

## WHY LAUNCH A BASIC INCOME EXPERIMENT?

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## CHAPTER 4

### WHY LAUNCH A BASIC INCOME EXPERIMENT?

#### 1. INTRODUCTION

The limited support for BI and the strong support for workfare-oriented policies (see chapter 1, Table 1) may well explain why there are no BI experiments but many, maybe thousands, welfare-to-work oriented experiments going on. BI and workfare can be seen as opposed ways to achieve more flexible labour markets. Peck and Theodore (2000, 124) assert that welfare-to-work experiments ‘seek to articulate a regulatory strategy concerned to make flexible labour markets work. “Work first” approaches, in particular, can be seen as part of a wider attempt to realign welfare provisions, incentive structures and work expectations in light of the “realities” of flexible employment; their aim is to (re)socialise welfare recipients for contingent work.’ However, as argued in the previous chapters, a BI can also be seen in the light of flexibility. Under a BI scheme a more flexible labour market may arise because of the elimination of minimum wage legislation and a less comprehensive legislation on employment conditions. By providing a BI unconditionally, the income and utility which potential workers derive from this no-work option serves as a floor. In principle at least, a substantial BI allows the possibility to deregulate the labour market, and in this way to combine the dynamics of American labour markets with the minimum income protection of European welfare states (see also chapter 2).

Comparing BI and workfare (or the shift towards activating labour policies replacing passive welfare), it is interesting to note that a BI experiment may serve as the right counter-experiment for all kinds of workfare-oriented experiments. As argued by Peck and Theodore (*ibid.*, 124-5), the recent popularity of workfarism in the US and the UK can to a large extent be attributed to the positive results of local workfare experiments in the early 1990s. Workfare experiments show the effects of mandatory welfare-to-work programmes compared to the normal treatment (e.g. the duty to apply for jobs, the duty to resume work as soon as possible) of a control group of welfare beneficiaries. Running a workfare and BI experiment simultaneously may show what a difference it makes if recipients must participate, as a condition of income support, in programmes designed to improve their insertion in paid work as under workfare, or if they can freely choose themselves what to do as under BI. It seems reasonable to maintain the services of job training and job counselling even for those receiving a BI if they need help to find a job, although making use of these services is on a voluntary basis. My proposal is to give the experimentals receiving a BI the same per capita value of the cost of these services in the form of a voucher. Because there are no BI experiments going on, we can only guess what the differences would be. For instance, it may well be the case that workfare experiments show better results in terms of labour market inclusion, but

that BI experiments show better results in terms of inclusion in all kinds of unpaid work. In any case, comparing the evaluation findings of workfare and BI experiments may give us some information about the effectiveness of welfare-to-work activities performed by employment agencies.

The structure of this chapter is as follows. Section 2 contains a non-exhaustive enumeration of the limitations of existing research to assess the effects of a major change in social security. Section 3 shows the equivalence between a BI and a negative income tax (NIT). Section 4 discusses the New Jersey negative income tax experiments. Although these experiments were held over a quarter of a century ago, some important lessons can still be drawn for new experiments to be initiated in the future. These are presented in section 5. Section 6 presents a structure for a new BI experiment, which can serve as a basis for discussion about any proposal to start such an experiment. In the final section the conclusions are elaborated.

## 2. THE LIMITATIONS OF THEORETICAL MODELS AND EMPIRICAL RESEARCH

On the basis of *theoretical* microeconomic research<sup>1</sup> something can be said about the direction of the expected effects, but not about the scale of these effects. Although the economic sustainability of a BI is controversial, there are some uncontroversial remarks which can be made. Firstly, the implementation of a BI will reduce the share of GDP which is distributed by the market. Consequently, *on average* a given work effort will be less rewarded when compared to the rewards accruing to the same amount of labour in a scheme of conditional social security. The ultimate effect does however not depend on the higher average tax rate, since the burden (especially of the marginal tax) varies according to one's position in the labour market. Social security recipients now face an effective tax rate equal to 100%, while part- and full-time workers face a much lower rate. The flat tax in the standard BI proposal entails a marginal rate which is comparable across members of society with a low and with a high income. One of the crucial questions therefore is whether the negative effects of a higher average tax rate for the latter group is greater than the positive effects of a lower marginal rate for the former group. Still, even more serious is that economic theory does not yield unambiguous clues about what we can expect for the effect of BI on human capital accumulation (see below), on low wage levels in the absence of minimum wage legislation and on female labour supply.

In the long term at least three effects can be distinguished which will influence human capital formation and hence the distribution of earning powers in the future under a BI scheme. Due to the raising of tax rates required to finance the BI, the net after-tax wage rate will probably be lower under a BI scheme for most workers. This may give a disincentive to invest in human capital, since every unit of human capital will then generate a lower stream of net earnings in the future. However, this is not the whole story. Two counteracting forces work to lower the cost of acquiring

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<sup>1</sup> See e.g. Besley (1990) and Creedy (1996).

human capital: (i) students over the age of 18 engaged in schooling will receive a BI, whereas most of them now have to incur large debts to finance their study; (ii) with lower net wage rates due to higher tax rates, the foregone earnings of full-time schooling become smaller.<sup>2</sup> Even if we had reliable forecasts about future net wage differentials between educational categories, we would also need to know the effect of monetary incentives on human capital formation (i.e. the allocation of students among educational categories). In sum, one cannot treat earning power, or wage rates, as exogenous in the long run.

It would be helpful if we would know what the effects are of abolishing the minimum wage, eliminating the poverty trap and the effect arising from the absence of preconditions on the behaviour of social security recipients for receiving a social benefit.<sup>3</sup> Not long ago, we have seen a flourishing, yet unresolved, debate on the effect of the level of minimum wages on employment.<sup>4</sup> Note that this debate is about the effect of a small change of minimum wages on employment. What is required here is an estimate of the effect of a complete elimination of minimum wages, in conjunction with the effect of the removal of the poverty trap and making the minimum income guarantee unconditional, on labour demand and labour supply which together will determine the new equilibrium values of wages and employment in the low wage sector under a BI scheme.

Finally, it is much more difficult to model the process leading to changes in the distribution of family income and decisions of family members with regard to labour supply than changes in individual income and labour supply.<sup>5</sup> A standard neo-classical labour supply model where all individuals are taken alike would probably generate entirely different outcomes compared to when one models family behaviour, e.g. when using the male chauvinist model. What is at stake here is the radical uncertainty regarding the effect of a BI on the division of labour within the household. This uncertainty is perhaps responsible for the fact that the feminist movement has not yet taken a clear stance on the BI proposal. There is the fear that the participation rate of women will decline because of the BI: the income loss (or opportunity costs) of not doing paid work becomes less, and a BI can be seen as a

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<sup>2</sup> A similar point is made by Atkinson (1995a, 135): ‘If the decision is based solely on comparing the expected gain in earnings with the earnings foregone while training, a tax which is simply proportional would reduce both by the same percentage, and the balance in the equation is unaffected. If the tax is at rate  $t$ , we would simply have a factor  $(1-t)$  appearing on both sides. It is only to the extent that the tax has a graduated marginal rate, falling more heavily on the earnings of trained labour, that the return to training is reduced. Of course this is an over-simplified representation, and costs such as university fees may well not be tax-deductible, but the essential point is that human capital investment largely takes the form of foregone earnings, so that if these earnings would have been taxed, the cost of the investment is reduced as well as the benefits.’

<sup>3</sup> Present social benefits are surrounded by all kinds of obligations (to apply for jobs, to retrain, to fill in forms every month, etc.), whereas a BI can be seen as a kind of (anonymous) gift. The sociological gift-exchange theory predicts that gifts have the tendency to elicit a counter-gift.

<sup>4</sup> See e.g. Kennan (1995), Dolado *et al.* (1996), Greenaway (1996) and Card and Krueger (1995).

<sup>5</sup> ‘To understand family income, one would have to understand not only the process generating other private income sources (dividends, interest and rent) and public income sources, but also the joint decision-making process among family members who adjust their labor supply, human capital, household formation and childbearing decisions in reaction to changes in outside sources of income, as well as to changes in the earnings of other family members’ (Gottschalk 1997, 22).

disguised wage ('hush money') for housekeeping and childrearing activities. However, even if this fear were realised one can still argue that not much is lost from the perspective of female emancipation. It is likely that a significant part of the decline in women's labour supply would result from women with easy, dead end and low paid part-time jobs quitting their jobs. It is not very likely that women with interesting, well paid full-time jobs would stop working (De Beer 1987, 52-53). Moreover, for both men and women who perform a full-time job it becomes more attractive to work less than full-time: since the share of labour market earnings in household income would become less under a BI scheme compared to the present scheme, the same reduction in working hours will lead to a smaller decline in household income under a BI scheme. This may increase the willingness of men to take a larger share of housekeeping and childrearing responsibilities by working less than full-time. Finally, it may well be the case that because of the abolishment of minimum wages more low wage and part-time jobs will become available for women under a BI scheme. The BI could then be seen as a kind of emancipation fee.<sup>6</sup>

*Empirical* research on labour supply shows a large variety of outcomes on labour supply decisions resulting from changes in the tax and transfer system. At best, empirical research of this kind can only give reliable estimates for small changes in marginal tax rates, or for small changes in the level of social benefit levels. Attempting to predict the social and economic effects of a major switch from conditional to unconditional social security is a different matter. Therefore, results obtained from empirical research into the effects of benefits, taxes and premiums on labour supply and labour demand only offers insight into the effects of those policy changes which do not cause a fundamental break with the existing system, such as a limited change in benefit levels or tax rates. On the basis of such research no statement can be made regarding the consequences of the completely different arrangement of the social security and tax systems resulting from the introduction of a BI. According to Barry (1997, 161):

... no tax and benefit simulation, however conscientiously carried out, can make allowance for the changes in behaviour that would arise under an altered regime. A subsistence-level basic income would face people with an entirely different set of opportunities and incentives from those facing them now. We can speculate about the way in which they might respond, but it would be irresponsible to pretend that by cranking a lot of numbers through a computer we can turn any of that into hard science.

To put it in a terse phrase, there is no hard science concerning the effects of a BI scheme.

Interviewing a representative sample of the population to survey public opinion is likely to be of little use either. We run the risk that people do not answer with complete honesty but give a socially acceptable answer. It is also quite likely that many people do not really know how they would react to the introduction of a BI,

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<sup>6</sup> See Robeyns (2000) for a more extensive gender analysis of the responses of women (specified by groups distinguished by earning capacity and labour market attachment) to the introduction of a basic income.

because it differs so much from the existing system. It is only when the consequences of a BI are personally experienced that the real meaning of a BI is fully realized and an appropriate answer can be given. The only reasonably trustworthy way to make a statement about the consequences of the introduction of a BI is conducting a field experiment. Such an experiment would involve a limited group of people in a limited area who would, during a limited time, receive a BI. By closely following and analysing the behaviour of this group of experimentals in comparison with a group of controls, not receiving a BI but for instance being subjected to a workfare scheme, we may get some additional insight into the effects of a BI on people's behaviour. Additional, because the information would be complementary to what can be concluded from back of the envelope calculations on the feasibility of BI, and more important, to the findings obtained from sophisticated models simulating an economy with unconditional grants replacing the present scheme of conditional benefits<sup>7</sup> and to the findings of empirical research on labour supply. There are numerous factors at work which influence labour supply decisions. One cannot hope to include all these factors simultaneously within the confines of an economic model. Economic models can, at best, isolate the effects of a few of these factors. An experiment may enable us to solve part of the puzzle, because the limitations of an experiment are of a different nature than those of economic models, whether theoretical or empirical. The main difference is that models rely on assumptions, whereas an experiment allows one to *directly observe* changes in labour market behaviour.

### 3. BASIC INCOME VERSUS NEGATIVE INCOME TAX

In this and the next section we are mainly dealing with the negative income tax (NIT). Any single tier NIT-scheme can be described by the level of the income guarantee and the withdrawal rate. Both a NIT and a BI provide a guaranteed minimum income to individuals or households, independent of labour market history or current labour market status, and without any work requirement. Whereas the BI is provided to all irrespective of the level of gross income, the level of the NIT depends on gross income. This may seem a large difference, but as Van Parijs (1992, 4) pointed out, both can yield exactly the same distribution of post-tax-and-transfer incomes. This is illustrated in the figure below.

If we take  $t$  as the tax rate,  $y$  as the gross income,  $\tau$  as the tax liability,  $B$  as the level of BI and  $N$  as the guarantee level of the NIT, the tax functions for BI can be written as  $\tau_{BI}(y) = ty$  (which corresponds to the horizontally shaded area in panel A of the figure) and for NIT as  $\tau_{NIT}(y) = -N + ty$  (which corresponds to the vertically shaded area in panel B of the figure as long as gross income is below break-even and to the horizontally shaded area if gross income is above break-even). Net disposable income  $y_d$  can then be written as:

$$y_{d,BI} = y + B - \tau_{BI}(y) = y + B - ty$$

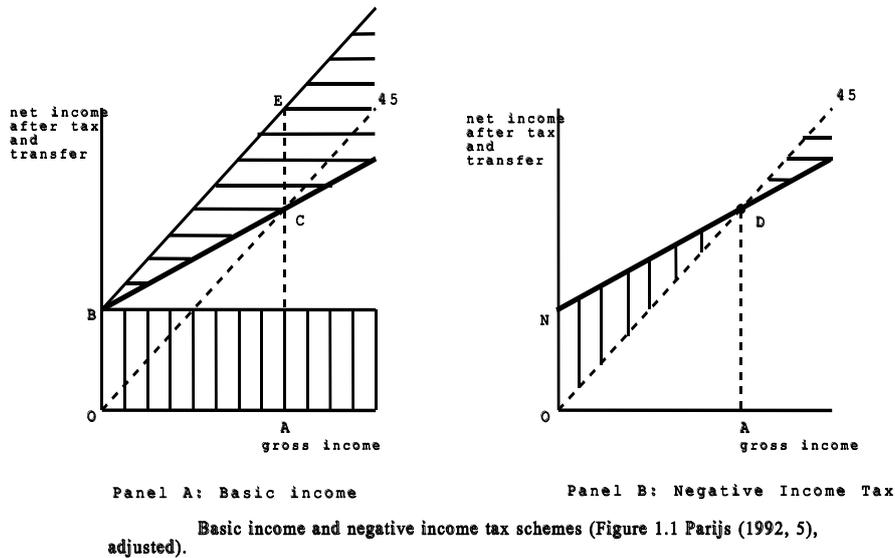
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<sup>7</sup> See the simulation results of Atkinson and Sutherland (1988) and Atkinson (1995a) using TAXMOD and of Gelauff and Graafland (1994) using the model MIMIC of the Dutch Central Planning Bureau.

and

$$y_{d,NIT} = y - \tau_{NIT}(y) = y + N - ty$$

Figure 1. Basic income vs. negative income tax



As can be clearly seen, the functions for net disposable income for equivalent (that is,  $N = B$  and equal tax rates) NIT and BI schemes are the same. The break-even level of income for the NIT and BI scheme can then be determined by the point at which net and gross income are equal, that is equating net disposable and gross incomes in the functions of net disposable income (graphically, the points of intersection C and D of the bold lines representing the post-tax-and-transfer income at various levels of gross income and the 45 degree line from the origin). For the NIT and the BI scheme the break-even level of gross income equals  $N/t$  and  $B/t$  respectively.

All single-tier NIT and BI schemes can thus be defined by two variables, the guarantee level and the tax rate. However, under a NIT scheme, the guarantee level is only paid to those individuals or households without any income, whereas the BI received does not depend on the level of gross income. The tax rate of the NIT can be considered as a kind of withdrawal rate as long as gross income is below the break-even level, because the amount of NIT paid by the government is reduced by that rate as income rises. Above the break-even level of gross income, the tax rate is just the normal rate at which gross income is taxed. As can be seen from the figure,

the break-even point D is the point at which the individual or household neither receives a transfer payment (the NIT proper), nor has to pay taxes. This corresponds with point C in panel A, where the tax liability CE exactly equals the amount of BI received. The main difference between both schemes is therefore purely administrative, namely whether transfer payments are made *ex ante* (BI) or *ex post* (NIT). In the Introductory chapter, Van Parijs gives three disadvantages of a NIT compared to BI following from the administrative difference. My proposal is to let participants in the experiment choose themselves whether they want a NIT or a BI.

#### 4. THE NEW JERSEY INCOME-MAINTENANCE EXPERIMENT

The New Jersey income-maintenance experiment can be considered as one of the first controlled large-scale field experiments in the field of economics. The details of this experiment are well documented in the three volumes edited by Kershaw and Fair (1976, Vol. I) and Watts and Rees (1977a, Vol. II and 1997b, III).<sup>8</sup> For sake of brevity, I will refer to these three volumes as I, II and III. The principal intention of the experiment was to get reliable information about the work incentives of the non-aged poor under a transfer scheme with an unconditional guaranteed minimum income around the poverty level (I, xiv). At that time, the NIT was taken as a serious alternative to the existing social legislation in the fight against poverty.<sup>9</sup> The prevailing policy to fight poverty used the conventional instruments of education, manpower training and public employment programs. It was a policy based on self-help and self-improvement, and its goal was to make 'tax payers' out of 'tax eaters' (Lenkowski 1986, 39). One of the factors responsible for the rising popularity of the NIT scheme as set out by Nobel laureates Milton Friedman (1962) and James Tobin (1966) was that the spectre of means-testing, causing more harm than good, seemed to come true: '... Negro men were unable to earn enough to support their families and so left home in order that their wives and children might obtain relief' (*ibid.*, 36).<sup>10</sup> The crisis of the welfare state, the welfare 'mess', induced the President to appoint a Commission on Income Maintenance Programs to 'examine any and every plan, however unconventional' (II, xxiii). The sharp rise in the number of people enrolled in welfare programs created the right atmosphere to look for another approach:

... rather than making welfare benefits harder to obtain, policy should aim at making it more appealing to give them up. Programs should be designed so that recipients would

<sup>8</sup> For other sources, see e.g. Masters and Garfinkle (1977), Burtless and Hausman (1978), Keeley *et al.* (1978) and Munnell (1987). See Widerquist (2004) for a (20 pages long) bibliography, academic as well as non-academic articles, on the NIT experiments.

<sup>9</sup> Two welfare reform bills issued by President Nixon embodying the idea of a NIT, the Family Assistance Plan and H.R. 1, even passed the House in 1970 and 1971 respectively, but both were ultimately rejected by the Senate. For details about the political stratagems going on behind these proposals, two books can be highly recommended: Moynihan (1973) and Lenkowski (1986).

<sup>10</sup> See also the figure in Moynihan (1973, 83) which illustrated the following: 'As male unemployment rates had gone up, so had the number of new AFDC cases. Down, down. Up, up. The correlation was among the strongest known to social science... Then with the onset of the 1960s the relationship weakened abruptly, and by 1963 vanished altogether. Or, rather, reversed itself. For the next five years the nonwhite male unemployment rate declined steadily and the number of AFDC cases rose steadily.'

always have a financial incentive to rely on something other than welfare benefits. Moreover, this should be done not by lowering benefits themselves (which would have been unfair to the 'truly needy'), but preferably by reducing them by less than the full amount of any additional earnings or other income a recipient might have. For every extra dollar or pound gained, the public support a person had been drawing would be reduced by, say, half as much, leaving him better off in total than before. Past a certain point, he would no longer be eligible for any assistance at all and instead would start paying taxes, ideally at the same rate by which his benefits had been lowered. This idea was most widely known as the negative income tax; if it was applied properly, its proponents argued, only those who were really unable to support themselves would remain on relief (Lenkowski 1986, 36).

The failure<sup>11</sup> to reduce (the rise in) the number of welfare recipients, despite the introduction of more severe conditions of entitlement and special programmes designed to help the poor to help themselves, and the rather optimistic view<sup>12</sup> on the behaviour of welfare recipients under a NIT scheme, mobilized enough support for the NIT-experiments.

Before more detailed information about the design of the NIT-experiments in the USA is presented, there are a few peculiarities which must be kept in mind when assessing the relevance of the experiments for the European context. Firstly, the population on welfare at the time the experiment started consisted mainly of female-headed families, since men were not entitled to social assistance (the only ones who could receive benefits were those with an unemployment or disability insurance (I, 9)). This is of course a major difference with the context in which an experiment nowadays would operate. The men enrolled in the New Jersey experiment were confronted with the fact that for the first time in their life they would receive welfare benefits, even if work was voluntarily abandoned, if they had no unemployment insurance and if they were not prepared to do any paid work at all.<sup>13</sup> This may lead to a higher estimate of the negative labour supply response than what we would expect from the introduction of an unconditional scheme today in Europe. Nowadays healthy men passing the means-test without any current income are entitled to social assistance. Those with a high preference for leisure and who have managed to be on the dole cannot reduce their zero labour supply any further when conditional social security is replaced by a BI scheme. The effect of cutting back hours of work among those with a low preference for paid work when easy accessible social security becomes available has to a large extent already manifested itself.

Secondly, the primary focus of the experiment were the labour supply responses to providing unconditional social security to low income *male* earners. Female

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<sup>11</sup> 'Dependency had become a social condition beyond the apparent power of social policy to affect, save possibly at the margin. *This was the heart of the Administration's understanding of the matter.* It is not a judgement that will be found in the archives. It was not even a judgement. It was simply an awareness of the limits of knowledge that gradually emerged and thereafter did not need to be dwelt upon or even acknowledged' (*ibid.*, 353).

<sup>12</sup> The NIT-plan was not 'predicated on the assumption that people don't want to go to work' (Moynihan 1973, 340).

<sup>13</sup> Lenkowski (1986, 56) states that '... one of the bedrocks of the existing policy [was] the tradition of not providing income on the basis of need to those able to work'.

headed families were already entitled to AFDC-benefits (Aid to Families with Dependent Children), and the Social Security Act of 1967 contained a kind of anti-cumulation measure to limit the effective (withdrawal) tax rate on AFDC benefits to 67% (I, 10). Giving these women the opportunity to enrol in a NIT-experiment would probably generate little additional information compared with what was already known. For these reasons, and because the female participation rate was low, it was decided not to include female-headed families in the experiment.

Thirdly, the ethnic composition of whites, blacks and Spanish-speaking of both the treatment and control groups was roughly one-third for each over all cities with a NIT-experiment (I, Table 2.3, 36). Labour supply responses turned out to be significantly different for each of the major ethnic groups (II, 77-85). It is likely that the ethnic heterogeneity of an experiment in Europe will be much lower.

Adding the cultural differences between the USA and Europe, the long time which passed since the experiments started, the gradual improvement of conditions of employment since then,<sup>14</sup> the overall decrease of the working week, and the relatively low poverty level compared with the present social minimum at which the minimum level of benefits is now pitched, means that the outcomes of the experiment are of limited use for answering the question whether the introduction of a BI or NIT around the social minimum in Europe today would have a detrimental effect on labour market participation. Atkinson (1995a, 150) states that 'The NIT experiments are generally considered to have reduced the range of uncertainty surrounding the response of hours of work to taxation...' However, '... there is no necessary reason to expect the results to apply equally in a European context. Those interested in a BI/FT [BI/flat tax] scheme in Europe might like to consider launching such an experimental research project, which would serve both to throw light on the economic effects of the reform and to demonstrate how it would work in reality.'

For readers unacquainted with the outcomes of the experiments, I quote the major findings:

The most important group... the experiment was specifically designed to examine, is that constituted by the non-aged, able-bodied males with family responsibilities. These are the people with the most labor to withdraw. These are the people about whom there is the most widespread fear that, given an income alternative, they will decide not to work. As it turned out, the effect for this group was almost undetectable... the employment rate for male family heads in the experimental group was only 1.5 percent less than that for the controls. For the number of hours worked per week, the difference amounted to just over 2 percent... The second group in terms of policy interest is the wives. The average family size in the sample was six, so the wives in the experiment were, on average, mother of four children. For this group, the differential between experimentals and controls was substantial, with experimental wives working 23 percent fewer hours per week than the controls, their employment rate being 24 percent less, and their average earnings per week totalling 20.3 percent less. This can be regarded as a desirable outcome, given the fact that wives in six-person families work hard inside the home, and that this work could well be more beneficial (cost-effective)

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<sup>14</sup> Better conditions of employment reduce the role of net wages as an incentive to elicit work effort and labour supply. Hence, lower net wages due to the higher required tax rates to finance a BI or NIT scheme will have a lower negative effect on labour supply.

from a national point of view than low-wage market labor. It should be noted, in addition, that although this relative reduction is large, it in fact starts from an average figure of only 4.4 hours a week... In the area of psychological and sociological responses, the effects were negligible. Cash assistance at the levels involved in this study does not appear to have a systematic impact on the recipients' health, self-esteem, social integration, or perceived quality of life, among many other variables. Nor does it appear to have an adverse effect on family composition, marital stability, or fertility rates (I, 20-21).

4.1. *The design of the New Jersey experiment*

Table 1 contains the parameters of the eight NIT-experiments, varying in income guarantee levels and withdrawal rates, which together constituted the graduated work incentives experiment in New Jersey.

Table 1. *The Negative-Income-Tax Plans Used in the New Jersey Experiment (I, Table 1.3, 10, adjusted.*

<i>Income Guarantee (in % of poverty line)</i>	<i>Withdrawal rate (in %)</i>		
	30	50	70
50	X	X	
75	X	X	X
100		X	X
125		X	

The plans in the upper right corner with high withdrawal rates and low income guarantees are least attractive and in the lower left corner most attractive to participants. To minimize transfer costs (the income support received), low income families were more than proportionally allocated to the least attractive plans in the top rows and higher income families to the most attractive schemes in the bottom rows (I, 13, 96-97). Aside from prospective transfer costs, the assignment of the number of families to each plan was determined by a policy weight given to each plan (the 75-50 and 100-50 plan got the highest weights) and by the expected attrition rate (drop out was expected to be inversely related to the generosity of the plan, and positively related to pre-enrolment annual income).<sup>15</sup> At the end of the

<sup>15</sup> The highest attrition or drop out rate, 50%, was expected for the controls, since they would only receive the fees for filling the forms and interviews. This is one of the reasons why the number of controls (632) is almost as large as the number of experimentals (725).

experiment, the overall attrition rate was 20%; 25% for the controls and 16% for the experimentals (I, 105). As could be expected, the attrition rate was higher the lower the guarantee level, the higher the withdrawal rate of the plan and the lower the amount of the last transfer payment (I, Tables 7.3-7.5, 109-111). Most impressive is the amount of effort spent on keeping attrition to a minimum and the quest to recover data on families who left, even outside the USA. At stake here is not only the representativeness of the empirical outcomes of the experiment, but also whether the outcomes could serve as reliable estimates for the costs of a national programme.<sup>16</sup>

Sample eligibility was restricted to families having one healthy man and with a family income below 150% of the poverty line (I, 8).<sup>17</sup> Two reasons were given for this decision:

First, those close to the field operators wanted to be sure that most of the sample would qualify for significant payments to keep the goodwill of the experimental participants and to minimize the number who dropped out during the experiment. Second, OEO [the official sponsor of the experiment] did not want to be in the position of funding a cash program that was primarily addressed to the nonpoor (I, 10).

Another choice was between running the experiment nation-wide with participants all over the USA or running it in compact geographical areas. There were two reasons that led to the latter choice. First, administrating and monitoring the experiment was much easier when all enrolled families live in the same area. Second, a nation-wide experiment has the disadvantage that participants operate on different geographical labour markets which necessitates to disentangle geographical labour market effects from individual labour supply responses. To compensate for the loss of randomness, New Jersey was chosen because the unemployment rate there was close to the national average.

#### *4.2. The operations, surveys, and administration*

The main purpose of conducting an experiment is to collect information. Obviously, this is very costly. The actual (and budgeted) administrative and research costs of the experiment were more than twice the actual transferred income support payments! (I, Table 1.6, 18). The experimentals in the NIT-experiment were interviewed more than twenty times: a (44-question) screening interview to determine eligibility, a (340-question) pre-enrolment before, and a follow-up

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<sup>16</sup> 'The people who drop out of the experiment may be the same as those who fail to be included in a national program (those who fail to register and those who fail to report their income or in other ways fail to maintain their right to benefits). To the extent that this is the case, the behavioral responses measured in the experiment will be a good measure of the responses that may be expected in the population as a whole. To the extent, however, that the people who drop out of the experiment differ significantly from those that remain and to the extent that they could be expected to be included in a national program, the estimates will be biased in a way that will impair the usefulness of the experiment as a guide to a national program' (I, 117-8).

<sup>17</sup> A major drawback of this decision was that families with both spouses working regularly were underrepresented.

interview after the experiment, twelve regular quarterly interviews, and six special one-shot interviews (I, 15, 24). Those who were interviewed received \$5 per interview. Income had to be reported on a monthly basis, and in return for filling in the income report form on time, \$10 was paid on top of the normal transfer payment.<sup>18</sup> Before enrolment, the participants received a clear article explaining the working of a NIT-scheme,<sup>19</sup> and an enrolment kit containing the rules of operation, a tax table from which they could read how much one could expect to receive at various income levels, a payments calendar and instructions for filling in the income report form (I, 29). The rationale behind the enrolment kit was to limit as much as possible the contact between staff and families. This was decided in order '... to replicate in so far as possible the operation of a universal negative-income-tax program. One of the important elements of such a system, in contrast to existing public assistance programs, would be this lack of direct contact; it was important to learn the extent to which the families could function without casework contact. In addition, frequent contact could only increase whatever Hawthorne or other experimental effects there might be' (I, 30).

The selection of the sample (both experimentals and controls) was done on a step-by-step basis. From a random sample of nearly fifty thousand housing units only three thousand were found eligible for a pre-enrolment interview. This comprehensive interview further reduced to half the number of those eligible for actual enrolment. In the end, 1357 families, 725 experimentals and 632 controls, were selected (I, Table 2.1 and 2.3, 31, 36). The characteristics of the final sample were compared with the 1970 Census data to ensure representativeness (I, Table 2.2, 34). In addition, a separate check was carried out to compare families who refused enrolment to those who accepted. Only minor differences were found (I, Tables 2.6-2.9, 40-43). A major factor which may explain why some families refused to participate, given their 1968 annual income, was the small initial transfer payment that they would receive for participation.

Concerning the rules of operation of the experiment, three major decisions had to be made: the definition of the family, the definition of income, and the accounting period (I, 75-81). A family was defined as relatives and adopted living with the male family head. Those leaving the family could take with them their part of the guarantee, but could not start a new filing unit. Regarding family income, decisions had to be made on whether or not to include items like gifts and inheritances, dividends, earned interest and rental incomes, life-insurance, rent subsidies, and medical costs. The accounting period was on a four-week or monthly basis.

The questionnaires covered five major (economic and sociological) topics: work and income patterns (about job training, job history, wife's labour force history, child care and welfare history), debts and assets (ranging from property ownership to the net worth of consumer durables), family life and background (about family composition, family planning, educational background, religion, hobbies, etc.),

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<sup>18</sup> The filing fees were forfeited if the income report form was not returned within four weeks. This filing fee was introduced ten months after the experiment started (I, 54).

<sup>19</sup> J. Tobin, J. Pechman and P. Miezcowski, *Is a Negative Income Tax Practical?*, *Yale Tax Journal* 77, 1967, 1-27.

political and social life (about political awareness, social networks) and other assorted topics (e.g. social status, self-esteem, worry and happiness, attitudes toward work, and job satisfaction) (I, 149-162).

##### 5. LESSONS DRAWN FROM THE NEW JERSEY EXPERIMENTS

Thanks to the elaborate documentation<sup>20</sup> of the New Jersey experiments, some important lessons can be learned for any new experiment. Firstly, for reasons given previously, the NIT-experiments did not include female-headed families. The reasons for this are no longer valid. Moreover, the female participation rate has risen sharply since then. An experiment today should therefore include female headed families. Secondly, although the USA is a large country, the reasons given above to run the experiment in one confined geographical area seem convincing. Even in smaller European countries there are large differences between labour markets in different parts of the country. Moreover, administrating the experiment nation-wide is probably more costly. Thirdly, the main purpose of the experiment is to collect information about labour supply responses. The quality of this information depends to a large extent on the co-operation of both experimentals and controls for providing timely and accurate information. For this reason, the instrument of filing fees which are forfeited if the required information is not adequate or on time is very useful. Moreover, for the controls and for families with zero transfer payments, the filing fees are the only rewards for participating in the experiment. Rather generous filing fees serve two purposes: (i) to enhance the quality of the information and (ii) to reduce the attrition rate among controls and experimentals with zero transfer payments. Filing fees should of course not be too high, or else they might disturb the behavioural effects of the BI itself. Finally, the researchers of the New Jersey experiments never set out in advance what effects they expected to find.<sup>21</sup> To fill this gap it is important that the main effects to be expected from the introduction of a BI scheme are listed beforehand.

The experiment in the USA was very costly. It is not realistic to expect that a comparable budget will be made available for an experiment in a smaller European country. In order to keep the total cost of the experiment on a modest level the following proposals are suggested. First to take into account that the administrative and research expenses of the New Jersey experiments were more than twice the actual income support transfer payments. To reduce non-transfer costs, especially interview and research costs, we propose not to collect information on psychological and sociological effects since these effects were found negligible. This may almost halve interview and research costs with a likely low loss of useful information. In order to reduce the amount of transfer payments, we propose to include in the

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<sup>20</sup> Especially the in this paper much cited Vol. I and the appendices, which contain a chronology of events, and official descriptions of the rules of operation for the programme, the definition of income, the definition of the family unit, the filing periods and procedures and the review board.

<sup>21</sup> The researchers involved in the experiment never agreed on, or set down in advance, a summary of what they felt was the most likely outcome for labour supply. 'In retrospect, this is unfortunate. Any attempt to do so now is bound to reflect, to some extent, our present knowledge of the results and thus understate the degree to which we have been surprised' (II, 14).

experiment mainly social assistance recipients and families with an income around the break-even level (see section 6 below). Further, a decision has to be made on the number of plans. The fact that the New Jersey experiment contained eight different plans was mainly due to the fact that at the time it started, able-bodied men were not entitled to social benefits - unless they had social insurance. In these circumstances, and given that the non-poor were excluded, it was not difficult to find enough participants for each plan. In present circumstances, one has to take the prevailing welfare arrangements into account. Now, an individualized BI equal to the social minimum for a single person household would mean that two person households on welfare would gain substantially. This is because the present social assistance benefit for a two persons household in all European countries is much less than twice the benefit for a single person household. At the same time, however, an individualized BI significantly below the present levels of social assistance makes it difficult to find enough participants among single person households. Probably only participants who expect to stop working or want to engage in full-time schooling during the experiment would gain in comparison with what they receive under the current scheme. For these reasons, a differentiated BI, equal to or somewhat below the present social assistance levels, is the most suitable to experiment with. This means that single person and two person households on welfare would receive a BI equal to, or somewhat below, the present social assistance benefit, differentiated for household composition.

Finally, the New Jersey experiments had a duration of three years. An experiment of such a limited duration impairs the reliability of the effects on the labour supply if these are to be translated into a permanent unconditional scheme.<sup>22</sup> Given the budget available for the experiment, the more we can save on administrative, research and transfer costs, the longer the duration of the experiment can be, and the higher its reliability for assessing the effects of a permanent BI scheme.

## 6. DESIGN OF A NEW BASIC INCOME EXPERIMENT<sup>23</sup>

The purport of this section is to outline the structure of a BI experiment, taking into account the insights obtained from earlier experiments, and to arrive at a better proposal by offering the opportunity to criticize our proposal. As a result, we hope that at the time some country or city is prepared to conduct such an experiment a well constructed plan will be available.

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<sup>22</sup> 'Consider first the male household head with a steady job involving hard work and long hours. If he knew that negative tax payments were permanent, he might instead take a job with lighter work and more normal hours. Yet for a period of three years, such a shift might seem too risky. At the end of the experiment, he would need the higher earned income but might be unable to get his old job back. For the steadily employed male head, the probability is that an experiment of limited duration will have smaller effects on labor supply than will a permanent program... Wives, teenagers, and other adults in the household are likely to be in and out of the labor force as family circumstances change. To the extent that periods of withdrawal from the labor force are planned in advance, a temporary experiment encourages the concentration of such periods during the experimental years, when the costs of not working are lower than normal' (II, 10).

<sup>23</sup> This section is written in close collaboration with Paul de Beer.

There are three reasons why a field experiment cannot replicate the real introduction of a universal BI:

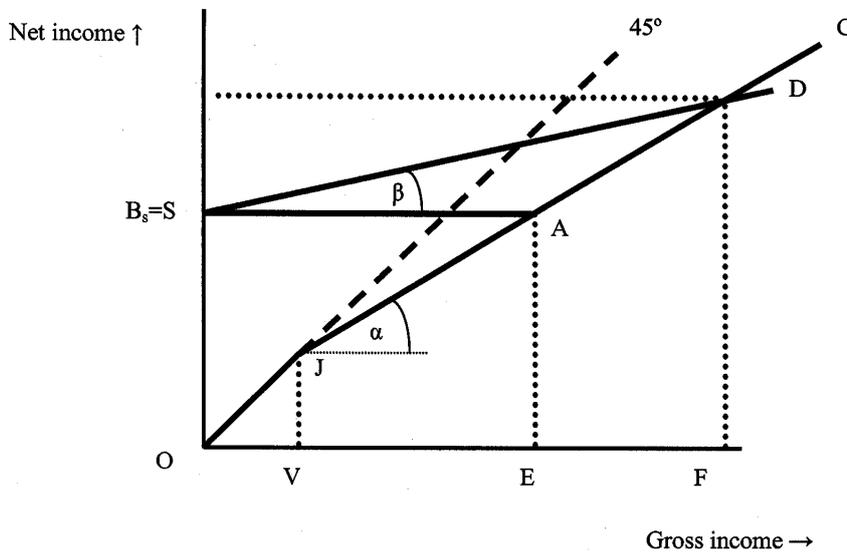
1. In a limited experiment it is not possible to change the external circumstances in the same way as would be the case with the real implementation of a BI. These circumstances include changes in minimum wage, the wage cost structure of employers, the labour demand (employment levels), shifts in economic structure between different branches of industry, shortening of labour time etc. Since the behaviour of (potential) workers (those working and those seeking work) is to a high degree determined by restrictions from the side of labour demand (i.e. the availability of jobs), the behavioural reactions to a BI will only manifest themselves to a degree in a limited experiment. On theoretical grounds it may be expected that a BI will result in a decrease in labour supply when calculated in average working hours per worker, but an increase of the labour supply when calculated in the number of employed persons. This means that the available work will be spread over a greater number of people. Since the first effect does not occur for the economy as a whole, participants (especially those who otherwise would be involuntary unemployed) in the experiment do not 'profit' from the dilution in the labour supply measured in hours per worker, which would occur with a universal implementation of the BI.

Another effect to be expected, which would not occur in a limited experiment, is that the net income (net wages plus BI) of workers with relatively unattractive work will increase in comparison with the net income of workers with relatively more attractive work. If everyone receives a BI, employers would need to attract employees for unattractive work by means of a higher wage or improved labour conditions. Since only a small number of people participate in the experiment, employers can attract unemployed people who do not participate in the experiment. As these are obliged to accept suitable work or else be penalised, employers would not have to increase wages or improve labour conditions.

2. In practice an introduction of a BI will mean an income improvement for some groups and an income reduction for others. To clarify this point, both systems are graphically illustrated in Figures 2 and 3 (for a single breadwinner family who enjoys twice the tax allowance  $V$ ) with the gross income on the horizontal and net income on the vertical axis. The line OJAC shows the possible combinations of gross and net (labour) income for workers and people who have no right to a benefit (mainly dependent housewives), SAC shows the gross-net trajectory of beneficiaries, both under the present system which is characterised by a tax free base of OV and a benefit level of OS. For example, someone with a working partner and a small part-time job earns a gross income of OV. Due to the tax-free base this person is not required to pay any tax, but he or she will have to pay tax as soon as the gross income is higher than OV. Figure 2 also illustrates one of the advantages of a BI over the present system, namely the elimination of the poverty trap SA. Under the present arrangement of social security the effective (marginal) tax rate on social assistance recipients is very high. This is partially due to the fact that supplementary arrangements such as rent subsidies and child care subsidies are dependent on income, and also, because working incurs additional costs. An

effective withdrawal rate of about 100% or even higher, is not exceptional. As long as the gross labour income of someone with a welfare benefit is lower than OE not much will change in net terms because the benefit and subsidies are deducted pro rata to the earnings. Finally, under the system of BI every adult receives an amount of OS or  $OB_s$ , with  $B_s$  the individual BI, but the tax rate on labour income will be higher than the average rate under the current system. Therefore the line  $B_sD$  is flatter than the line JAC.

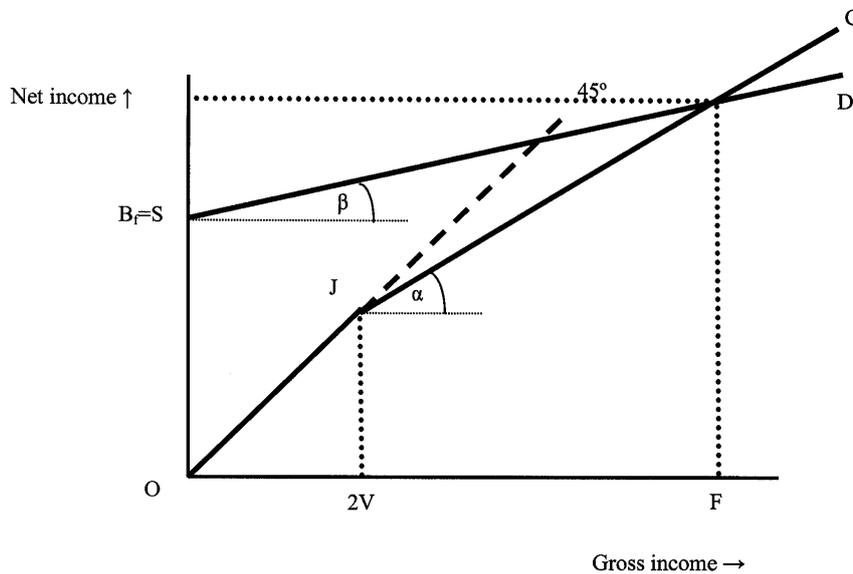
Figure 2. Conditional social security (SAC or OJAC) and unconditional social security (SD).



The net income of breadwinners with a gross income of OF is not affected (see Figure 3), no matter which system is adopted (of course, the level of OF depends on the tax rates under both systems). Those with a gross income lower than OF will improve their net income under a BI scheme. For the experiment this means that it is unlikely that people whose income deteriorates because of the introduction of a BI (those with a gross income higher than OF) will be willing to participate in the experiment. Those who are prepared to do so, for instance because it enables them to work less with a small sacrifice of income, do not form a representative sample of the group that would suffer from the introduction of a BI. Therefore, only those for whom the introduction of a BI has a neutral or positive effect will be included in the experiment. It is no problem that especially in this group undesirable behavioural effects are to be expected. People whose situation improves due to a BI would, in the opinion of many opponents of BI, withdraw or partially withdraw from the labour market. In any event the experiment will show whether this effect does indeed take place. However, the experiment could become rather costly especially if

people who profit financially from a BI take part in it. If this does not form an insurmountable budgetary problem, then there is no objection to letting these people (all with gross incomes between O and F) take part in the experiment. If the available budget is limited it would be preferable to let only those participate who hardly profit or suffer from a BI (that is, those with pre-enrolment gross income near OF).

Figure 3. Conditional social security (OJC) and unconditional social security (SD) for a breadwinner family.



3. Changes in the tax, premium and subsidy schemes that go hand in hand with the introduction of a BI, are difficult to simulate. In a limited experiment it will not be possible to change tax rates, premium levies, and the issue of subsidies. Therefore their effect will need to be represented in the level of the BI itself. As a consequence of this, for many participants in the experiment the BI will not have the characteristic of a constant and unconditional amount but of a benefit or subsidy which is greatly dependent on personal situation and individual behaviour. Taking the previous point into consideration, this means that in practice the BI will have a largely fictitious character for a large group of people. The simulated amount of the BI is cancelled out by the simulated increase in taxes or the reduction in subsidies or benefits. To be sure, even in the existing situation different payments and deductions on the payslip have for most people a fictitious character. This will apply even more with the BI experiment, because officially there are no changes in the tax deductions. By giving the participant regularly a summary of their simulated income

situation (e.g. a monthly summary with their fictitious BI, (extra) taxes, subsidies, wage, etc.) the BI can be brought as close as possible to reality.

On the basis of all these considerations, it would seem advisable to limit the experiment to those groups of the population for whom a BI can be relatively simply simulated and without much costs and from whom substantial and – taking the BI discussion into consideration – relevant behavioural reactions can be expected. The two groups that meet these criteria are beneficiaries of minimum level benefits (especially social assistance recipients) and workers who would neither see improvement nor deterioration in their incomes by the introduction of a BI. In addition prospective entrepreneurs could be considered for inclusion in the experiment.

### 6.1. Social assistance recipients

Simulation of the BI is not complicated in as far as the group of social assistance recipients is concerned: they simply retain their current benefits but it is changed into an unconditional benefit equal to, or somewhat lower than, the current level (see Figure 2). The obligation to seek work would no longer apply and earning extra money (themselves or their partner) would be possible without a limit. Because the real introduction of a BI would go together with a lower net wage (because of the necessary tax increase) this effect could be simulated by deducting part of the extra earnings from the benefit. A tax rate of 50% can be taken as the lower boundary. By giving single person households and those living together the current benefit as a BI, we can experiment with two different levels of BI. The benefit of a couple would be divided into half a benefit for each partner. A decision has to be made whether single people starting to live together during the experiment can keep their high benefit or cannot. It seems unavoidable that couples who separate are given the opportunity to get an extra benefit to top-up their BI to the social minimum of a single-person household.

### 6.2. Workers

For workers at the break-even level initially nothing changes when they receive a BI. This break-even level is defined as the gross income at which it makes no difference in terms of post-tax-and-transfer income whether one is subject to the present conditional scheme or to an unconditional scheme.<sup>24</sup> Only workers whose income is around the break-even level can participate in the BI experiment. The BI that they receive is compensated by the extra tax they would have to pay with the introduction of a BI (among other reasons because the tax free amount and the transfer of the tax allowance among partners would cease and because of loss of earmarked, but income-dependent subsidies). They would only notice the existence

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<sup>24</sup> To determine the break-even level, define  $t'$  as the tax rate and  $V$  as the tax allowance operative under the conditional scheme, and  $t$  as the tax rate under a scheme with a BI at level  $B$ . The break-even level is then determined by the equation which equates net income at the different schemes, holding gross income ( $y$ ) constant:  $(1 - t')(y - V) + V = y + B - ty$ , so  $y = (B - t'V)/(t - t')$ .

of the BI if something in their situation changed, e.g. if they began to work less. Therefore, at the start of the experiment, they would receive an explanation of the consequences for their net income resulting from different decisions that they could take during the experiment. If for example someone with a full-time job worked one day less per week, this would lead to a less than proportionate decrease in net income, instead of an approximately proportionate income decrease of about 20% which would be the case under the current system. So the experiment acts as a stimulus for participants to work less.

Somewhat problematic is that those who start to work longer or receive a promotion will earn less in a BI system than in the current system. Because of the previously mentioned reasons, it is unlikely that this effect can be expressed in the experiment. For this person, with a gross income now above OF, it would have been better not to participate in the experiment. Therefore a simulation of the effect of a BI will not be possible when an income improvement occurs. On theoretical grounds it is unlikely that in the short run the introduction of an income-neutral BI will lead to a desire to work longer (the so called income effect is zero while the substitution effect is negative) so the lack of this (possible) aspect does not seem too serious.

Another problem presents itself with workers who are sole providers. With the real introduction of the BI the non-earning partner would receive her own BI which would be coupled with an income reduction for the breadwinner (who, it is true, would also receive a BI but who would have to pay more tax because of the cessation of breadwinner allowances). Even if total net family income remains the same, it is probably not possible to simulate the income transfer from the breadwinner to the dependent partner. The breadwinner would have to be contractually obliged to transfer part of his wage to his partner: it is not likely that many traditional breadwinners would be prepared to do this (and if they do, they would not form a representative sample). In such cases the BI of the partner would have to be fictitious and only play a role if the partner started working, in which case we face again the problem that if the breadwinner does not work less time than before, the family income would, as a result of the BI, increase by less than under the present system.

The BI can be well simulated with the so called one and a half earners, that is with couples of which the man has a full-time job and the woman a part-time job (or the other way around). It is often expected that these women will cease working, because even without work they would receive their own income. If the introduction of a BI works neutrally for two-earner families, their situation will improve compared with the current system if the woman stops working. If this behaviour manifests itself during the experiment on a large scale (as opponents of the BI expect), it will become rather costly because all women who stop working receive a BI without consequences for their partners income.

### *6.3. Prospective entrepreneurs*

Beside the two categories mentioned above (beneficiaries and workers or families for whom a BI has a neutral effect) a third group, prospective entrepreneurs, could

be considered for inclusion in the experiment. According to its proponents a BI stimulates prospective entrepreneurs because initially it is not necessary to earn a full wage. If revenues are equal to costs and no profit made, they still have a BI to live from. The population could be called upon to apply to participate in the experiment if they are interested in starting their own business. Their present social economic position (employed worker, beneficiary, or housewife) would not be of concern (as long as one is not already independent). Not all of them receive a BI, because participants have to be coupled with other applicants who would serve as a control group.

The criteria that must be met have to be clearly defined to prevent the experiment from being misused. For example, workers would be required to terminate their employment (to prevent them from receiving a double income) and housewives will need to prove that they have started their own business (to prevent them from using the BI as a housewife wage). The BI of those who close their businesses during the experiment will have to be stopped (to prevent sham businesses). Furthermore the BI for new independents will be lowered if they make a profit in order to simulate the effect of higher tax rates.

#### *6.4. The cost of the experiment*

The suggested limitation of the experiment to only three groups, namely social assistance beneficiaries, workers with a break-even gross income, and prospective entrepreneurs, is motivated by the wish to keep the cost of the experiment as low as possible (or, with a given budget, to make the number of participants as large as possible and to extend its duration), and by research-technical reasons.

In the experiment, the benefit of beneficiaries is replaced by the same amount (or a somewhat lower amount) as a BI so that initially there will be no extra costs involved. The government may well lose revenue because the occasional, extra earnings of participants in the experiment can no longer be deducted from their benefits. However, these losses do not add to the expenditure cost of the experiment. If the BI proves to be a stimulus to work legally instead of in the black economy, as may often be the case at this time, the government's revenue may even improve.

Initially there are no extra costs for workers with a gross income at the break-even level. They do not experience a change in their net income if they do not change working hours. Their BI is financed from the higher tax rates they pay when they participate in the experiment. Extra costs only occur when participants in this group decide to work less. The maximum size of these costs is equal to the number of participants multiplied by their BI. This cost would only be incurred if all the participants in this group decided to stop working. For the group of prospective entrepreneurs, the costs which burden the budget of the experiment apply right from the start. Workers previously in permanent employment or people without an income who participate as entrepreneur in the experiment, receive a BI every month. Any extra deduction on their profit will at best only partially compensate for this.

### *6.5. Effects of a basic income to be researched*

The emphasis in the experiment will be on the labour market effects. There is much disagreement in this area, and these effects are of great importance for determining the feasibility and desirability of a BI. In concrete terms the research is about the consequences of BI on labour supply. Will workers work less or will some people even stop to work? Will beneficiaries be more prepared to accept low paid or part-time jobs? Will non-participating partners ('housewives') seek and take more or less paid work? Will a BI cause interesting behavioural reactions in all sorts of other areas? Although the latter may not be crucial for the judgement of BI's feasibility, it is nonetheless worth watching. For instance, the effects of a BI on consumption patterns, family composition (living together or separate), role division between men and women, the way leisure time is spent and things like participating in volunteers work, social participation, etc. are all possible consequences worth taking into account.

Admittedly, the information that a limited field experiment such as the one suggested here can furnish falls short of what we need to know to say something relevant about the feasibility of the BI proposal (see section 2 above). Still, depending on the outcomes, the experiment may provide some useful information. If those receiving a BI reduce their labour supply significantly in comparison with the control group, then the opponents of BI have, to say the least, the benefit of doubt in their favour. If experimentals and controls do not show any significant differences in behaviour, then it cannot be concluded that a BI has no adverse impacts on total labour supply. It may indeed well be the case that the experimentals perceive the duration of the experiment too short to change their behaviour. Finally, if experimentals, especially those for whom the conditional social assistance benefit is replaced by a BI, show a marked increase in labour supply in comparison with the control group, then this suggests that the bite of the poverty trap is serious and this would strengthen the plea for a BI. The experiment may show that the opponents of BI are right, but it cannot conclusively decide the same in favour of the advocates of BI.

## SUMMARY AND CONCLUSIONS

If the idea of a BI is ever going to come high on the political agenda, we have to know which kinds of social and economic effects can be expected by its implementation. Some economic models try to address this issue, but outcomes are very sensitive to how the labour market is modelled and what model makers believe to motivate people. A limited field experiment may enable us to fill part of this gap, because the limitations of empirical research and economic models is of an entirely different nature than the limitations of a real life experiment. For instance, in the former type of analyses, it is implicitly assumed that parameters describing the labour market behaviour of economic agents remain constant. Moreover, these

estimated or calibrated parameters are obtained by imposing a particular labour supply function on agents. The advantage of a real life experiment in this respect is that we can directly measure the labour supply responses of experimentalists compared to those in the control group. Contrary to the results obtained from empirical research on the labour supply responses of some group to non-labour income, we do not need to extrapolate the results to other groups, nor are the outcomes influenced by what model makers believe motivates people.

The study of the BI proposal is therefore surrounded by great uncertainties with respect to the changes in citizens' patterns of behaviour in response to such a reform. I doubt whether any firm conclusions can be drawn from either theoretical models or empirical research which try to scrutinize the effects of a substantial BI. With this as the point of departure, three interrelated issues were addressed. Firstly, I argued that a limited field experiment of a BI may solve part of the puzzle concerning economic feasibility. Secondly, any new experiment should be held informed of the New Jersey NIT experiments. Although the outcome of these experiments cannot be considered representative for the expected effects of the introduction of a BI in Europe or even the USA today, some important lessons can be drawn for setting up a new experiment. Finally, field experiments are rather costly ways to collect information, and thereby in designing this new experiment, we have to sail between Scylla and Charybdis. We must not allow experimental costs rise too high, and we must try to collect as much relevant information as possible. Hopefully the proposed design of an experiment may help to overcome some obstacles to the launching of a BI experiment somewhere in Europe, alongside the many workfare-oriented experiments already in place.



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