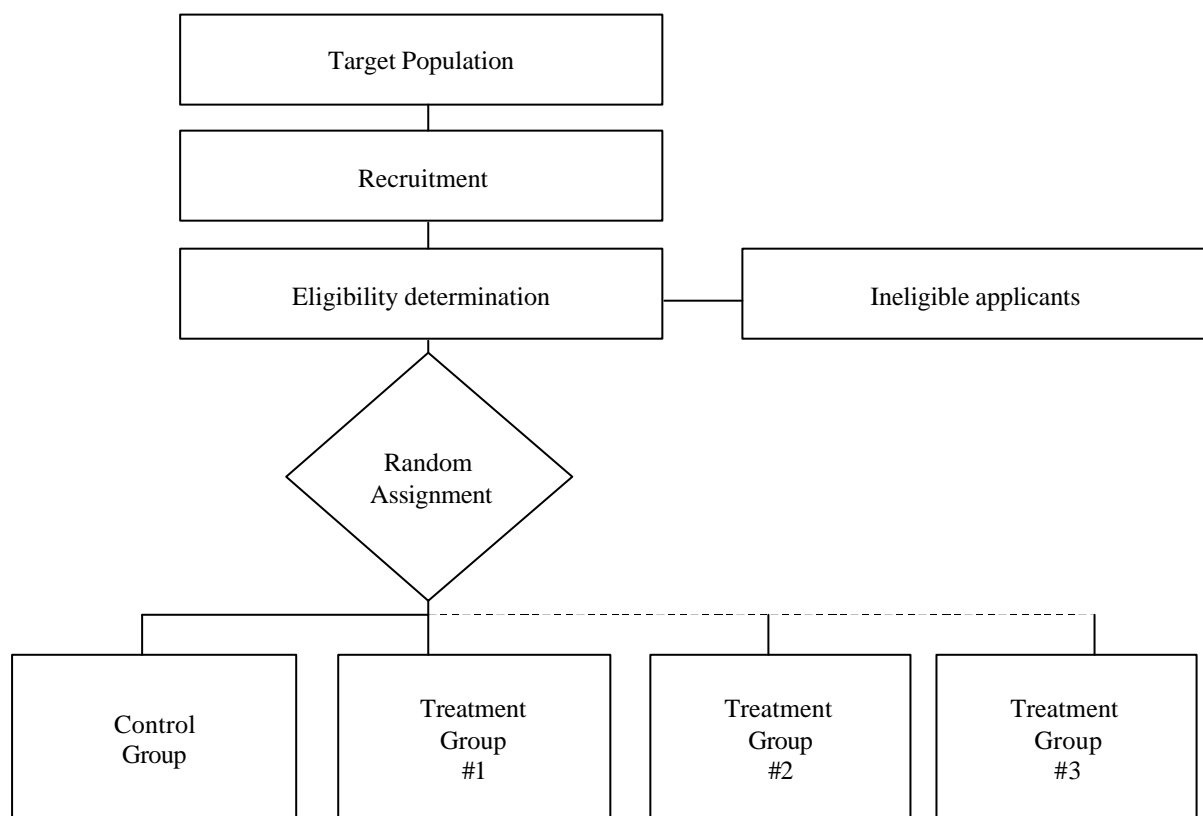
Assigning individuals randomly to treatment and control groups can be done in a number of different ways. This chapter outlines both alternative models of random assignment (Section 3.1) and some actual examples from the North American literature (Section 3.2). Some of the practicalities of doing random assignment are considered in the remaining sections.

#### 3.1 Models of Random Assignment

##### 3.1.1 *The basic model*

The basic model for random assignment is shown in Figure 3.1.

**Figure 3.1 The Basic Model**



A variation is to have more than one treatment group – known as a ‘differential impact design’. This allows the relative impact of alternative treatments to be assessed. Examples are the National Evaluation of Welfare-to-Work Strategies (NEWS) evaluations (Atlanta, Georgia; Grand Rapids Michigan; and Riverside, California) where two treatment groups are being used to test two alternative treatments, a ‘work first’ approach and a ‘human capital

development' approach for helping lone parents move off benefit into paid work. A third group is the control group. Estimates can be made of each treatment's **net** impact (by comparing the treatment group against the control group) and the **relative** impact of the treatments (by comparing treatment groups with one another). However, it is often not administratively feasible to have more than two or three treatments operating at the same time (Riccio, 2000).

### *Eligibility determination*

Individuals are recruited (or referred in a mandatory programme) and randomly assigned following the determination of their eligibility for the programme. (Recruitment issues and options are discussed in Section 2.3). The eligibility criteria determine the flow of cases to the programme - known as the 'pipeline' (Boruch, 1997). The eligibility criteria reflect legal requirements, organisational and programme rules and, with a voluntary programme, the applicant's informed consent. Information on eligibility is usually collected on application forms. Eligibility should be determined as if there was no random assignment study, so that the study resembles as closely as possible the programme's 'regular' in-take process. It is important that only the formal eligibility criteria are used to determine whether applicants go forward to the random assignment. Screening using informal criteria will lead to randomisation bias, as it alters the composition of the sample. In the STETS study, for example, some of the referral counsellors at certain sites (notably in New York) only referred clients they thought were job ready. The extent of such screening is unknown (Riccio and Price, 1984).

Eligibility must be determined before random assignment to ensure a fair test of treatment(s) (Boruch, 1997). It is recommended (Boruch, 1997) that random assignment occurs as close as possible to the delivery of the treatment. This is because the longer the period the greater the likelihood that individuals selected into the treatment group will not show up to receive the treatment. If sample numbers are then reduced this will decrease the statistical power of the study. Moreover, the person may engage in a similar activity provided by someone else rather than wait for the promised treatment.

Variations on this basic model may be required to accommodate:

- voluntary programmes;



- rationing of programme places; and
- programme assessment processes.

### ***3.1.2 Voluntary model***

With a voluntary programme, the recruitment stage may involve outreach or door-to-door recruitment using caseworkers, advertisements in local media, and telephone and letter contacts with potential applicants (see Section 2.3). Voluntary programmes also often include giving people further information during an ‘orientation’ session.

#### ***Orientation session<sup>5</sup>***

Having gained potential applicants’ interest, they are given further information at what is known as the orientation stage. The aim of orientation is to provide more detailed information about the programme, its benefits, what participation entails, random assignment and its implications, and the data collection methods to be used. Orientation can be done face-to-face in the home, in the workplace, in groups at community centres and other locations (Personal communication) or over the telephone. Both verbal and written presentations can be used (Orr, 1999). Group orientation sessions can vary in size, for example, in one study from 30 to 3,000 people (Orr et al., 2000). The groups can be held on different dates and at different times (including evenings and at weekends). They may involve a presentation and different media, such as video (Personal communication). In, for instance, the JOBSTART study, high school dropouts were informed about the programme (basic education, occupational skills and help getting employment) and that selection was by random assignment both verbally and in writing. Only then was their consent to take part sought.

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<sup>5</sup> Orientation sessions may also be a feature of mandatory programmes.

As already mentioned, often in voluntary programmes those taking further part in the study (that is, the applicants) may be asked to complete a consent form. This says that the individual has agreed to be randomly assigned. In addition, individuals may give consent to taking part in a survey (although they may later opt out) and to the researchers having access to relevant administrative records. The baseline questionnaire (see Section 4.1) can also be completed at this time.

In a demonstration project, the evaluators might address orientation groups. With an on-going project, this may be infeasible and the orientation has to be conducted by the delivery organisation's staff.

A further variation is that the orientation stage occurs after the random assignment (see Self-Sufficiency Project example below).

### ***Non-participants***

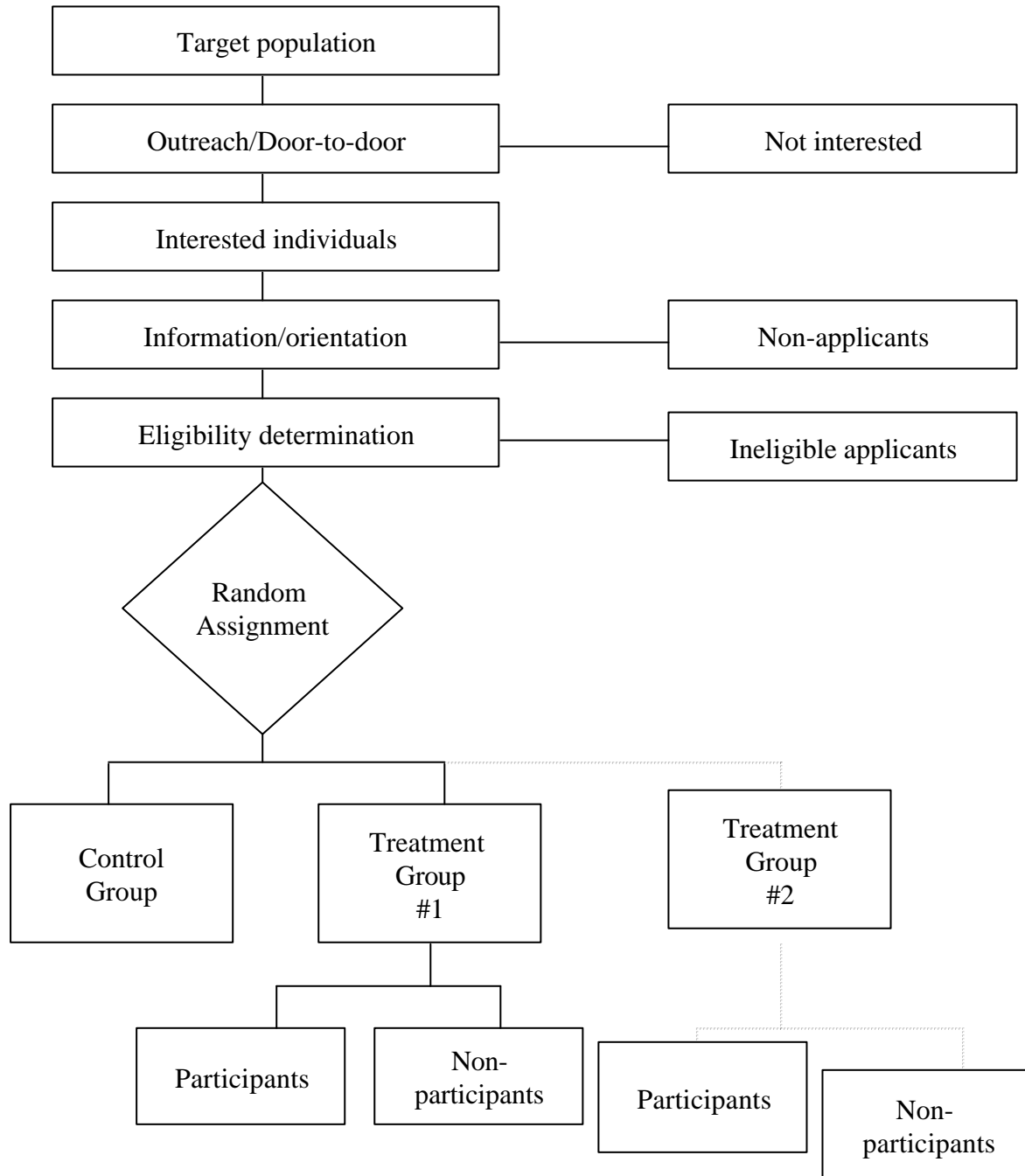
Not all of those agreeing to take part and assigned to the treatment group will participate in the treatment; there will be non-participants or no-shows. Following random assignment, it is the offer of a service in a voluntary programme or the imposition of a service in a mandatory project that distinguishes the treatment group from the control group. It is the offer of services to, or the imposed policies on, the treatment group that constitutes the experimental treatment. These must be distinguished from the actual take-up of, or compliance with, the service<sup>6</sup>. This means that the net impact will reflect the offer of the service to the entire treatment group, regardless of whether they did or did not receive services from the provider. Assessment of net impacts based on the allocated group rather than the actual treatment received is known as 'intention-to-treat' analysis. It is the intention-to-treat analysis that provides the reliable estimate of the impact of the programme. Once this information is available, it is possible to infer the impact of the receipt of the service on participants (see Orr, 1999). However, the evaluation should establish the extent to which services were actually used by members of the treatment group in order to interpret estimates of net impact.

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<sup>6</sup> For example, in the New Hope Study approximately three-quarters of those assigned to the treatment group claimed a programme benefit (Brock et al., 1997).

This suggests the more elaborate model shown in Figure 3.2.

**Figure 3.2 Random Assignment for a Voluntary Programme**



This gives treatment groups and a control group a representative of the potential participants in the programme, who have been selected by staff as eligible for the programme.

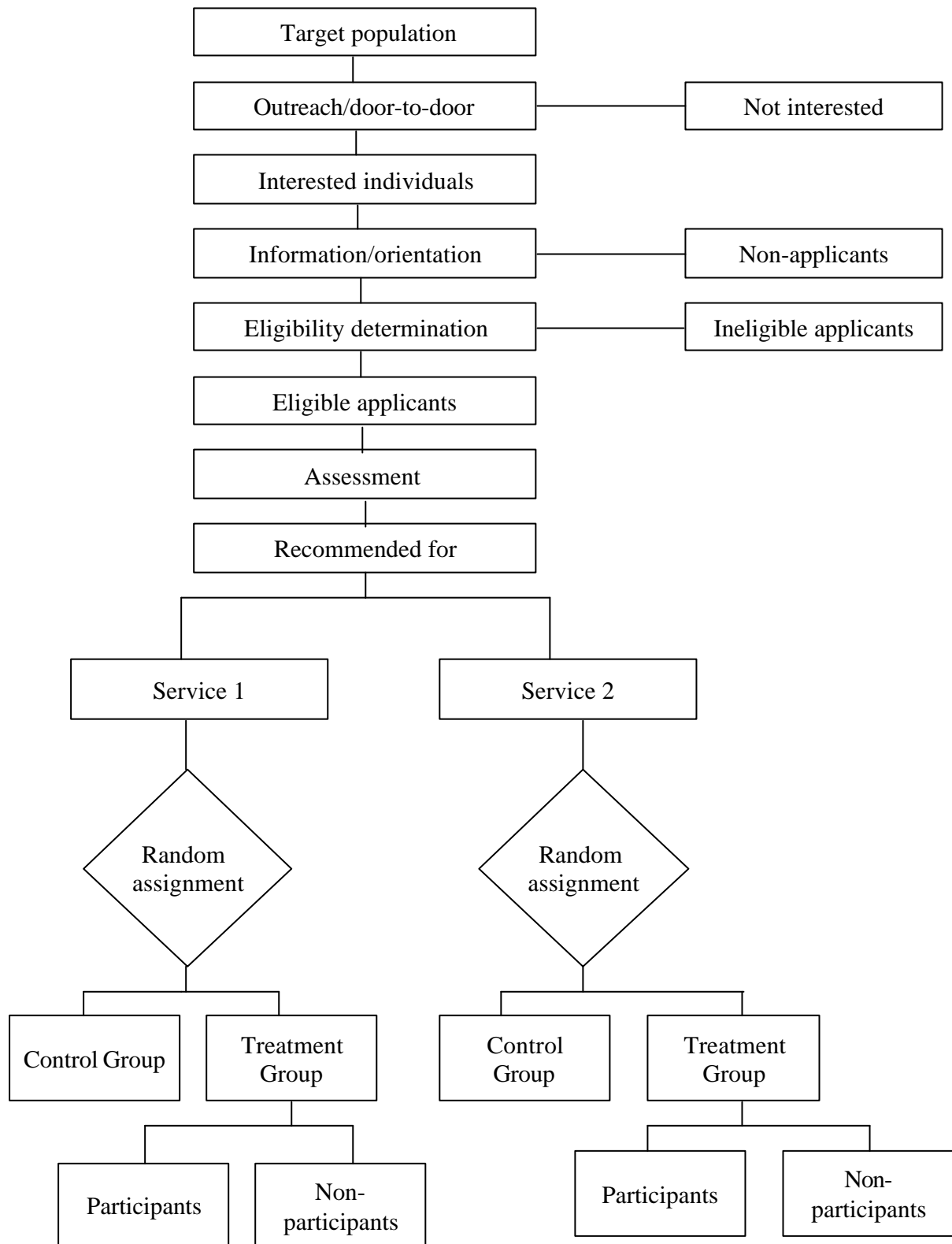
There may be another stage after ‘eligible applicants’ and prior to ‘random assignment’ in the above model in a demonstration project. Where the number of eligible applicants exceeds the number of places on the programme, some other criteria may be used to determine who is admitted to the random assignment (such as, first-come, first-served) (Orr, 1999).

Alternatively in an on-going programme the random assignment itself, may be the mechanism used to ration the places on the programme amongst eligible applicants.

### ***3.1.3 Priori assessment model***

The designs above allow the **relative** impacts of various treatments to be determined by randomly assigning applicants who satisfy the programme’s eligibility criteria to one of the treatment groups and the control group. However, this design overrides any assessment process undertaken by the administering organisation, as allocation to treatment groups is done randomly. This may not be acceptable to staff and participants. Moreover, it can create perverse treatment groups, for instance, if University graduates were assigned to basic literacy classes. To overcome this, random assignment can take place after staff have conducted an assessment and recommended applicants for different treatments. This gives the design in Figure 3.3.

**Figure 3.3 Random Assignment with Prior Assessment**



However, this means that the relative impacts of the treatments can no longer be assessed as differences in treatment ceases to be the only difference between the groups. The net impact of each treatment is obtained by comparing it with its control. Furthermore, comparing the experiences of all members of the treatment groups with those of all of the control groups gives an estimate of the programme's overall impact. However, the three treatment groups cannot be compared with one another because they are dissimilar in treatment and baseline characteristics (Doolittle and Traeger, 1990).

This design is problematic if the pre-random assignment assessment includes the delivery of any significant services, such as help with job-search. As this assistance would be available to controls, the effect would be to underestimate the impact of the programme. Furthermore, staff knowledge that their assessment is followed by random assignment might influence the assessment process. For example, Björklund and Regnér (1996) report that in the Norwegian Training Programme evaluation pre-random assignment screening procedures were modified by local staff so that the number proceeding more closely resembled the number of places available on training courses, thereby reducing the potential for random assignment.

Screening may also occur after random assignment. People assigned to Project NetWork (Exhibit C) treatment groups were assessed (medical, psychological and vocational) before personalised action plans were prepared. An estimated seven per cent were screened out of the programme at this stage because they were deemed incapable of work. However, this screening out was not applied consistently across the project sites (Leiter et al., 1997). In addition, there could be delays in waiting for the assessments and some clients lost interest in the programme during this period

## 3.2 Examples of Random Assignment Models

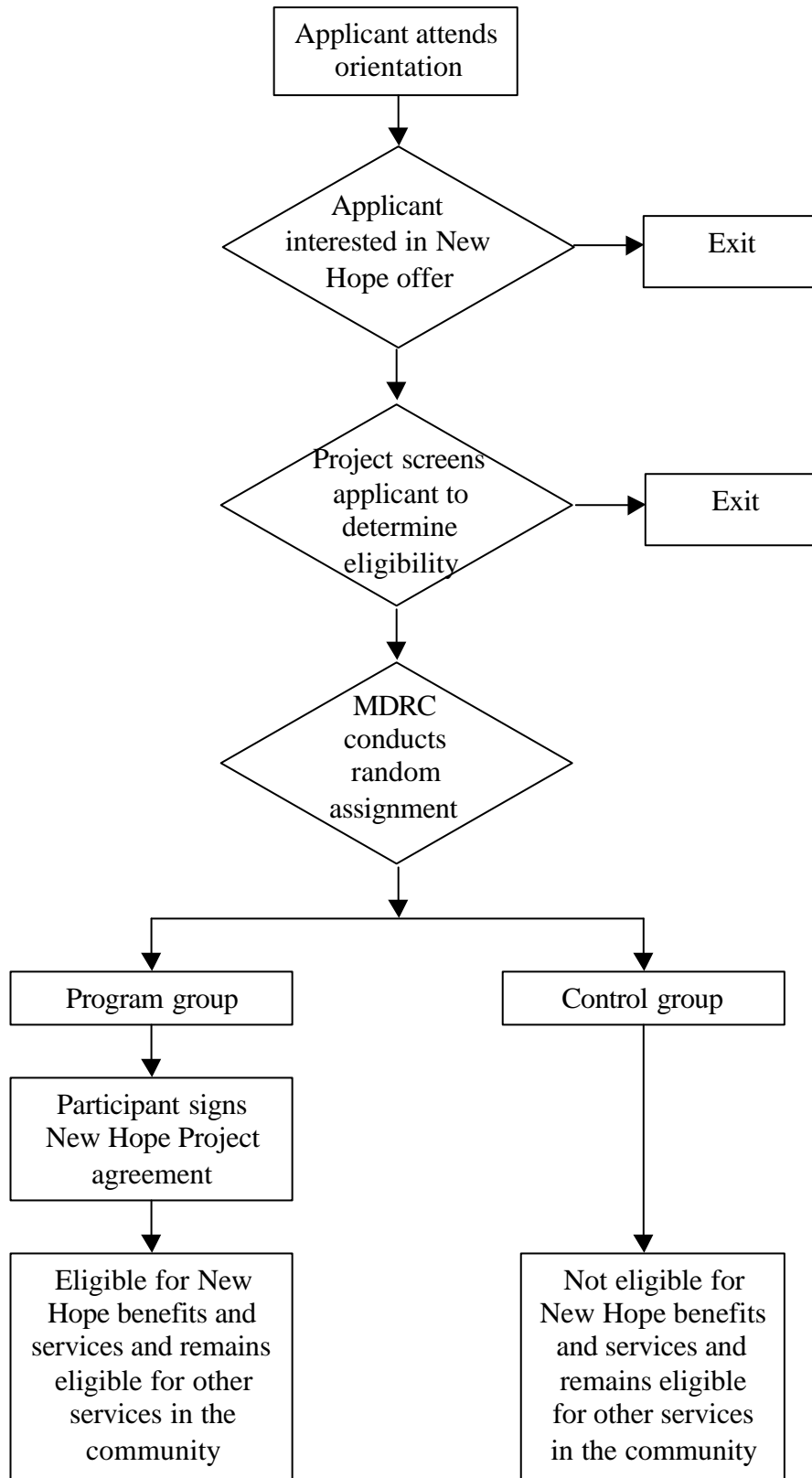
### 3.2.1 *New Hope Study*<sup>7</sup>

As previously discussed, extensive outreach work was conducted to recruit applicants (see Section 2.3.7). Interested individuals were invited to attend an orientation session where staff explained the offer, eligibility criteria, study objectives and the random assignment process (see Figure 3.4). People wishing to take part then met with staff to determine their entitlement. If they qualified, people were asked to complete a baseline questionnaire. New Hope staff then telephoned MDRC and gave the applicant's name and social security number. The random assignment was completed and individuals were immediately told of their assignment status. Members of the treatment group were asked to sign a 'participation agreement' and controls were given a list of other employment-related service in the community.

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<sup>7</sup> This is taken from Brock et al., 1998 (see Exhibit B).

**Figure 3.4 An Overview of the Random Assignment Process for the New Hope Project**



Source: Brock *et. al.* (1997), Figure 1.1, p. 11



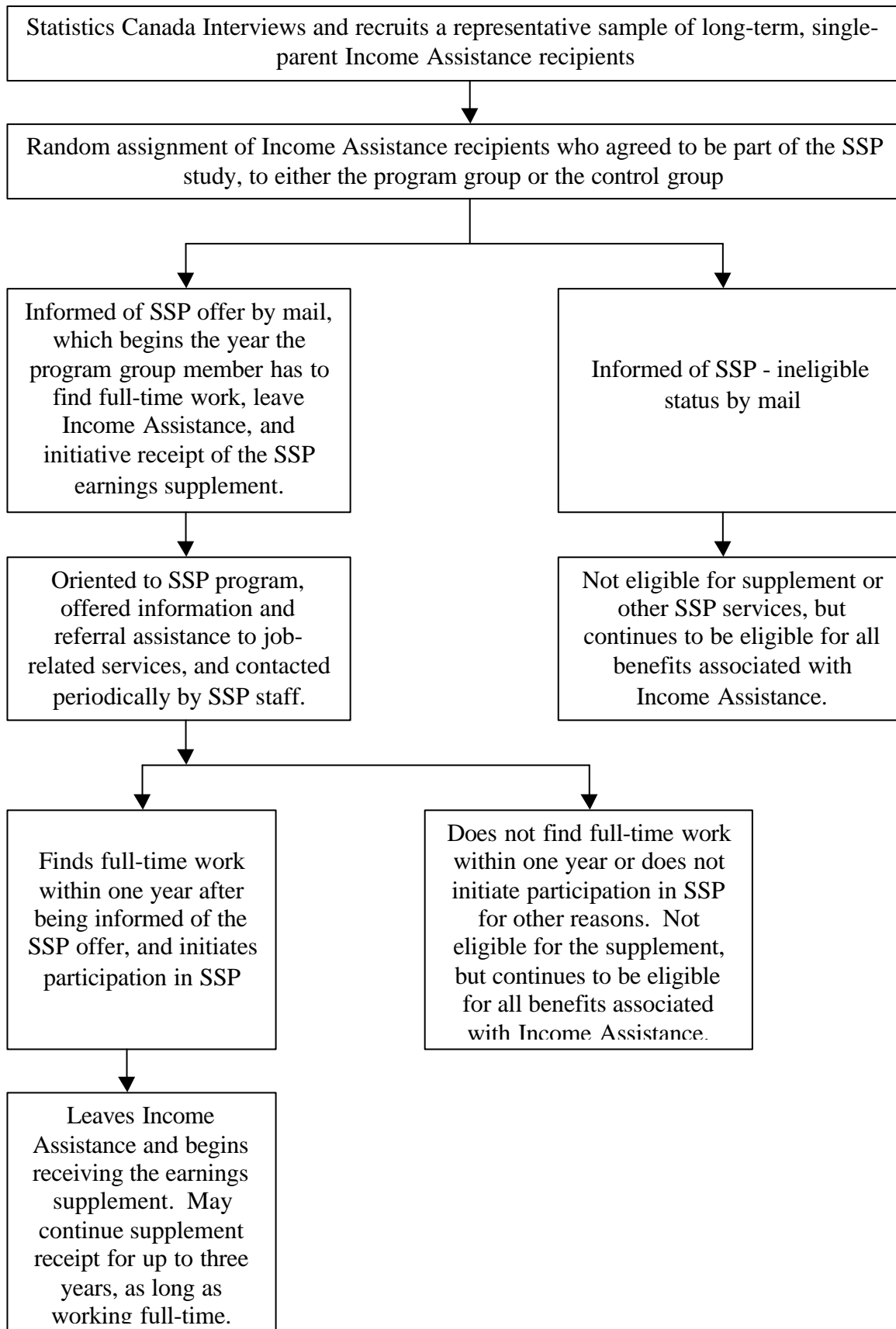
### 3.2.2 *The Canadian Self-Sufficiency Project*<sup>8</sup>

Recruitment for the main Self-Sufficiency Project was conducted between November 1992 and March 1995 (see Exhibit D). Each month Statistics Canada identified people in the target population, (that is, lone parents currently claiming Income Assistance who had also done so for 11 of the last 12 months, and who were aged 19 years or more and resident in British Columbia and New Brunswick) using administrative records. A ‘field sample’ was then randomly selected by Statistics Canada for interview. At the interview a baseline questionnaire was administered, the project was described, an informed consent form was read to the interviewee and any questions were answered (see Figure 3.5). Signing the consent form meant that the lone parent agreed to participate in the study, to take part in subsequent surveys and to allow Statistics Canada to collate relevant administrative records. Interviewees were told that only Statistics Canada would have access to data that could uniquely identify them, that participation did not affect benefit entitlement, that they could refuse to answer any survey questions, and ‘... *that 50 per cent of those who agreed to join the study would be randomly selected to become eligible to “get additional money” if they “find a full-time job within the next 12 months.*’ (Lin et al., 1998, p. 8) Approximately, 90 per cent of the field sample signed the consent form and completed the baseline questionnaire. The 6,028 lone parents were then randomly assigned to treatment and control groups. Lone parents were informed of the random assignment by post. Those in the treatment group were then invited to orientation sessions where they were given further details about the study.

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<sup>8</sup> This account is taken from Lin et al., 1998.

**Figure 3.5 An Overview of SSP Sample Intake and Program Participation**



Source: Lin *et. al.*, 1998.

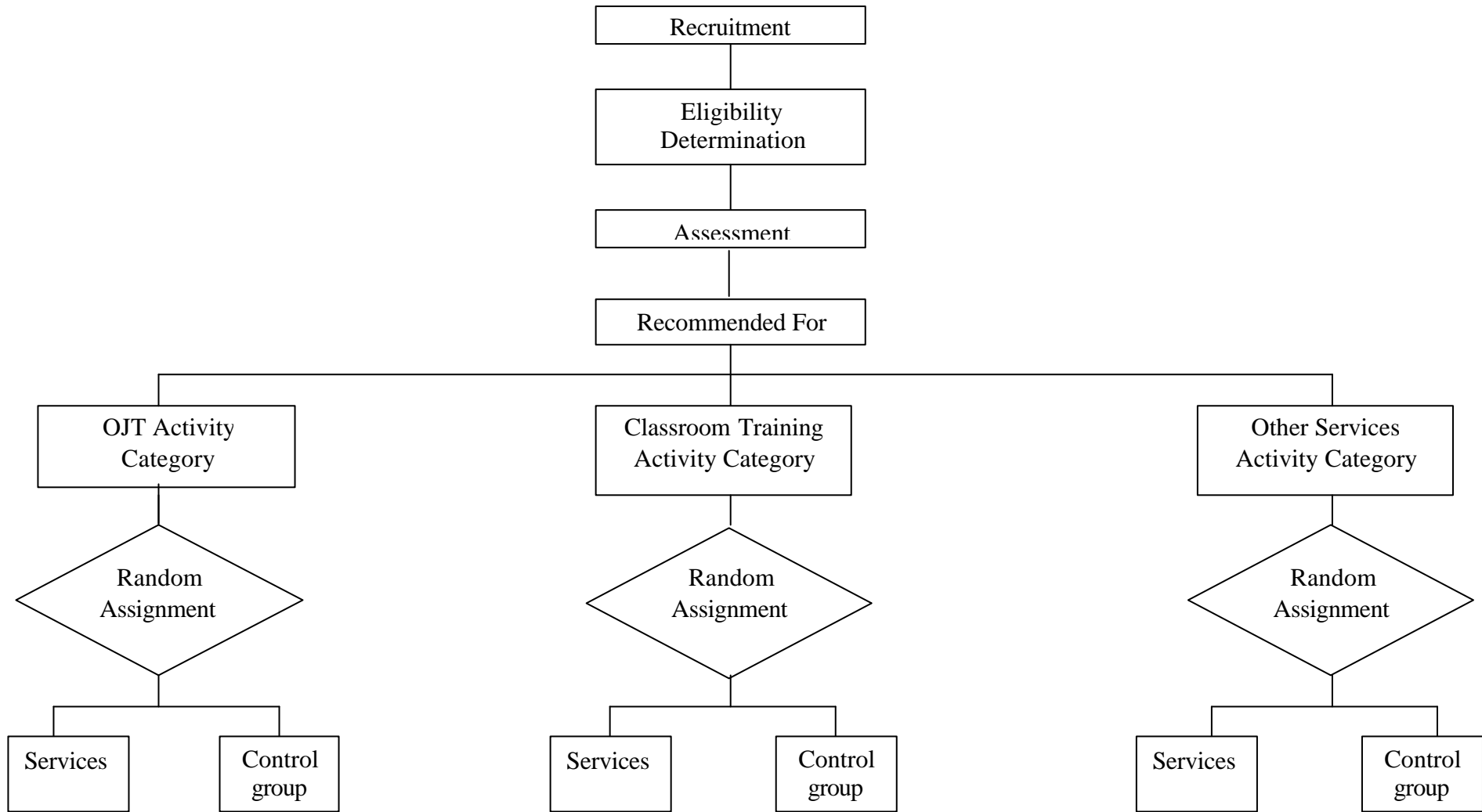
### 3.2.3 *The National JTPA Study*<sup>9</sup>

The National JTPA Study involved random assignment after staff had assessed applicants and recommended them for one of three treatment categories (Exhibit A) (see Figure 3.6). In general, the staff determined applicants' eligibility for the programme and briefly told them about the study and random assignment (see Figure 3.7). As part of the application process, applicants completed the baseline questionnaire. Applicants were then informed again of the study and allowed to ask questions. Several local offices used a video that outlined the goals and procedures of the study. Some offices made their own video. After the study had been explained, applicants were asked to sign an informed consent form, this indicated that they understood the study procedures and gave the researchers permission to have access to relevant administrative records. Staff then carried out their usual assessment procedures and, if appropriate, recommended a treatment category. MDRC was then contacted on a free phone number. Staff provided MDRC with details about which treatment category was recommended and basic demographic information (to monitor the build-up of the samples). The telephone calls could be made for individuals or groups of batched applicants. During the call MDRC made the random assignment and staff were informed of the result. Members of the treatment groups were referred to appropriate service providers and controls were told they were ineligible for the programme for 18 months, but were given a list of other services available locally.

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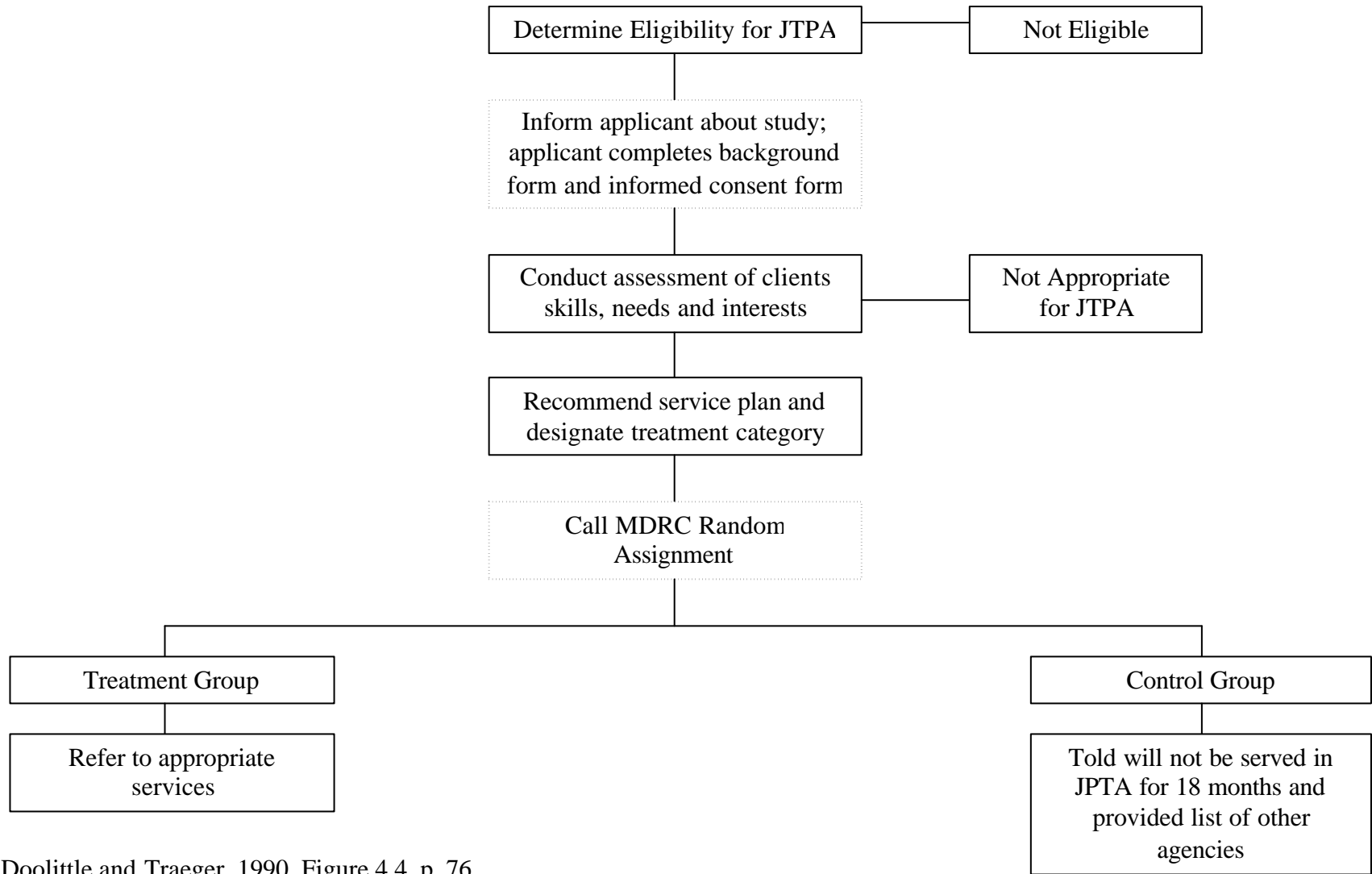
<sup>9</sup> Taken from Doolittle and Traeger, 1990.

**Figure 3.6 Random Assignment Model for the National JTPA Study**



Source: Doolittle and Traeger, 1990, Figure 4.5, p. 87.

**Figure 3.7 Random Assignment Procedures in the National JTPA Study**



Source: Doolittle and Traeger, 1990, Figure 4.4, p. 76.

### **3.3 Implementing Random Assignment**

Critical to the evaluation design are decisions about the point of assignment – at what point in the process applicants are assigned – and whether the assignment is done centrally or locally.

#### ***3.3.1 Point of Assignment***

Random assignment can occur at a number of points in the referral and intake processes of a programme. For example, both STETS and Project NetWork (Appendix A) included the referral of potential applicants, conceivably random assignment could be undertaken by the referral organisation, as part of the application process administered by the agency delivering the intervention or when determining access to certain types of provision such as training or help with job-search activities. The point of assignment is critical, because it defines who is in the sample and membership of the treatment group and control group is determined. Accordingly, the research question must determine the point at which randomisation takes place.

In general, if the policy focus is on participants in the programme rather than on the eligible population, then the random assignment should be located as late in the application process as is possible (Orr, 1999). The earlier the point of assignment, the greater the number of non-participants (Orr, 1999).

However, the decision on the point of assignment must also take into account operational considerations and the possible objections of staff to the programme. There are pros and cons for having the point of assignment early or late in the application process. The earlier controls are identified, the less the administrative burden on the organisation as they have to be processed through fewer stages. The individual is also informed sooner and might not have had his/her expectations raised too highly by the prospect of the intervention. Further, staff become less emotionally committed to people who have been assigned to a no service group. However, more people may have to be recruited (thereby increasing the cost and burden on staff) in order to ensure sufficient numbers of individuals who actually receive the treatment. Furthermore, if staff undertake an assessment of applicants before assigning them to a treatment, it may be appropriate to do the random assignment after this assessment. The

longer random assignment is delayed the more information staff can collate and the greater the likelihood of an appropriate assignment to a treatment category<sup>10</sup>.

### ***3.3.2 Centralised or localised random assignment***

Random assignment may be done centrally or on-site. If done centrally it may be done by the evaluation team, or by the evaluation's sponsor. In America, random assignment is often done centrally and by organisations independent of the service provider (Boruch, 1997).

A centralised approach requires a local member of staff to contact the relevant office (by telephone or facsimile) and provide certain details<sup>11</sup>, the random assignment is then done and the local office informed of the assignment.

The advantages of centralised random assignment are that it:

- Gives the research sponsor and/or the evaluators' control over the random assignment and hence the confidence that it is being conducted properly.
- Minimises the opportunities for local staff to 'tamper' with the random assignment.
- It facilitates monitoring and quality control. The sponsor/research team has a record of the random assignment that can be cross-checked against local office records.

The disadvantages are:

- It is time-consuming – although this may not be a problem, unless the service provider's application process requires applicants to immediately go on to the next stage.
- It is expensive – staff may have to be recruited to do the random assignment, and extra telephone lines may have to be installed.
- Errors may occur – the wrong name or National Insurance number may be copied down at either end. Reconciling central and local hard copies of the random assignment can be time-consuming.

Alternatively, local staff may do the random assignment. The advantage of on-site random assignment is it can be done very quickly – applicants can be given an immediate response. (This, however, is not necessarily an advantage. It may be easier for staff if they are not

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<sup>10</sup> An individual may, of course, still be assigned to the control group within this treatment category.

<sup>11</sup> These details for the 'random assignment record' and this is discussed in more detail in Section 3.4.2.

subject to blame for the random assignment if those assigned to the control group are informed by post rather than face-to-face (see Section 2.1.1.)

The disadvantages of doing random assignment locally are it can be prone to error and, more importantly, assignments can be manipulated especially if a relatively simple procedure is used such as whether a person's National Insurance ends with an even or odd number. However, this can be minimised by using a random assignment co-ordinator within the local office, who can be trusted to conduct the process properly, and/or a computerised system<sup>12</sup>.

### 3.4 Randomisation Procedures

The criteria for selecting the randomisation procedure are:

- The algorithm must generate a 'genuine' random assignment:

*'... random assignment is an all-or-nothing process. It doesn't help to be a little bit random.'*

Gueron, 1999, p. 5

- Staff must not be able to manipulate the outcome of the random assignment procedure.
- Staff must have no foreknowledge of what the next assignment will be. As a result staff are unable to treat people differently because they are unsure of the outcome of the random assignment.
- Staff must not be able to alter the result of the random assignment.

The main means for doing random assignment are (see Boruch, 1997; Orr, 1999):

- Simple allocation – individuals are assigned singly, in sequence.
- Block allocation – individuals are assigned within a block of say five or ten people. The assignment to the treatment and control groups within each block is random. Many U.S. and Canadian evaluators use computers and block randomisation (Orr, 1999; Personal communication).
- Stratified allocation – individuals are classified by relevant stratifying criteria (such as, ethnic origin or gender) and then randomly assigned.

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<sup>12</sup> This was done in the STETS study (Exhibit E).



Computerised systems for randomisation can also check (using National Insurance numbers and applicants' names) whether someone had been previously assigned (Kulik and Bell, 1992)<sup>13</sup>. If not, then the random assignment is made. If there are doubts staff may carry out enquiries to verify that randomisation is appropriate.

### ***3.4.1 Use of National Insurance Numbers***

In the U.K. National Insurance numbers have been used to randomly allocate individuals to treatment and control groups (Personal communication). For example, if the assignment ratio is 1:1, then individuals with National Insurance numbers ending with an odd number are, say, assigned to the control group and evens to the treatment group. Assignment using National Insurance numbers is relatively simple and straightforward (Personal communication). However, assignment using National Insurance (or social security (Boruch, 1997)) numbers is not unproblematic:

- Staff may know whether a client will be assigned to the treatment group or control group, and this may influence their behaviour towards the client. They might, for example, be more pro-active in encouraging clients to take up non-experimental services if they knew they were going to be assigned to the control group or not encourage them to complete the baseline questionnaire. It is important that staff do not know the outcome of the assignment in advance as this could bias the estimate of the net impact (Doll, 1998).
- Similarly, if clients knew in advance that their National Insurance number meant they would be assigned to the treatment group or control group, then this could affect their decision to participate in a voluntary initiative and/or their responses to (attitudinal and behavioural) questions in any (pre-assignment) baseline questionnaire.
- Some individuals have duplicate National Insurance numbers and others are missing a number.
- Combined with assignment at local offices there is scope for accidental and deliberate misallocation of individuals to groups through 'misreading' the last digit (see above).

### ***3.4.2 The randomisation record***

Those conducting the random assignment must keep a record of key details that will enable the analysis to be done later on. This record should include:

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<sup>13</sup> This will be easier if the random assignment is carried out centrally, as the various computer systems would have to be linked if it was done locally.

- Applicant's:
  - Name;
  - Personal identifiers: current address, National Insurance number, gender, date of birth;
- Local staff:
  - Name;
  - Office identifier; and
- Date of random assignment.

## 4 DATA COLLECTION

### 4.1 Baseline Survey Questionnaire

Data collected on individuals prior to treatment are known as baseline data. Baseline data is used to (Boruch, 1997; Orr, 1999):

- Verify that eligibility conditions for programme entry have been met;
- Verify the randomness of the assignment to treatment and control groups. There should be no statistically significant differences between groups (see Section 5.1);
- Describe the key characteristics of the study population;
- Support any ‘pipeline study’ (Section 2.3.3) through providing information about the in-flow to the programme;
- Improve the study’s statistical power, a measure of the overall variability of the experiment, by taking into account in the analysis pre-treatment characteristics that are correlated with outcome variables;
- Assist in non-experimental analyses; and
- Help assess the generalisability of the results.

Baseline surveys are sometimes used in social experiments to ascertain the above information.

In some instances baseline questionnaires can be administered to people who do not apply to the programme. Information can be collected on the reasons for their non-application.

The survey can be conducted face-to-face (in the home or elsewhere) or by telephone. Where these surveys are used, they are usually administered during the orientation session (see Section 3.1.2). They can be self-administered or completed wholly by, or with assistance from, agency staff or interviewers who are part of the research team. Baseline data may also be collected from administrative records.

Usually, the baseline survey is administered before random assignment. The questionnaires can take 30 minutes to complete and cover family composition, education and training, work experience, earnings and benefit receipt (Doolittle and Traeger, 1990). The Self-Sufficiency Project baseline survey, for example, took on average 30 minutes and collected information

on: respondents' demographic characteristics, household composition, employment, earnings, education and training, childcare needs, attitudes towards work and welfare, housing, disabilities and current income.

The Project NetWork baseline survey was a longer interview, and was conducted shortly after the random assignment lasting 60-90 minutes (Rupp et al., 1994). It covered education and training, health and functional limitations, transportation problems, employment history, earnings, personal attitudes, income and benefits, emotional and cognitive well-being, receipt of programme services, respondent assessment of Project NetWork, and knowledge of relevant benefit rules (Kornfeld and Rupp, 2000).

Concurrent with administration of the baseline survey, other information may be collected that aids the study. For example, information was collected in Project NetWork on a Supplemental Interview Form (Appendix D) which covered identifiers (for linking records), contact details with relatives and friends (in order to trace applicants if they had moved), and details concerning the person's impairment or illness and language preferences that could affect the conduct of any survey interview.

Follow-up surveys of baseline respondents allows unbiased estimates of the impact of the programme to be calculated using survey based outcome measures.

## **5 MAINTAINING THE INTEGRITY OF THE RANDOM ASSIGNMENT**

The integrity of the random assignment must be maintained for the duration of the study (Gueron, 1999). As discussed earlier, it is almost inevitable that some members of the control group receive a similar or the same service as those in the treatment group. In the U.S., for example, this is known to have occurred in some of the education and training evaluations (Friedlander et al., 1997). Controls can attend, through other routes, the courses delivered by the same training providers as those in the treatment group. In the National JTPA Study, for instance, some controls received classroom based education and training similar to the treatment group. Thus, programme effects were not measured relative to the absence of any services, rather it was the incremental effect of the programme that was measured. A serious problem, often called substitution bias, can occur if the very existence of the evaluated programmes alters the services that are available to members of the control group. This would occur, for example, to the extent to which any special training provision for the treatment group frees places in existing programmes and thereby alters the number of training opportunities available to the control group.

### **5.1 Options for Ensuring the Integrity of the Random Assignment**

Steps to ensure the integrity of the random assignment include:

- Checks that applicants have not previously applied and had been assigned to the treatment or control groups. In the National JTPA Study (Doolittle and Traeger, 1990), staff involved in the application process were required to check applicants against a cumulative list of all people that had been random assigned. This list was updated weekly. Repeat applicants were reminded of the study and of their assignment status, which they retained.
- The monitoring and policing of the delivery of services to ensure members of the sampled population are offered or denied the correct services. Steps are required to ensure that members of the treatment group receive the treatment and that the control group is not contaminated by its members receiving the treatment. This may become difficult and burdensome with a differential impact study. Any departures should be recorded, as this information will help in the interpretation of the study's findings. By assigning individuals a unique ID number, an audit trail can be generated to follow people through the study. The random assignment record can be cross-checked against administrative and survey data records to ensure consistency. In addition, minimising the time between

the random assignment and the delivery of the experimental treatment should reduce the risk of its members not receiving the treatment (Orr, 1999).

- Pilot or pre-test the random assignment procedures. Or using previous studies may serve as a pilot - for example, Project NetWork was preceded by two smaller scale studies.
- Conduct formal statistical analysis of the randomisation algorithm in order to verify that the assignment was, in fact, random (Boruch, 1997). In addition, observational studies may be used to investigate how the random assignment was carried out in practice.
- Periodically, the baseline characteristics of the treatment and control groups can be compared to investigate whether there are any systematic differences, as the key characteristics for both groups should be similar. This can be done for the sample as a whole and for each local office or site (Kulik and Bell, 1992). Statistical tests can be used to reveal whether any differences arise from non-chance factors and so require investigation.
- The build-up of the sample can be monitored on a weekly basis. This provides information on when the target sample size is likely to be met, and whether the flow from any local offices is unexpectedly low. The latter would require investigation.

## **5.2 Maintaining Contact with Participants**

The retention rate is those co-operating later on the experiment as a proportion of those initially enrolling in the programme. The longer the period since the baseline survey the lower the retention rate is likely to be. Boruch (1997) quotes a number of studies where the retention rates for treatment groups range from 39 per cent for a dental care study after four years to over 95 per cent for a pre-school experiment after eight years. It is important that retention rates be kept as high as possible by maintaining contact with those taking part in the study. Otherwise sample sizes are reduced and this will decrease the statistical power of the experiment and the ability to detect small differences in impacts. An even more serious problem, which is known as attrition bias, can occur if retention rates differ between those who are helped by a programme and those who are not.

Maintaining contact requires considerable resources. It is usual to collect applicants' names, addresses and telephone numbers when recruited in order to locate them. Information on someone else, such as a friend or a relative, who may know their future whereabouts can also be collected (see Appendix D). Studies have also used methods that have been successful in

longitudinal studies, such as sending birthday cards and personalised reminders (Op cit.). Even outreach methods like canvassing local retail outlets and community organisations have been used to trace people.

## 6 LEGAL ISSUES

Despite the extensive use of social experiments in the U.S. there appears to have been relatively few instances of legal action brought against programme sponsors and evaluators (Personal communication). There is also relatively little information about those cases in which legal action has known to occurred. The following summarises the available information (Personal communications):

- Job Corps – a job training programme for disadvantaged young people. A case was filed on behalf of a number of controls because they were denied services that could have benefited them. The Judge temporarily halted the study arguing that Government officials had breached federal law by failing to subject the design's methodology to public review. A settlement was reached whereby the Government pledged to contact control subjects by 2000, which was after the embargo period ended, and enrol them in the programme if they were still eligible. Compensation (\$1,000) was also paid to 15 plaintiffs for providing information to the court.
- In an experimental test of a negative income tax plan, a local prosecutor attempted to obtain data on subjects in order to prosecute people for benefit fraud; he did not obtain the data.
- In another experimental test of a negative income tax plan, someone sued claiming that the programme caused his wife to leave him. The court rejected the claim.



## **7 CONCLUSIONS**

This report has reviewed some of the evidence on previous use of random assignment. Potential difficulties and strategies and options for tackling them have been outlined. This chapter draws some general conclusions on conducting random assignment (Section 7.1) as well as commenting on the transferability of U.S. and Canadian experience to the U.K. (Section 7.2) and on the links between experimental and non-experimental methods (Section 7.3).

### **7.1 General Conclusions**

The quote by Gueron in Section 3.4 is telling: ‘you cannot be ‘a little bit’ random’. Randomisation sets a standard that has to be met in establishing and maintaining a social experiment. Not only must the experimental design incorporate random assignment, but neither the operation of the programme nor any complementary non-experimental methods must undermine it.

### **7.2 Transferability**

Notwithstanding the random assignment experiments conducted by the Employment Service, there have been relatively few social experiments conducted in the U.K. A couple of our key informants pointed out that people in the U.K. may hold a different notion of ‘fairness’ to those in the U.S.; with the U.K. placing a greater emphasis on equity of treatment. This could affect how advocates of random assignment present the case for random assignment to the public and programme applicants. Presumably, stressing the lottery of random assignment, how this can be fairer than other methods for allocating services; and that there are benefits to clients, service providers and society as a whole from using the strongest possible methodology for testing a programme. Hogwood (2000) reviews possible reasons for the lack of social experimentation in the U.K. However, this review has not identified any technical reasons why random assignment should not be used to evaluate social policy in the U.K., given the necessary political will. Indeed, there is an opportunity to learn from U.S. experience and avoid some of their mistakes.

### **7.3 The Wider Evaluation**

All evaluations involve the making of assumptions, which require testing and verification. Social experiments warrant complementary non-experimental methods to support the

interpretation of impacts. Beyond measurement of impacts, social experiments usually incorporate a process or implementation analysis and a cost-benefit analysis. Process analysis examines how the programme was implemented and its wider environment. In the U.K. process evaluations can be known as formative evaluations and they have been a feature of policy evaluations. Similarly, cost-benefit analysis is often a component of U.K. social policy evaluations.

The possibility of substitution bias means the evaluation needs to incorporate an investigation of all of the related and similar services used by treatment group and control group members, and complementary non-experimental analyses to explore the use of substitute services.

The possibility of randomisation bias justifies a 'pipeline' study, that is, an analysis of the 'flow' of applicants and others into the programme.

Finally, close contact between the policymakers and those doing the evaluation is desirable in order to exploit fully the power of the methodology (Björklund, A. and Regnér, H., 1996; personal communications).

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## **APPENDIX A**

## APPENDIX A: EXHIBITS

*Exhibit A:* National JTPA (Job Training Partnership Act 1982) Study (Doolittle and Traeger, 1990)

The Job Training Partnership Act programme was introduced in 1982 and aimed at one million disadvantaged young people and adults. The evaluation was of the on-going voluntary programme, which provided classroom and on-the-job vocational training, help with job-search and other services. The programme is delivered by approximately 600 local agencies, who commission other agencies to provide services. This is described as an ambitious social experiment, which was difficult to implement. An agreed design had to be negotiated by the researchers, local sites and the U.S. Department of Labor. A statistically representative sample of sites was sought for the study. However, this was not possible because local agencies could not be required to participate and most (approximately 90 per cent) refused to co-operate. Eventually, 16 sites reflecting a variety of local settings were recruited. However, there were proportionally too many agencies from the Midwest and too few from the South.

Staff screened and assessed applicants for different treatments. The total sample of 20,606 was divided between three 'treatment categories' – on the job training (7,433), classroom training in occupational skills (7,106) and other services (6,067). Individuals allocated to each of these categories was randomly assigned to a treatment group and a control group. The achieved sample was 7,817 below the original target, mainly because of problems in recruiting sites, difficulties enrolling people in the programme, opposition to random assignment from service providers and staff concerns about informing individuals of the outcome of the random assignment. The random assignment was undertaken between November 1987 and September 1989.



*Exhibit B:* The New Hope Project, Milwaukee, Wisconsin (Brock, et al., 1997)

Evaluation of a voluntary demonstration project conducted in two inner city neighbourhoods in Milwaukee. The project aimed to increase employment and combat poverty through offering a package of generous financial incentives and job opportunities. Applicants had to reside in the targeted areas, be aged 18 or over, willing and able to work full-time and have a household income at or below 150 per cent of the federal poverty level. The programme was run by a non-governmental organisation. Recruiting applicants was a major challenge for the study. Over a 16 month period, beginning in July 1994, the project team recruited and randomly assigned 1,357 people to a treatment group (678) and a control group (679). This was 162 people more than the original sample target. The team learnt that successful recruitment requires a concerted and sustained effort, and that targeted mailings and community outreach work achieved the best returns.

*Exhibit: C* Project NetWork (Kornfeld and Rupp, 2000; Kulik and Bell, 1992; Leiter et al., 1997; Rupp et al., 1994 and 1996)

A voluntary demonstration project to test the use of case management and referral services in the rehabilitation and employment of disabled people in receipt of U.S. Social Security Disability Insurance payments and Supplementary Security Income payments<sup>14</sup>. Those eligible were aged over 16, not in paid work and not actively involved in any other work focused programme. There were two intake streams: current Disability Insurance and Supplementary Security Income claimants and new Supplementary Security Income applicants. Four models of case management and referral were tested at eight sites:

- The Social Security Administration case manager model (Dallas and Fort Worth, Texas);
- The private contractor case manager model (Minneapolis and Phoenix/Las Vegas);
- The State vocational rehabilitation agencies case management model (New Hampshire and Richmond, Virginia); and
- The Social Security Administration referral to local providers model (Tampa, Florida and Spokane, Washington/Coeur d'Alene, Idaho).

The case or referral manager arranged for rehabilitation and employment services, helped develop an individual action plan and provided employment counselling services.

The eight sites were not randomly selected. Indeed, they are unlikely to be representative of the U.S. in terms of local socio-economic conditions.

The study commenced in June 1992 and the enrolment period lasted 15 months. Those joining at the beginning could have received services over two years whilst those entering towards the end received up to nine months of services. The study finished in 1995.

The study involved the random assignment of 8,248<sup>15</sup> voluntary participants to treatment (4,160) and control (4,088) groups across the four models. A sample of 138,613 eligible non-participants taken from social security records provided basic information on non-participants. In addition, the design used a stratified sample for the baseline survey. Not all members of the treatment and control groups were surveyed. Interviews were held with 2,555 participants (that is, treatment and control group members) and 884 non-participants between March and December 1993. Most of the participants (1,521) were re-interviewed two to three years later (June to November 1996). Two groups were over-sampled<sup>16</sup>: those claiming benefits for two to five years and those aged 15-18 years.

The main method used to recruit existing claimants was a rolling programme of mailings. However, this led to delays in local offices, as staff could not quickly process the returned postcards from those wanting more information about the project. The recruitment of new benefit claimants was also problematic. Staff did not always recruit new claimants. This was partly because of the (a) backlog of postcards, (b) the extra paperwork involved and (c) belief by staff that new benefit claimants were not a good source of subjects for the study due to their ill-health. Indeed, new claimants said that they believed their health precluded them from working and consequently they did not understand why they were being asked to participate in a return to work scheme.

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<sup>14</sup> The demonstration also waived certain benefit rules believed to cause disincentives to work or case management. The waivers applied to members of both treatment and control groups.

<sup>15</sup> The target was 8,400 volunteers.

<sup>16</sup> Around 8.5 times the rate of other eligible applicants.

Of those initially agreeing to attend an informational (orientation) interview to learn more about the programme, only one-half subsequently attended. In most sites this interview was conducted face-to-face, but in two sites most were conducted by telephone. Three sites also used group interviews to give information on the programme.

The estimated participation rate is 4.9 per cent.

*Exhibit: D* The Self-Sufficiency Project, Canada (Lin et al., 1998)

The evaluation is of a voluntary demonstration project where lone parents on social assistance (Income Assistance) were offered an earnings supplement for moving off benefit into full-time work. The study was conducted in British Columbia and New Brunswick. Recruitment for the main study took place between November 1992 and March 1995. Eligible claimants were invited to take part in the study and then randomly assigned to the treatment group or a control group. A second treatment group, 'SSP Plus' that offered help with job-search and job counselling was subsequently added to the design. Of the 6,028 applicants randomly assigned, 2,880 were assigned to the treatment group, 229 to SSP Plus and 2,849 to the control group.

Applicants knew they were to be informed of their random assignment by post, and that they could telephone SRDC (the evaluators) to obtain further information, if they wished (Personal communication). There was a separate telephone number for the control group, so that staff knew the call was from a member of the control group and could be better prepared. Some people did telephone. Most of those calling wanted clarification about their assignment to the control group, to ask what this implied for them and what they should do next. Some wanted to explore changing their assignment status and/or express disappointment about not receiving the SSP supplement. A few were disappointed, even angry, at being in the control group, but would admit that it was good that something was being done to reform the system. A small number telephoned to withdraw from the study; and some of these were persuaded to stay in the project. Those wishing to withdraw were posted a release form which they had to sign and return. People ineligible for the treatment also occasionally called (Personal communication).

*Exhibit: E* Structured Training and Employment Transitional Services (MDRC, 1982; Riccio and Price, 1984)

A voluntary demonstration project that sought to help 18 to 24 year olds with a learning difficulty and poor or no work histories move into unsubsidised employment through providing on-the-job training. The study comprised three phases:

- assessment and work readiness training of no more than 500 hours of work experience, training and support in a low-stress environment;
- transitional services during which participants worked for a minimum of 30 hours per week with local employers. Wages could be partially subsidised and on-the-job training was geared to the participants gaining unsubsidised employment with their employer; and
- post-placement support services were provided after a participant became a regular, unsubsidised employee.

The project was a multi-site experiment operating in New York, Cincinnati (Ohio), St Paul (Minnesota), Tucson (Arizona) and Los Angeles (California). Applicants were enrolled in the evaluation between mid-October 1981 and mid-January 1982. The original intention was to randomly assign 1,000 eligible applicants. However, in the event, only 251 people were randomly assigned to the treatment group and 245 to the control group. This was mainly due to funding constraints and difficulties in persuading employers to provide job placements during a recession. Recruitment problems also arose because some sites did not have good links with referral agencies.

Follow-up interviews were held 36 months after enrolment.

## **APPENDIX B**

## **APPENDIX B: GUIDELINES FOR APPROACHING SITE STAFF ABOUT RANDOM ASSIGNMENT**

This appendix is taken from Gueron (1999, pp. 9-11)

### ***General rules***

1. The right frame of mind is critical. Remember, you want them more than they want you. Even if initially they are eager, eventually they will figure out how much is involved and realize they are doing you a service if they say “yes.” Don’t say “no” to their suggestions unless they deal with a central element of the study (for example, no random assignment). You may well need to come back later with a modified design (for example, a different intake procedure) when the pickings of sites look slim. Remember to be friendly and not defensive. They really cannot know for sure what they are getting into, and their saying “yes” will be much more likely if they think you are a reasonable person they can work with over time.
2. Turn what is still uncertain into an advantage. When they raise a question about an issue that is not yet sorted out, tell them they have raised an issue also of concern to you and they can be part of the process of figuring out how to address it.
3. Make sure you understand their perspective. As much as possible, try to “think like them” so you will understand their concerns.
4. Never say that something about the research is too complex to get into. This implies they are not smart enough to understand it. Work out ways to explain complicated things about random assignment using straightforward, very concrete examples rather than research terms.
5. Be sensitive about the language and examples you use. Occasionally you will run into someone who has a research background and wants to use the jargon, but normal people are often put off by terms that are everyday, short-hand expressions to researchers. For example, many people find the terms “experiment,” “experimental,” “control group,” “service embargo,” and even “random assignment” offensive. Use more familiar, longer ways of saying these, even if they are less precise or even technically wrong. Site staff often react negatively to discussions of how random assignment is often used in medical research, probably because they are only familiar with outrageous examples.
6. If some issues are sure to come up (ethics, operational issues, site burden), raise them yourself. This shows that you understand the implications of random assignment, have grappled with them yourself, and think they can be addressed.
7. If pressed on an awkward issue about random assignment, do not give an evasive answer. For example, if site people forcefully ask if you really mean they will have to deny services

to those in the control group, say “yes.” Then, explain the reasons for the rule, and address the underlying concerns that led them to raise the question.

8. If someone is unreservedly enthusiastic about the study, he or she doesn’t understand it. While it might sound nice to let them cruise along happily, if their continued support matters, you must make sure they understand what they are getting into.

9. Make sure you highlight the benefits of participating. Usually, the key one is site-specific findings. Don’t mislead them or allow them to think they will get more than you can deliver. Often, they want a lot of “inside the black box” type results.

10. Negative momentum can occur and must be countered. If things start going bad in many sites, regroup and rethink the model and the arrangements you are offering before things get out of hand.

### ***Learning about the program***

1. Ask as many people as possible how the program works. Different perspectives are vital. You need to know things at a micro level that only local people can know.

2. Don’t rely too much on their estimate of participation rates. Unless they have an extraordinary management information system, most program operators have never had a reason to ask the type of client-flow questions needed to decide the details of a random assignment design.

### ***Developing the details of the model and closing the deal***

1. Operational issues are your problem, and you have to get them to buy into the study before they become their problem. You know you have made progress when they start helping you figure out how to address the problems.

2. Don’t be surprised by the level of “detail” you will have to address. Something that seems like a minor point to you from a research perspective may turn out to be a crucial operational barrier to putting the model in place. Try to learn the vocabulary about the “details” so they will realize you understand and take their issues seriously.

3. Realize that in working out procedures you will be dealing with people representing very different perspectives. Program directors worry about different things than managers or the line staff. Be sensitive to the differences in perspective, and realize that a good director may give the managers who represent the line staff a veto over participation if you cannot address their concerns. Support by an outside Board or director removed from program operations is not enough, although it is a start and will open the door. Administrative managers must be on board.

4. Protect the core of the study, and figure out what you can give on. Do not lose people over something not central. Depending on the study, non-central items might include: who controls lists of people referred for random assignment, exclusion of certain groups of people from random assignment, temporary changes in the random assignment ratio to assure an adequate flow of program participants, length of the service embargo for controls, limited services after random assignment for controls.
5. Sometimes the best response to a question about how a procedure would work is to ask a question in response. The goal is to develop procedures for the study that disrupt the program as little as possible. When they raise a tough operational issue, the starting point is what they normally would do if the study were not in place. So ask them, and then go from there. Often, this will suggest minor changes that everyone can live with.
6. Realize that model development is an iterative process. New issues come up over time that will need to be addressed. Expect a continued balancing between research preferences and operational constraints.
7. Develop a memorandum of agreement both parties can live with. Don't push or even allow a site to sign an agreement you think they cannot fulfil. A key factor to be realistic about is sample size. Don't set targets they cannot meet.
8. Money can often fix some problems, but don't get into a position where it looks as though you are trying to bribe them into betraying their ethics. Operational issues relating to staffing can often be helped by financial support. Serious ethical concerns cannot be addressed in this way.

### ***Community relations***

1. No news is good news. Imagine yourself as a reporter. Would you rather write about the human interest side of the study ("Poor used as human guinea pigs") or the abstract policy and research issues that motivate the study? You should expect most local news stories done before findings are available to be negative if the reporter understands what random assignment is.
2. Make sure the site knows you will take the bullets for them. Convince the site that they have a compatriot who will join the battle if things get rough.
3. There are pros and cons of you initially playing a prominent role in explaining the study. Ideally, it would be best if the site took the lead in building support for the study, because it shows they understand and really do support it. However, usually they can be surprised by local opposition or are not as good as you in explaining the reasons for the study or its



procedures. If there is doubt how a meeting will go, fight for a role without implying that the local people don't understand the study or know the local situation.

4. Be available to brief agencies affected by the study and advocates, but don't expect them to be won over instantly. It takes a long process to convince someone that this type of research is OK. Make sure site staff understand the pros and cons of outreach to other groups versus a low profile. Then let the site staff decide how to play this.

5. Prepare a press kit, and leave it up to the sites what to do with it. This should be viewed as a defensive rather than an offensive weapon, to be used if called for.

6. Develop a thick skin, and do not get defensive when speaking with the press or community groups. There is one exception: If your personal integrity is attacked, fight back. You are not a "Nazi."

7. Never say something is too complex to discuss or refuse to acknowledge key issues as legitimate. Ultimately, participation involves trust. Random assignment isn't business as usual, and site staff have to know you are leveling with them.

#### ***Training local staff on study procedures***

1. Taking the time to write a good manual, with examples, is time well spent. A detailed manual describing the study rationale and the intricacies of program intake and random assignment, and providing scripts for site staff, will serve as a valuable training tool and future reference for site staff.

2. Realize that the training may be the first time many have heard much about the study and that you must win them over. At the beginning of training, explain the reason for the study and random assignment and your common concern about people in the study. Try to get the site directors to lay the groundwork for the study and to show up at the training to indicate their support.

#### ***Setting the right tone for study implementation***

1. Program managers should understand that it is better to tell you about issues early, before they get serious and can threaten the study. Try to convince people that you might be a source of possible solutions, based on MDRC's past experience.

2. Make sure they understand you will show as much flexibility as possible on procedures. Sites that decide to participate sometimes come to view the initial procedures as holy writ.

They may nearly kill themselves trying to follow them without realizing you might be able to make a change that won't matter to the research but that will make their lives much easier.

They probably will have trouble distinguishing between rules central to the core of the study and those that can be played with at the margins.

## **APPENDIX C:**

Letters of notification of random assignment status

(Source: Orr, L. et al., 2000, Appendix D)

**Housing Authorities**  
**City and County of Fresno**  
Welfare-to-Work Voucher Study

**Date**

**SSN:**

*Dear*

Congratulations! You have been selected to receive a Welfare-to-Work rental assistance voucher. Please meet with a housing assistance specialist to receive your Welfare-to-Work voucher and obtain more information.

Sincerely,

NAME of HA representative ~

**Housing Authorities**  
**City and County of Fresno**  
Welfare-to-Work Voucher Program

**Date SSN:**

Dear :

We regret to inform you that you were not selected to receive a Welfare-to-Work rental assistance voucher. As you know, there were more applicants for assistance than the number of vouchers available. Therefore, the available vouchers were awarded through a lottery that gave every eligible applicant an equal chance.

Even though you will not receive a Welfare-to-Work voucher today, your name will remain on the regular Section 8 waiting list for further consideration and you will retain your eligibility for any other services offered by the Housing Authorities of the City and County of Fresno or other agencies. -

If you have questions about this decision with regard to your Welfare-to-Work voucher application or your continuing eligibility for regular Section 8 assistance, please feel free to call me at .

Sincerely,

NAME of HA representative

## **Control Group Notification Letter - Atlanta and Augusta Housing Authorities**

Date

Dear :

As we discussed during your interview when you applied for a Welfare-to-Work voucher, the Atlanta [Augusta] Housing Authority is participating in a special study to help us find out if the Welfare-to-Work voucher program is effective for families. Since a limited amount of assistance is available through the program, not everyone who applies will receive a rental assistance voucher. The final selection of voucher recipients is being made by lottery with each eligible individual having the same chance of receiving a voucher. The selection process is now finished, and you were not selected to receive a Welfare-to-Work voucher. The selection process was completely random and the fact that you were not chosen has nothing to do with your qualifications.

Even though you will not receive a WtW voucher, your name will remain on our Section 8 waiting list for further consideration. The fact that you were not selected to receive a voucher also will not affect your eligibility for any other services offered by the Atlanta Housing Authority or other agencies.

The information you gave us when you applied for the program will be used as part of the study mentioned above. A research firm may be contacting you in a year or two to ask you some more questions about your experiences. We hope you will agree to answer these questions since the study will help us to design better programs to help families moving from welfare-to-work. If you have any questions about the study or about your selection, please . call Michelle Wood, toll free at 888-304 2133.

Sincerely,

AHA signature

## **Control Group Notification Letter**

**[To Be Printed on Spokane Housing Authority Letterhead]**

Date:

Dear :

As we discussed during the orientation session today, the Spokane Housing Authority is participating in a special study to help us find out if the Welfare-to-Work voucher program is effective for families. Since a limited amount of assistance is available through the program, not everyone who applies will receive a rental assistance voucher. The final selection of voucher recipients is being made by lottery, with each eligible individual having the same chance of receiving a voucher. The selection process is now finished, and we regret to inform you that you were not selected to receive a Welfare-to-Work voucher. The selection process was completely random and the fact that you were not chosen has nothing to do with how qualified you are.

I

Even though you will not receive a WtW voucher, your name will remain on our Section 9 waiting list for further consideration. The fact that you were not selected to receive a WtW voucher will not affect your eligibility for any other services offered by the Spokane Housing Authority or other agencies.

The information you gave us today when you applied for the program will be used as part of the study mentioned above. A research firm may be contacting you in a year or two to ask you some more questions about your experiences. We hope you will agree to answer these questions since the study will help us to design better programs to help families moving from welfare-to-work. If you have any questions about the study or about your selection, please call Michelle Wood or Michael Baker toll free at 1- (888) 304 -2133.

Sincerely,

**[SUA Signature]**

## **APPENDIX D:**

**Supplemental interview form for Project Network**

(Source: Abt Associates, 1992, pp 13-18.)



**PROJECT NETWORK SUPP. ENTAL INTERVIEW FORM (SIF)**  
 Instructions on Reverse Side

<b>A. Background Information</b> CM Initials: _____ FO #: _____ 3-Screen Interview Date: ____/____/____ Name of Network Applicant: _____ SSN: _____ Date of Birth: ____/____/____		Check one: <input type="checkbox"/> New SSI Applicant <input type="checkbox"/> Current SSDI/SSI Beneficiary/Recipient
---	--	--

**B. Disability Concerns**  
 If possible, please provide any details on the Project NetWork applicant's special needs for participating in an hour-long interview. For example, a sign language interpreter, a family member or friend to interpret speech, or someone who can answer factual questions for the applicant if he/she suffers from a severe cognitive impairment. If an interpreter or proxy is needed, indicate, if possible, who might be appropriate and available for this role, and his/her phone number if different from the applicant's.

\_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

**C. Other Concerns**  
 If possible, please indicate the applicant's primary language (if not English or Spanish) and whether a possible interpreter lives in the home. Add any further comments on the applicant's living situation or other unusual circumstances that might be helpful in preparation for the interview.

	Yes	No	Don't Know
Is applicant fluent in English?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Is applicant fluent in Spanish?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Does the applicant's condition pose any danger to the interviewer?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Does the applicant have a phone?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Are there restrictions in the applicant's time availability?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**D. Please list three persons who will always know how to locate the applicant; please do not include persons who live with the applicant:**

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_  
 Address: \_\_\_\_\_ Phone: (\_\_\_\_) \_\_\_\_\_ - \_\_\_\_\_ If no phone, check here

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_  
 Address: \_\_\_\_\_ Phone: (\_\_\_\_) \_\_\_\_\_ - \_\_\_\_\_ If no phone, check here

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_  
 Address: \_\_\_\_\_ Phone: (\_\_\_\_) \_\_\_\_\_ - \_\_\_\_\_ If no phone, check here

**INSTRUCTIONS FOR COMPLETING THE SUPPLEMENTAL INTERVIEW FORM  
FOR PROJECT NETWORK APPLICANTS**

**Section A:**

Fill in Case Manager Initials, Field Office Number, and the 3-Screen Interview Date. The 3-Screen Interview Date is the date when the CM interviews the Project Network applicant to obtain the information for the CMCS. Check whether the Project Network applicant is a new SSI applicant or a current SSD/SSI beneficiary/recipient. Then provide her/his name, SSN, and date of birth for identification purposes.

**Section B:**

The intent of this section is to collect information that will help the interviewer prepare for scheduling and conducting the interview. Provide any details on the applicant's disability that might affect our ability to conduct an hour-long interview, and if possible, provide any suggestions for meeting the applicant's special needs. For example, if the applicant needs assistance communicating and you can provide the name of someone who might assist, please provide the person's name and how we might contact him or her. Also, if you believe the applicant to be incapable of participating in an interview because of mental illness or impairment please comment on the condition and, if possible, suggest an appropriate proxy (i.e., a person close to the respondent such as a family member who can participate in the interview on behalf of the respondent) whom we might contact to complete the interview.

**Section C:**

Provide any other information that might cause difficulty in attempting to schedule or conduct the interview. Examples include applicants who speak neither English nor Spanish, applicants taking medication which affects their mental condition, applicants who might be potentially violent or who live in possibly unsafe neighborhoods, applicants who do not have a telephone, and applicants who are unavailable at specific times of the day.

**Section D:**

Provide information for three persons who might be contacted if we are unable to locate the respondent directly. These persons should not live in the same household with the respondent and might include family, friends, physicians, or therapists.

If there are any questions about this form call the Project Network Hotline: 1-800-892-9456.

Keep the bottom copy for Field Office records and send the top copy to Abt Associates Inc. at:

**101 North Wacker Drive  
Suite 400  
Chicago, Illinois 60606-7301  
Attn: Mary Hess**

**Supplemental Interview Form  
Question-by-Question Instructions**

<b>Section A: Background Information</b>		
RM Initials: _____	FO #: _____	3-screen Interview Date: ____/____/____
Name of Project NetWork Applicant: _____		SSN: _____
Date of Birth: ____/____/____		
Check one: <input type="checkbox"/> New SSI Applicant		
<input type="checkbox"/> Current SSDI/SSI Beneficiary/Recipient		

Section A of the Supplemental Interview Form (SIF) asks you to provide basic, identifying information about the Project NetWork applicant. This information will ensure that the SIF can be linked to the applicant's CMCS record. Instructions for specific questions are provided below:

**RM Initials.** The referral manager should write his or her initials in the space provided.

**FO #.** This is your SSA Field Office number. [Tampa Downtown = Q48 and Carrollwood = Q49]

**3-screen Interview Date.** This date refers to the date on which you interviewed the applicant to obtain the information for the CMCS. Record month first, then day, and then year. [NOTE: This date may differ from the Record Date established on the CMCS.]

**Name of Project NetWork Applicant.** The applicant's first name should be printed first, followed by middle initial and last name.

**SSN.** This refers to the applicant's Social Security number. Check the number against the SSN entered into the CMCS to make sure that it is entered correctly here. Please make sure the number is accurately recorded, that numbers are not transposed, and that all nine numbers are recorded legibly.

**Date of Birth.** Month, day, and year of birth.

**Type of Beneficiary/Recipient.** Check the appropriate box to indicate whether the individual is a new SSI or SSI/SSDI concurrent applicant or a current SSDI beneficiary or SSI recipient.

**Supplemental Interview Form  
Question-by-Question Instructions**

**Section B: Disability Concerns**

If possible, please provide any details on the Project NetWork applicant's special needs for participating in an hour-long interview. For example, a sign language interpreter, a family member or friend to interpret speech, or someone who can answer factual questions for the applicant if he/she suffers from a severe cognitive impairment. If an interpreter or proxy is needed, indicate, if possible, who might be appropriate and available for this role, and his/her phone number if different from the applicant's.

Section B of the SIF asks you to provide information on the applicant's disability and special needs for participating in an interview. You should provide any details on the applicant's disability that might affect our ability to conduct an hour-long interview, and if possible, provide any suggestions for meeting the applicant's special needs. For example, if the applicant needs assistance communicating and you can give the name of someone who might assist, please provide the person's name and telephone number. If you believe the applicant is incapable of answering factual questions because of a severe cognitive impairment, please comment on the condition and, if possible, suggest an appropriate proxy (i.e., a person close to the respondent such as a family member who can answer these questions on behalf of the respondent) whom we might contact to complete the interview.

**Supplemental Interview Form  
Question-by-Question Instructions**

Section C: Other Concerns	Yes	No	Don't Know	If possible, please indicate the applicant's primary language (if not English or Spanish) and whether a possible interpreter lives in the home. Add any further comments on the applicant's living situation or other unusual circumstances that might be helpful in preparing for the interview.
Is applicant fluent in English?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Is applicant fluent in Spanish?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Does applicant's condition pose any danger to interviewer?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Does applicant have a phone?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	
Are there restrictions in the applicant's time availability?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	

Section C of the SIF asks you to provide details on the applicant's primary language and other aspects of the applicant's condition and living situation that might affect the interview. You should check all appropriate boxes to give complete information about aspects of the applicant's situation that may affect the way in which the interview is conducted. Please check one box in each row. Instructions for specific questions are provided below:

**Is applicant fluent in English?** Indicate whether the applicant is comfortable carrying on a conversation in English. We want to make sure that our interviewer will be able to conduct the interview in the language in which the applicant is most comfortable.

**Is the applicant fluent in Spanish?** Indicate whether the applicant is comfortable carrying on a conversation in Spanish. If the applicant is not comfortable with either English or Spanish, please indicate his or her primary language.

**Does the applicant's condition pose any danger to the interviewer?** Indicate whether the applicant has ever put RMU staff in danger or had to be supervised during interviews.

**Does the applicant have a phone?** Indicate whether the applicant has a telephone.

**Are there restrictions in the applicant's time availability?** Indicate whether there are certain times of the day during which the applicant may not be available for an interview due to schedule constraints or patterns in medical symptoms or medications over the course of the day. Please indicate in the comment space which times of day the applicant is not available.

**Supplemental Interview Form  
Question-by-Question Instructions**

**Section D: Contact Information**

Please list three persons who will always know how to contact the applicant: please do not include persons who live with the applicant:

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_

Address: \_\_\_\_\_ Phone: \_\_\_\_\_ If no phone, check

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_

Address: \_\_\_\_\_ Phone: \_\_\_\_\_ If no phone, check

Name: \_\_\_\_\_ Relationship to Applicant: \_\_\_\_\_

Address: \_\_\_\_\_ Phone: \_\_\_\_\_ If no phone, check

The final section of the SIF asks you to provide contact information to help locate the applicant for later interviewing.

**Names, addresses and telephone numbers of three relatives, friends, care-givers.** The contact information we ask for is very important. We need to know how to get in touch with Project NetWork applicants to conduct interviews. If an applicant moves and does not leave a forwarding address, the only way we can locate her or him is by asking friends, relatives, care-givers, and other acquaintances outside of the applicant's household. **These contact persons will not be called unless absolutely necessary.**

Our previous experience has indicated that close relatives (parents, siblings) and long-term friends are the most useful in locating the applicant. **You should list only those relatives and friends who do not live at the same address as the applicant or with each other.**

This information will be kept strictly confidential and used only if we have trouble locating the applicant.