WHY LAUNCH A BASIC INCOME EXPERIMENT?

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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[1] 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 human capital: (i) students over the age of 18 engaged in schooling will receive a BI,

[1] See e.g. Besley (1990) and Creedy (1996).

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.[2] 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.[3] Not long ago, we have seen a flourishing, yet unresolved, debate on the effect of the level of minimum wages on employment.[4] 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.[5] 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 disguised wage ('hush money') for housekeeping and childrearing activities.

[2] 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 $(1\text{-}t)$ 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.'

[3] 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.

[4] See e.g. Kennan (1995), Dolado *et al*. (1996), Greenaway (1996) and Card and Krueger (1995).

[5] '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).

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.[6]

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, because it differs so much from the existing system. It is only when the

[6] 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.

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[7] 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, τ 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 $\tau_{\mathrm{BI}}(y) = ty$ (which corresponds to the horizontally shaded area in panel A of the figure) and for NIT as $\tau_{\mathrm{NIT}}(y) = -N + ty$ (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:

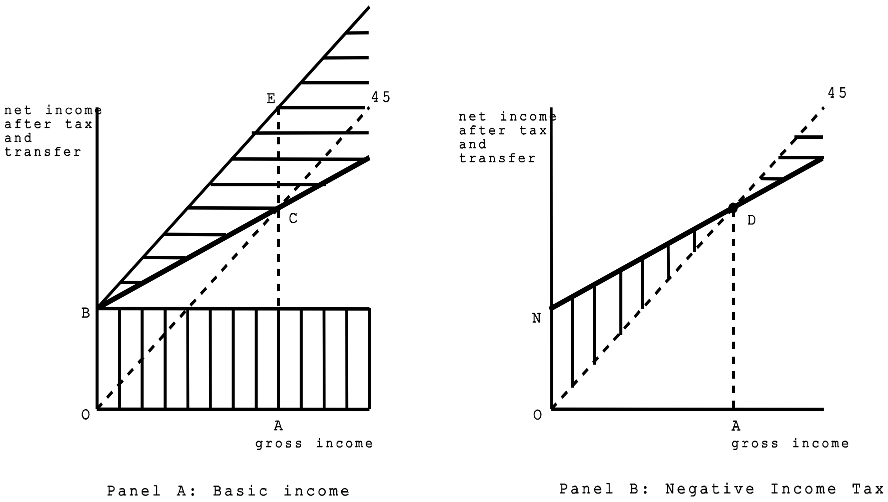
$$y_{\mathrm{d,BI}} = y + B - \tau_{\mathrm{BI}}(y) = y + B - ty$$

[7] 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

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

Figure 1. Basic income vs. negative income tax



Basic income and negative income tax schemes (Figure 1.1 Parijs (1992, 5), adjusted).

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).[8] 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.[9] 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).[10] 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

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[8] 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.

[9] 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).

[10] 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[11] 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[12] 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.[13] 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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[11] '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).

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

[13] 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,[14] 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)

[14] 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.

	Withdrawal rate (in %)		
Income Guarantee (in % of poverty line)	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).[15] At the end of the

[15] 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.[16]

Sample eligibility was restricted to families having one healthy man and with a family income below 150% of the poverty line (I, 8).[17] 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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[16] '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).

[17] 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.[18] Before enrolment, the participants received a clear article explaining the working of a NIT-scheme,[19] 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.),

[18] 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).
[19] J. Tobin, J. Pechman and P. Miezscowski, 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[20] 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.[21] 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

[20] 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.

[21] 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.[22] 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[23]

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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[22] '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).

[23] 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:

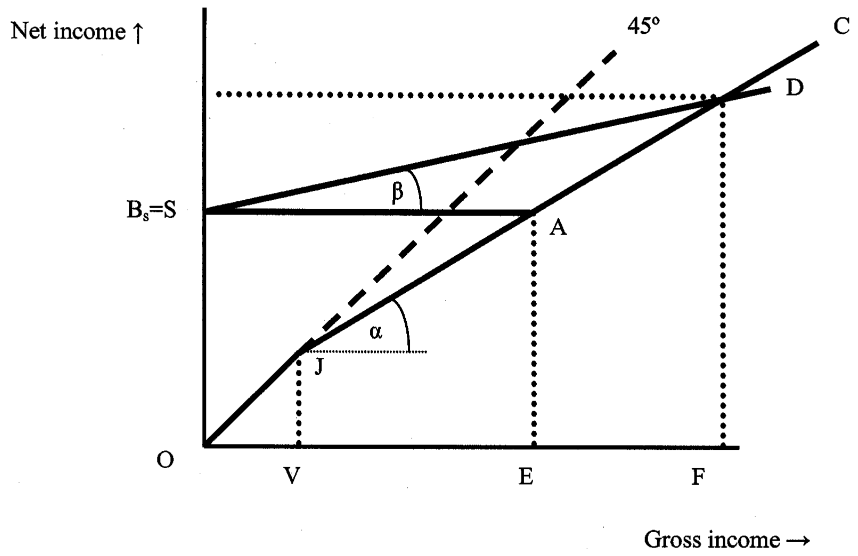
1. In a limited experiment it is not possible to change the external circumstances in the same way as would be the case with the real implementation of a BI. These circumstances include changes in minimum wage, the wage cost structure of employers, the labour demand (employment levels), shifts in economic 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

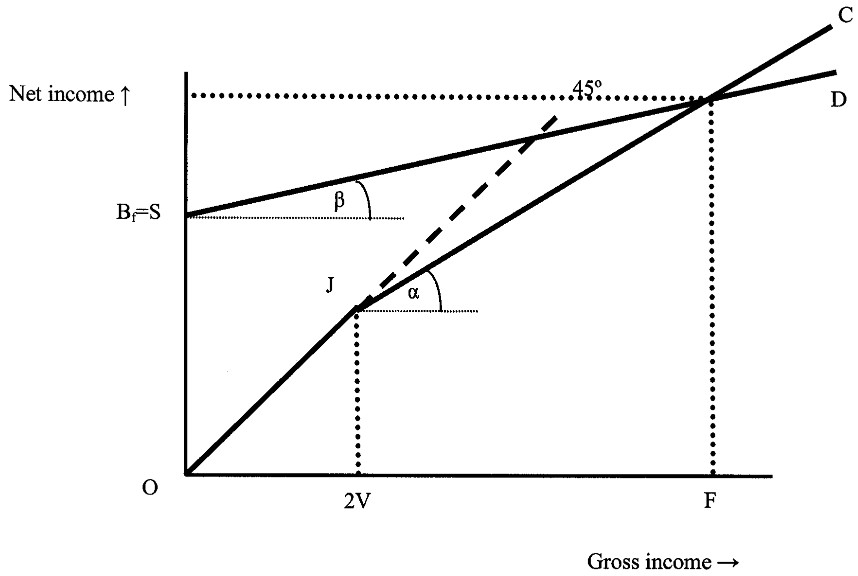
effective withdrawal rate of about 100% or even higher, is not exceptional. As long as the gross labour income of someone with a welfare benefit is lower than OE not much will change in net terms because the benefit and subsidies are deducted pro rata to the earnings. Finally, under the system of BI 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.[24] 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

[24] 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 experimentals 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.

A Failure to Communicate:

What (if Anything) Can we Learn From the Negative Income Tax Experiments?

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Abstract

The U.S. and Canadian governments conducted five negative income tax experiments between 1968 and 1980. The labor market findings of these experiments were an advance for understanding the effects of a basic income guarantee, but their conclusiveness is often overstated. A review of nonacademic articles on the experiments reveals poor understanding of the results. One often overlooked cause of this misinterpretation was the failure of researchers to make clear that the experiments could not estimate the demand response and therefore could not estimate the market response to the program. Although the evidence does not amount to an overwhelming case either for or against the basic income guarantee, some important conclusions can be drawn, if they are drawn carefully.

JEL Codes: I3, J2, and J3

Keywords: basic income, negative income tax, experiment, redistribution

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Between 1968 and 1980, the U.S. Government conducted four negative income tax experiments, and the Canadian government conducted one. The results of these experiments are extremely important to growing debate today about the basic income guarantee (BIG). Although the modern basic income guarantee discussion tends to focus on the basic income (BI) variant of the proposal rather than on the negative income tax (NIT) variant tested in the experiments, the two are similar enough that any conclusive findings from the experiments is of great value for the current discussion.[1] Although the NIT experiments had significant limitations, they yielded results that are extremely important to the current debate and that must be understood properly. This article reviews those results and clears up common misconceptions about them.

More than 200 scholarly articles on these experiments have been published in journals and books (see Bibliography B for an extensive list). Most of these articles were written in the 1970s and ’80s, but a few continue to come out today (O’Connor, 2001; Greenberg, Linksz, and Mandell, 2003, Levine et al, 2004). The debate died down without a clear consensus on what the results of the experiments implied for policy, and the results were widely misinterpreted in the popular media (see Bibliography A for a list of nonacademic articles on the experiments). The experimental results continue to be cited both by supporters and opponents of the redistribution of income as evidence for the workability or the unworkability of a guaranteed income. The experimental results seem to be a political Rorschach test in which an observer’s conclusions reveal more about the observer than about the observed.

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[1] I use the terms “basic income guarantee” and “guaranteed income” to mean any policy that ensures some minimum level of income for all citizens. “Basic income” ensures a minimum income by paying *everyone* regardless of their private income. The “negative income tax” ensures a minimum income by paying *anyone* whose private income slips below a certain level.

For example, in 1993, long after the results were in and the initial flurry of articles was over, Hum and Simpson declared in the *Journal of Labor Economics*, “Few adverse effects have been found to date. Those adverse effects found, such as work response, are smaller than would have been expected without experimentation” (Hum and Simpson, 1993a). But in the same issue, Anderson and Block (1993) mused about why so many social scientists continue to support the negative income tax “in the face of an avalanche of negative results” provided by the experiments. The most important reason for this disagreement is that the general result of the experiment was what everyone expected: all else equal, the treatment group worked less than the control group. This agreed; the central question was how much less would the treatment group work? Along with many other statistics, the experiments provided numerical estimates of that answer. The estimates required not only quantitative evaluation of their accuracy, but also qualitative interpretation of their meaning and that inspires widely differing opinions. Perceptions of the experiments in the media and in the political arena have been confused and superficial; neither the results nor the disagreements about how to interpret the results were understood by politicians or the media.

This paper focuses on the labor market findings of the NIT experiments arguing that although the experiments were an advance for social science and for understanding the effects of a basic income guarantee, the conclusiveness of the labor-market results is often overstated. Researchers either presented their research as more conclusive that it was or failed to prevent the lay audience from making that misperception. One often overlooked cause of this misinterpretation was the failure of researchers to make clear that the experiments could not estimate the demand response and therefore could not estimate the market response to an NIT.

Although the evidence does not amount to an overwhelming case either for or against the basic income guarantee, some important conclusions can be drawn, if they are drawn carefully.

Part one summarizes the operation of the experiments. Part two discusses the limits of the experiments for drawing conclusions for a national policy. Part three discusses the labor market findings of the experiments in light of their limitations. Part four discusses the political and media perceptions of the experiments. Part five concludes with a summary of the lessons of the experiments both for the basic income guarantee and for the dissemination of statistical research to a lay audience.

Part 1: The Experiments

The five experiments conducted in the United States and Canada are known collectively as “the income maintenance experiments,” “the guaranteed income experiments,” or “the negative income tax (NIT) experiments.” They began at a time when the elimination of poverty was the stated goal of the presidential administration, when there was a growing movement for economic rights, and when many social scientists and policymakers believed that social policy reform was heading in the direction of a guaranteed income. But by the time all of the results were available the movement for eliminating poverty had dwindled and the idea of “welfare reform” was beginning to be associated with dismantling rather than rationalizing the welfare system.

The NIT experiments were the first large-scale social experiment to use the scientific method of randomly assigning human subjects into treatment and control groups just as medical researchers do when testing drugs. Some social scientists have called them, “experiments in how to conduct experiments," and it is arguable that they had larger influence on future social experiments than in the examination of the policy they were designed to test.

The primary aim of the NIT experiments was to test the side effects rather than the effects of a basic income guarantee. The central goal of an income support program is to raise the welfare of the destitute, and that it can do that is something that does not need to be tested. Although the effect on poverty of most social policies (AFDC, TANF, EITC, job training, education, etc.) requires testing, the conclusion that an NIT with a guarantee rate at the poverty line can eliminate poverty is true by definition.

The effects of the negative income tax on health, homeownership, low-birthweight, school performance, and other indicators of the well-being of recipients were tested and reported in many studies (Avrin, 1980; Boumol, 1974; 1977; Bradbury 1978; 1986; Cain 1977; Elesh and Lefcowitz, 1977; Hall 1980; Hanusheck, 1986; Kaluzny, 1979; Keeley, 1980c; 1980d; Kehrer and Wolin, 1979; Kerachsky, 1977; Knudsen, Scott, and Shore, 1977; Ladinsky and Wells, 1977; Lefcowitz and Elesh, 1977; Mallar, 1977; Masters, 1978; Maynard, 1977; Metcalf, 1977a; Michael, 1978; Middleton and Allen, 1977; Murnane, Maynard, Ohls, 1981; Nicholson, 1977b; O'Connor, Madden, and Madden, 1979; Ohls, 1980; Poirier, 1977; Pozdena and Johnson, 1980; Rea 1977; Robins, 1980b; Rossi, 1975; Thoits and Hannan, 1980; Weiss, Hall, and Dong, 1980; Wooldridge, 1977). Most of these studies show positive effects, even for hard-to-change variables such as school performance and low birthweight, but discussion of these effects is beyond the scope of this paper. For an overview of some of these effects see Levine et al (2004).

Another side effect, the effect of the experiments on the divorce rate inspired a large amount of controversy (Bishop, 1980; Cain 1986; Galligan and Bahr, 1978; Ellwood, 1986; Groeneveld, Tuma, and Hannan, 1980a; 1980b; 1983; Hannan, Tuma, and Groenveld, 1977;

1978; Hum and Choudry, 1992; Tuma, 1986), but these findings are also beyond the scope of this paper). See Hannan and Tuma (1990) and Cain and Wissoker (1990a; 1990b) for two sides of this debate.

Table 1 summarizes the basic facts of the five NIT experiments. The first, the New Jersey Graduated Work Incentive Experiment (sometimes called the New Jersey-Pennsylvania Negative Income Tax Experiment or simply the New Jersey Experiment), was conducted from 1968 to 1972. The researchers originally planned to conduct the entire experiment in New Jersey, but they were unable to find enough poor whites there and had to open a second location in Wilkes-Barre, Pennsylvania to round out a racially representative sample. The treatment group originally consisted of 1,216 people and dwindled to 983 (due to drop outs) by the conclusion of the experiment. The sample size consisted of black, white, and Latino, two-parent families with incomes below 150% of the poverty line, and with a male "head," who was not approaching retirement.[2] Treatment group recipients received a guaranteed income for three years.

The Rural Income Maintenance Experiment (RIME) was conducted in rural parts of Iowa and North Carolina from 1970 to 1972. It functioned largely as a rural supplement to the New Jersey experiment, which focused on an urban population. RIME began with 809 experimental subjects and finished with 729. The treatment group received a guaranteed income for two years. Subjects met the same criteria as the New Jersey Experiment except that single-parent, female-headed households were also included. Few, if any, Latinos were included in the sample. Both RIME and the New Jersey experiment began under the direction of Office of Economic

[2] Husbands were usually the primary income earners in a family, and researchers tended to describe this role with the status-implying term "head of household." Women could not be "heads" unless they lived with children and without a husband.

Opportunity (OEO) and were completed by the Department of Heath, Education, and Welfare when OEO was abolished.

The largest NIT experiment was the Seattle/Denver Income Maintenance Experiment (SIME/DIME), which had an experimental group of about 4,800 people in the Seattle and Denver metropolitan areas. The sampled included black, white, and Latino, families with at least one dependent and incomes below $11,000 for single-parent families and below $13,000 for two parent families. The experiment began in 1970 and was originally planned to be completed within six years. Later, researchers obtained approval to extend the experiment for 20 years for a small group of subjects. This would have extended the project into the early 1990s, but it was eventually cancelled in 1980, so that a few subjects had a guaranteed income for about 9 years, during part of which time they were led to believe they would receive it for 20 years.

The Gary Income Maintenance Experiment (which is never abbreviated) was conducted between 1971 and 1974. Subjects were mostly black, single-parent families living in Gary, Indiana. The experimental group received a guaranteed income for 3 years. It began with a sample size of 1,799 families, which (due to a large drop-out rate) fell to 967 by the end of the experiment.

The Canadian government initiated the Manitoba Basic Annual Income Experiment (Mincome) in 1975 after most of the U.S. experiments were winding down. The sample included 1,300 urban and rural families in Winnipeg and Dolphin, Manitoba with incomes below C$13,000 (Canadian) per year. By the time the data collection was completed in 1978, interest in the guaranteed income was seriously on the wane and the Canadian government cancelled the project before the data was analyzed. Fortunately, university-based researchers were eventually able to obtain and analyze the data, so that results are available today (Hum and Simpson 1991; 1993a).

Two parameters are central to the design of any guaranteed income. The first is the guarantee level or the minimum income level (G in Table 1), which is the amount the recipient receives if she has no private income. Theoretically, the guarantee level can be any number between zero and per capita GDP. If G is too low, the NIT will not significantly reduce poverty or increase income security, if it is too high, it will have such strong work disincentive effects that the program would be unaffordable. The experiments intended to find out whether a guarantee level sufficient to seriously reduce or even eliminate poverty was feasible. For that reason guarantee levels between 50% and 150% of the poverty line were tested.

The U.S. experiments all defined the guarantee level relative to the poverty line, testing nine different guarantee levels: 0.5 (50% of the poverty level) was tested in the New Jersey and Rural Income Maintenance Experiments. 0.75 was tested in all four of the U.S. experiments. 1.0 (just enough to eliminate official poverty) was tested in all of the U.S. experiments except SIME/DIME. 1.25 was tested in only in the New Jersey Experiment, and 1.26 and 1.48 were tested only in SIME/DIME. Mincome, which defined its guarantee level in Canadian dollars rather than relative to the poverty level, tested guarantee levels of C$3,800, C$4,800, and C$5,800 per year. These levels were near the poverty line at the time.

The other central parameter of any guaranteed income system is the marginal tax rate (t in Table 1), also known as the "take-back rate:"[3] the rate at which benefits are reduced as the

[3] The practical working of the marginal tax rate is slightly different if the guaranteed income is administered as a basic income rather than as a negative income tax.

recipient makes private income.[4] In other words, the marginal tax rate is the effective income tax rate per dollar of private income for recipients of the negative income tax. A higher marginal tax rate is associated with a lower a overall tax-cost of program[5] but also with greater the work-disincentives, and a greater potential “poverty trap.” A lower marginal tax rate is associated with a greater redistribution of income towards people with incomes above the poverty line. Redistribution to this group might be desirable in terms of equity (as a reward for low-wage workers), but to do so would increase the cost of a program primarily conceived as an anti-poverty policy.[6] For these reasons, it is important to know what kinds of take-back rates are feasible and the work-disincentive effects of each. The experimenters tested nine different values of t: 0.3 (30%) was tested in the New Jersey and Rural Experiments. 0.35 was tested only in Mincome. 0.4 was tested only in Gary. 0.5 was tested in all of the experiments except Gary. 0.6 was tested only in Gary. 0.7 was tested in the New Jersey Experiment, RIME, and SIME/DIME. 0.75 was tested in Mincome. SIME/DIME tested two nonlinear income functions with marginal tax rates of 0.7 minus 0.025 times income and 0.8 minus 0.025 times income. The effect of these two nonlinear functions was to impose higher marginal tax rates on lower levels of income and lower marginal tax rates on higher levels of income.

The use of so many different rates of G and t, reduced the numbers of subjects receiving each type of treatment, and therefore reduced the statistical reliability of the results for each.

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[4] Private income could include interest, dividends, capital gains, etc. But for the participants in these experiments it was overwhelmingly wage income.

[5] Higher marginal tax rates could be associated with higher taxes costs if the supply of labor had a very high elasticity of substitution, but this was not expected and did not prove true in any of the experiments.

[6] The basic income movement today puts less stress on the issue of poverty reduction and more stress on broader equity goals that make the issue of spending money on those already above the poverty line is less important.

Some of this tradeoff is worthwhile to allow for testing of a greater variety of potential parameters, but the experiments might have benefited from more coordinated effort to test a uniform group of widely spaced parameters.

Table 1 summarizes the configuration of the experiments.

Part 2: What the experiments could and could not measure

Within the context of the work-effort response, there were conceptual questions about which parameters and which effects deserved most concern. Results were reported for income and substitution effects of various levels of G and t, but the most discussed statistic was the simple question of the overall effect of the various treatments on the hours of work of the average recipient, and so I will focus on that here as well. There were also conceptual questions about how findings on work hours should be used: were they important because they represented the shift in the labor supply curve, because they had implications for the tax cost of the program, or because they had implications for the efficiency cost of the program? Overwhelmingly, the concern came to be the overall change in work hours and their effect on the tax cost of an NIT. Economists focused on this issue, even though only the work disincentive effects of the marginal tax rate (not the guarantee rate) represent a true cost in terms of economic efficiency (Hall, 1986).

The experiments produced many precise and technical estimates for the effect on hours of work, but what we learned from these estimates is small in comparison to what we simply do not know about the effects of a national program on work hours. Three obstacles (that make it difficult to draw conclusions about national policy) can be understood with reference to Figure 1. First, there was no stated agreement about what level of work disincentive would be considered acceptable. How much of a decrease in H on Figure 1 is too much? Second, there were problems with the fallacy of composition. That is, how well the response of the treatment group to the experiment represented the response of a wider population to an actual program. How well does the experimental shift from A to B represent the true shift from A to B? Third, the experiments measured the supply response to an NIT, but they were incapable of measuring the demand response, which made them incapable of determining the market response to an actual program. How much did the estimated shift from A to B differ from the shift from A to C that would determine the final effect on hours and costs?

The first two of these problems have been well discussed by the scholars who wrote about these results, but were not well understood in media reports on the experiments. The third received only minor discussion by academics and virtually no discussion in the media or in Congressional testimony. The rest of the section discusses these three problems in more detail.

2.1 The lack of an agreed acceptable level of work-disincentive

Many of the authors who have written on these experiments have complained that there was no criteria laid down for what decline in work-effort would be considered acceptable. Although this fact allowed sides could claim that the results vindicated their beliefs, there are two reasons why this criticism of the experiment is overstated: The experiments did give conclusive answers to several objective questions, and the goal of the experiments was inquiry; they were not expected to be a precursor to immediate implementation if work effort declined by less than a percentage. The NIT was simply a policy that Congress was interested in learning more about, and in that respect there was no need for a simplistic yes-or-no result.

There were, in fact, three objective yes-no questions about the work-effort response that the experiments answered quite well, all of which are very important to the BIG debate: First, would a large number of people respond to an NIT by withdrawing entirely from the labor force? The experiments found no evidence of such behavior. Some of the experimenters said that they were unable to find even a single instance of labor-market withdrawal (Levine et al forthcoming). Second, would the work-effort response be large enough to threaten the financial viability of an NIT? The experiments found no such evidence. Third, would there be any work-effort response? The experiments found that there was a non-negligible work-effort response.

There is a large range between a negligible work-disincentive and one that is so large that it makes the experiments unaffordable. Most researchers who worked on the experiments were not surprised that the results fell into that range, and it simply means that anyone who reads the results must make a judgment about them. That judgment is a matter of an opinion, about which people are likely to disagree. Therefore, the experiments gave both sides the ability to judge the results favorably.

2.2 The fallacy of composition and the representativeness of the experiments

The representativeness of the experimental results was affected both by sampling and by the extent to which the experiments could replicate an actual policy change.

The experiments did not draw a random sample of data. Only low-income families were tested; most of the experiments sampled only families with incomes below 150% of the poverty line. Gary and SIME/DIME sampled higher income participants but only in small numbers. Because only low-income families were tested, most of the experimental families did not have the kind of jobs that gave them a reason to stay committed to the labor force. Such families have a greater incentive and a greater ability to withdraw from the labor force than families with better paying, more secure jobs. This method of drawing the sample does not make the experiments "wrong" it merely means that they focused on the reaction of the poorest segment of the labor force, and must be read accordingly. Moffitt (1979b) estimated that the labor supply response of eligible low-income individuals would be –4.5% but the response of the labor market as a whole would be only –1.6%. However, it should be noted that a response by higher-income people, if there is one, has greater effect both for the efficiency cost and the tax cost of an NIT.

Participants were not randomly assigned to treatment groups. In order to reduce the costs of the experiments, the researchers tended to assign those with higher pretax incomes more generous programs (higher levels of G and lower t). This strategy enters an important bias into the estimated responses to these parameters.

Many of the results are not attributable to the NIT per se but to the fact that most of the NIT plans tested were more generous than the existing welfare programs that the control group was eligible for (Robins and West, 1980b). Butless (1986) observed that the average tested program was much larger than anything likely to be introduced and therefore overstated the work-effort response. The question of whether an NIT system or conditional welfare system or a similar size would have a larger work disincentive is still unanswered.

Few if any single, childless individuals were sampled. This is the group might have a larger work-effort response, because (aside from Food Stamps) they were not eligible for any non-work-based benefits, as parents were at the time.

The experiments measured the short-run response to a temporary change in policy, but we really want to know the long run response to a permanent change in policy. This problem could mean that the experiments either overestimated or underestimated the work-disincentive effect. As Harold Watts described it, an experimental plan that recipients know will be in place for only a few years, is the equivalent of putting leisure time on sale: When laundry soap is on sale, people buy more of it, and we can expect a similar response when leisure is on sale. People, who might want to take a few weeks or months off work sometime in the next ten years, might as well take it while the experiment is going on (Levine et al forthcoming). On the other hand, because the experiments were only temporary, recipients knew that they had to return to the workforce eventually, and might have been less likely to drop out for fear of losing work experience or losing their place in line for promotion. It is questionable whether many of the recipients had jobs that elicited such loyalty to the labor market, but arguably a permanent NIT could give workers a disincentive toward building the kind of attachments to the labor force that might lift them well out of the bottom of the income distribution later in life. The possibilities for biases in either direction do not necessarily cancel each other out, but they do show that those who make claims that the long-run effect is certainly larger than the experimental effect (Burless, 1986; Anderson and Block, 1993) are making claims that are not supported by evidence or theory.

Metcalf (1974), Ashenfelter (1978) and Robins (1984) discussed the problem of limited-duration experiments and efforts to solve it. The best evidence on this issue provided by the experiments comes from the SIME/DIME “20-year” recipients. It is unclear whether these recipients believed the experiment would last for 20 years, and they would have been wise not to, as it was cancelled after 9 years. These recipients did not behave terribly different from other experimental group (Robins, 1984), but even if the experiment had gone on for the full 20 years it could not have estimated everything we want to know about long-term and cultural effects of an NIT.

Other problems included Hawthorne effects, complicated experimental rules, attrition, and underreporting of income by the experimental group. Hawthorne effects are changes in behavior that resulting from being watched and/or from trying to influence outcome of an experiment. Ferber and Hirsch (1978) argued that many participants did not seem to understand the eligibility rules. Attrition is likely to lead to bias towards exaggerating the value of the work-disincentive effects because those who worked the least had the most to gain by remaining in the experiment. Underreporting is important because the control group had no incentive to misrepresent their private income, while the experimental group did (Greenberg, Moffitt, and Friedman, 1981). They may also have had a greater ability to get away with underreporting than they would if an actual policy were in place. Ashenfelter (1986) speculates that underreporting might have been the main cause of the difference in reported income between the control and experimental groups, which would greatly bias the results toward over estimation of the work-disincentive effects.

2.3 The inability of the experiments to measure the demand response

The researchers involved were clearly aware of the absence of a demand response and of its theoretical importance, but with few exceptions (such as Browning, 1971; and Greenberg, 1983) it received little attention in the literature. To determine the market effect, researchers would have to know the elasticities both of labor supply (which the experiments estimated) and of labor demand (which the experiments could not estimate). The following analysis assumes no unemployment. If unemployed workers replace the work reductions for NIT recipients, the effect of an NIT on total labor hours, output, and the efficiency cost of an NIT will be mitigated, but the effect on the labor hours of recipients and on the tax cost will not be mitigated.[7]

Examining the extreme cases reveals the range of possible outcomes. Figure 2 shows the effects of a completely inelastic demand for labor. In this case, firms need a fixed amount of workers and will pay whatever they must to get it. If so, no amount of labor-disincentive effect will cause any long-run decrease in work effort; the entire result of the work-disincentive effect would be to raise wages; and there would be no equilibrium decline in hours worked and no efficiency cost. Figure 3 shows that, if the demand for labor is perfectly elastic (if firms will hire any amount of labor at the going wage, but won't pay even a cent more for it), the market equilibrium will be entirely determined by the horizontal shift in the supply of labor just as measured by the experiments.

The more general results are that the equilibrium level of work effort will be somewhere between the initial equilibrium (point A) and the horizontal shift in supply (point B), and that the equilibrium wage will be as high or higher than the initial wage. In other words, the market equilibrium will be somewhere in the shaded area in Figure 4. Without information on elasticities, it is impossible to say precisely where in this region the equilibrium would be. Thus, instead of estimating the equilibrium outcome of a negative income tax, the experiments estimated *the boundary of a region of possible outcomes*.

It should be noted that it is theoretically possible for the equilibrium point to be in the region to the upper left of point B if the labor supply is backward bending. However, backward bending requires that workers' demand for goods is so inelastic that a decrease in wages will cause them to work more hours to maintain their level of consumption. That is quite reasonable

[7] See Greenberg (1983) for a more detailed discussion of this issue in the context of unemployment.

for someone whose labor is the primary or the only source of income. But if a generous guaranteed income is in place, a lower wage reduces the portion of income attributable to work. It becomes unlikely that workers will work more and more to maintain the level of a smaller and small part of their income. Therefore, it is unlikely that labor supply would backward bend for workers in the low wage market when a substantial NIT exists. Also, if it did exist it would be likely to lead to a very large increase in wages as the backward bending supply forced the price farther up the supply curve.

If a backward bending labor supply is ruled out, the lack of ability of the experiments to estimate the market response to a guaranteed income has several important effects on the estimates:

- The reduction in labor hours would be smaller than estimated by the experiments.
- The increase in income of recipients (and therefore) the effect of the program on poverty would be larger than estimated (via increased wage rates).
- The cost of the program in terms of tax dollars would be smaller than estimated.
- The efficiency loss of the program would be smaller than estimated.
- The increase in wages would create a cost to firms that the experiments could not estimate.

In other words, the experiments found upper-bound estimates for the decline in hours worked, lower-bound estimates for the effect of the program on the income of recipients, upper-bound estimates for the cost of the program in terms of tax dollars and efficiency loss, and no estimate of the cost of the guaranteed income in terms of higher wages.[8]

Given this inherent limitation of the experiments, there are two reasonable ways to present results: One is to obtain the best available estimates for the elasticities and simulate the outcome (Betson, Greenburg, and Kasten, 1980; 1981; Betson and Greenburg, 1983, Greenberg, 1983). The other is to present them was what they were: estimates of the boundaries of a range of possibilities. Instead, as shown in part 3, demand effects were sometimes ignored and often treated with a small caveat. When treated with a caveat it was often included on a list of things that could bias the estimates, such as factors mentioned in 2.2, but few brought attention to the important difference between those biases and the difference between a point estimate and an estimate of the boundary of a range.

Part 3: The work-disincentive results of the experiments

Nearly half of the scholarly articles on the negative income tax experiments deal in some way with empirical results for work incentive effects, and many of those present original estimates. Table 2 summarizes the findings of several of the studies on the work-effort response to the NIT experiments, giving the difference in hours worked by the experimental group relative to the control group in hours per year and in percentage terms. Results are reported for three categories of workers, husbands, wives, and “single female heads” (SFH).[9] Data was also collected for the work effort of youths, but is omitted from this table in the interest of brevity.[10]

[8] This is not an economic cost, of course. But it is a cost to an interest group that might interest policymakers.

[9] Meaning women with children and no husband.

[10] Youths tended to have work-effort responses comparable in percentage terms to wives and single mothers. It was not correlated with an increase in school attendance, but was correlated with an improvement in school performance.

The five experiments found a range of work-effort reduction from -0.5% to -9% for husbands, which corresponds to a reduction of about ½ hour to 4 hours per week, 20 to 130 hours per year, or 1 to 4 fulltime weeks per year. The three studies averaging the results from the four U.S. experiments (Robins, 1985; Burtless, 1986; Keeley, 1981) found work reduction effects of 5%, 7% and 7.9% respectively.

The response of wives and single mothers was somewhat larger in terms of hours, and substantially larger in percentage terms because they tended to work fewer hours to begin with. Wives reduced their work effort by 0% to 27% and single mothers reduced their work effort by 15% to 30%. These percentages correspond to reductions of about 0 to 166 hours per year. The labor market response of wives had a much larger range than the other two groups, but this was usually attributed to the peculiarities of the labor markets in Gary and Winnipeg where particularly small responses were found.

Robins (1985), Robins and West (1980a; 1980b), and Moffitt (1979a) all clearly present their findings as the difference between the labor supply of the treatment group and the control group, which should avoid any confusion with broader labor market findings to anyone who understands the difference, and one would expect everyone who reads technical articles is likely to understand. Others added a simple caveat (Keeley et al, 1978a; Moffitt, 1979b), but some were not as careful to avoid confusion. Orcutt and Orcutt (1968) claimed that the experiments could produce unbiased estimates of the disincentive effects and earnings effects of an NIT, when the lack of a demand response clearly makes this impossible (Browning, 1971). Ferber and Hirsch (1978, p. 1385) referring to the "labor supply response" as the "labor market response" despite explaining the difference later in the article. Kelly and Singer (1971) write, "No experiment paper should be complete without mention of possible response bias," but do not mention the experiment's inability to measure demand response as a source of bias. West (1980b, 642) mentions three ways NIT can affect wages without mentioning the demand response. Most of these slips are small, but the omission of demand is more significant when researchers attempt to carry the results over to the cost of a national program.

Table 3 reports some of the labor market findings other than the simple difference between the hours worked by the treatment and control groups. Robins, Tuma, and Yeager (1980) and Tuma and Robins (1980) found that the percentages are much larger if labor response is considered in terms of the increase in the length of spells our of work or the rate at which people who aren't working return to employment. These results largely reflect the fact that the reduction in labor hours was not primarily caused by workers reducing their hours of work each week but by remaining nonemployed longer if and when they became nonemployed. Increased periods of nonemployment might have an efficiency benefit if they lead to better matches between workers and firms.

Several studies estimating the additional tax cost caused by the work-effort response found widely divergent results. Rees and Watts (1976) estimated it would add 5% to 10% to the tax cost of the program. Ashenfelter (1978) estimated that the cost of the program without labor market effects would be 78% of cost with labor market effects, which is equivalent to saying that the reduction in work effort would increase the tax cost of the program by 28%. Keeley et al (1978a) estimated that the labor supply response would account for 23% to 55% of total program costs (equivalent to an increase of 30% to 122%). Burtless (1986) estimated that work disincentive would nearly triple the tax cost of the program. All of these studies neglect the demand response, implicitly assuming that demand is completely elastic. Rees and Watts's conclusion is that the costs are small and so apparently don't think it necessary to say that a demand response might make the costs even smaller. Only Keeley et al (1978) explicitly make the assumption of perfectly elastic demand. They admit that this reduces the accuracy of the results, and justify the assumption by speculating that employers could easily replace NIT recipients with workers who are not covered by the program.

Most of the studies that did include a demand response used data from the NIT experiments to examine particular changes in policy such as Carter's Program for Better Jobs and Income (Betson, Greenberg, and Kasten, 1980a; 1980b; Betson and Greenberg, 1983), and so are not very useful for correcting cost estimates of an NIT for demand responses. Only Greenberg (1983) applied a microsimulation model with a demand effect to the cost of an NIT as examined in the experiments. He found that a wage response could slightly mitigate the effect on hours and costs but the general pattern remained in which a dollar spent on poverty reduction raises the incomes of the poor by less than a dollar,[11] but his results are tentative because they depended on assumptions about the elasticity of demand, the level of unemployment and the substitutability between NIT recipients and other workers (Greenberg, 1983). Bishop (1979) used a general equilibrium framework to examine the impact of several antipoverty programs including NIT on efficiency. The focus on efficiency rather than tax cost means that his results are not directly comparable to the others, but he finds that the NIT would produce a demand response that would increase wages and therefore it would reduce both the efficiency loss and the tax cost of the program. Unfortunately there do not seem to be any articles employing a demand response in otherwise comparable models that generate comparable estimates of tax cost, hours worked, efficiency lost, and impact on inequality.

[11] Personal correspondence.

These results are not extremely divergent or controversial, and they are not terribly conclusive on the issue of whether the government should introduce a basic income guarantee, but they can be spun to make an apparently strong case either for or against it. Most of the scholarly works did not seem to consciously spin the results with a few exceptions such as Burtless (1986) and Anderson and Block (1993). Although Burtless displays knowledge of the difficult issues involve in the experiments, he betrays an effort to nudge the conclusion in direction. He declares a 7% decline in work effort to be “large.” He discusses various biases in the estimation of labor supply that point in both directions, but hastily concludes that the balance the labor supply effects are overestimated, and fails to recognize the significance of underreporting bias (Ashenfelter, 1986). He does not mention that his cost estimate is substantially larger than any of the others, and he does not mention that it is biased by the omission of a demand response. Anderson and Block (1993) seem to use Burtless (1986) as their primary source, but make a one-sided representation even of his account, omitting many of his caveats and clarifications. They go farther than Burtless by attributing poverty to a lifestyle “choice” on the part of recipients because so many people in poverty do not work, ignoring such a basic economic concept as *unemployment*. They ignore the demand side of the labor market, failing to note that poverty also represents the “choice” of employers in the low-wage sector who pay wages that leave workers in poverty even if they work fulltime. Anderson and Block’s normative and positive arguments are both one-sided and therefore not very valuable.

Despite these two exceptions, the presentation of the data in the official reports and in most published works was good science and not political spin. But as part 4 shows, once that data made its way into the public arena, it was spun anyway.

Part 4: Political and Media Perceptions of the Experiments

Hopefully, parts 2 and 3 have demonstrated that the findings of the NIT experiments are far more complex, subtle, and ambiguous than one might be led to believe by findings such as an X% decline in hours worked. But as this section shows, the complexity of the results was largely lost on politicians and members of the media to whom the findings were reported. Bibliography A contains a survey of about 50 articles from the popular media on the experiments.

The experiments gained significant attention in the press only twice. In 1970–1972, when Nixon's Family Assistance Plan (FAP) was under debate in Congress, and in 1977–1978 when Carter's Program for Better Jobs and Income (PBJI) was under consideration. Both plans had elements of a negative income tax; neither was a pure guaranteed income, although FAP was considerably closer to it than PBJI. In 1970, the first experiment had only been under way for two years and researchers believed that they were at least three years away from being able to produce meaningful results, but at the insistence of the administration and some members of Congress, the researchers released preliminary reports showing no evidence of any work disincentive effect.[12] Some other members of Congress (rightly) could not believe the result, and commissioned a review of the results from an independent auditor that concluded the results were "premature," which was just what the researchers had initially warned.

Results of the fourth and largest experiment, SIME/DIME, were released while Congress was debating PBJI. Dozens of technical reports with large amounts of data were simplified down to two statements: It decreased work effort and it supposedly increased divorce. The small size of

[12] The reason that the preliminary reports so greatly underestimated the work-effort reduction was probably that workers took several years to adjust their behavior to the new policy (see Robins and West, 1980b).

the work disincentive effect that pleased so many of the researchers hardly drew any attention. Never mind that everyone going into the experiments agreed that there would be some work disincentive effect; members of Congress were appalled; and columnists across the country responded with a chorus of negative editorials decrying the guaranteed income and ridiculing the government for spending millions of dollars to find out whether people work less if you pay them not to work.

The United Press International (1977) simply got the facts wrong saying that the SIME/DIME study showed that “adults might abandon efforts to find work.” The UPI apparently did not understand the difference between a decline in work hours while continuing to work, and abandoning the labor market. The Rocky Mountain News claimed that the NIT “saps the recipients’ desire to work.” Jones (1977) writing for the *Seattle Times* presented a relatively well-rounded understanding of the results, but despite this, simply concluded that the existence of a decline in work effort was enough to “cast doubt” on the plan. Similarly Rich (1978, November 18) implied that evidence showing the NIT “might cause recipients to work less” is enough to disqualify the program from consideration. Raspberry (1978) declared the experiments a failure simply because people worked less.

Senator Daniel Patrick Moynihan who had written a book in support of the guaranteed income a few years early and who had been one of the architects of FAP, recanted his support for the guaranteed income as a result of the SIME/DIME findings. He is a sociologist and would be expected to have a sophisticated understanding of statistical data, but he implied in a letter to William F. Buckley later published by the *National Review* that the mere existence of a work disincentive effect was an important factor in his recantation. He stated, “But were we wrong about a guaranteed Income! Seemingly it is calamitous. It increases family dissolution by some

70 percent, decreases work, etc. Such is now the state of the science, and it seems to me we are honor bound to abide by it for the moment." He held Congressional hearings on the results in November of 1978 to discuss the evidence. Although a large amount of good information was presented (U.S. Senate, 1978), media reports and politicians' comments on the experiments did not betray a real understanding of the findings

Headlines such as "Income Plan Linked to Less Work," and "Guaranteed Income Against Work Ethic" appeared in newspapers following the hearings. The Knight News Service (1978) quoted Jodie Allen of the Labor Department commenting on Spiegelman's cost estimates saying, "It could easily turn out that the government might spend billions of dollars on benefit payments and have little effect on the families' incomes. Instead, most of the (government) expenditures would offset reductions in earnings." Only a few exceptions such as Carl Rowan for the Washington Star (1978) considered that it might be acceptable for people working in bad jobs to work less, but he could not figure out why the government would spend so much money to find out whether people work less when you pay them to stay home.

Spiegelman, one of the directors of SIME/DIME, defended the experiments in the Washington Star (1978), saying that the experiments provided much needed costs estimates that demonstrated the feasibility of the NIT. He said that the decline in work effort was not dramatic, and could not understand why so many commentators drew such different conclusions than the experimenters. Demokovich (1978) was one of the few popular writers who considered the work-effort reduction to be small, but the more common reaction was given by Senator Bill Armstrong of Colorado Citing only that a work disincentive effect existed, Armstrong said the experiment was, "An acknowledge failure. Let's admit it, learn from it, and move on" (Brimberg, 1980).

What we had there was a failure to communicate. The scientists who presented the data were not entirely to blame for this misunderstanding, as Burtless (1986) remarked, "Policymakers and policy analysts … seem far more impressed by our certainty that the efficiency price of redistribution is positive than they are by the equally persuasive evidence that the price is small." It may be impossible to communicate such complexities to an audience interested in sound bytes or bottom lines, but social scientists have a responsibility to do a better job than we did in this instance. The understanding of the NIT experiments displayed in the popular press was superficial and obviously the result of spin. Few commentators kept figures like 5% to 7% in perspective. None of the articles in the popular media that I was able to find betrayed any understanding that the experiments measured only the horizontal shift in the labor supply function. None seemed to understand the elementary economic principle that a change in supply necessitates a demand response that can greatly affect the equilibrium outcome.

Part 5: Conclusion

It would be very easy to spin on the results in either direction. A positive spin would focus on the size of the work disincentive effects. The experiments clearly contradicted two of the most common arguments against a basic income guarantee: The experiments found *no evidence* that a negative income tax would cause some segment of the population to withdraw from the labor force, and the experiments found *no evidence* that the supply response would increase the cost of the program to the point that it would be unaffordable (even ignoring the mitigating demand response). Certainly, some level of G would make an NIT untenably, but the results implied that a guarantee level as high as 150% of the official poverty level would be well within the bounds of financial feasibility. Also, the experiments predicted that the full labor market response in the work hours of primary income earners would fall into a range of about 0–5% or 0–7% and where in that range it fell would depend on the elasticity of demand for labor. The reduction in work hours could be called "small," and it could be mentioned that it would have the side benefit of increasing wages, further reducing poverty and inequality.

A negative spin would require a focus on three facts: First, there was a statistically significant work disincentive effect, allowing willing laypersons to draw the fallacious conclusion that there was therefore a substantively significant work disincentive effect. Second, work reductions of 5% to 7% among primary earners in two-parent families and reductions of up to 27% for other earners could be called "large." Third, the work disincentive increased the cost of the program over what it would have been if work hours were unaffected by the NIT. Estimates of the added cost vary from 10% to 200%, and it is not difficult to focus on the larger estimates.

Even if the public had been made to understand more of the complexities of results, as long as there is a significant political block believing that any work disincentive is unacceptable, the NIT experiments were bound to give ammunition to NIT opponents. To that extent it was a mistake for any guaranteed income supporters to agree to the experiments in the first place. Robert Reischauer (1986) asked what would have happened if the introduction of Social Security had been preceded by a similar experiment? It would certainly have shown that people saved less for their retirement, retired sooner than they otherwise would have, and relied less on traditional feelings of family responsibility for elders. Such findings would have challenged prevailing norms and would have given considerable ammunition to Social Security opponents. But there is a danger in focusing too much on the strategic value of the experiments to supporters and opponents. There is more to scientific inquiry than political advantage. The experiments were not a propaganda device, and although what we learned form them was tentative and limited, it is worth knowing.

Why was the limitation of a missing demand response treated so lightly? Perhaps, as a general trait, scientists like to focus on the results of their research, not its limitations. Perhaps, those presenting the data might have assumed this fact was too obvious to be bothered with among social scientists or too difficult to be dealt with by a lay audience. Perhaps, opponents didn't want to bring it up because it waters down their argument that the work disincentive is "large" and the costs are "high." Perhaps, supporters didn't want to bring it up because it is easier to make the case that the work-disincentive is "small" than to make a case that a work disincentive would have a desirable effect on wages. Using the small argument requires only an objective look at empirical evidence—if one can objectively define small. But using the desirability argument requires not only empirical data that the experiments could not produce, but also a much more complex normative argument. It affronts those who want to keep wages low to keep profits high and those who espouse the extreme version of the work ethic stating that everyone without property must at all times even at poverty wages.

To those who believe that low-wage workers need more power in the labor market, the NIT experiments demonstrated the feasibility of a desirable program. To those who believe all work-disincentives are bad, the experiments demonstrated the undesirability of a well-meaning program. These normative issues separate supporters from opponents of the basic income guarantee, and therefore, the NIT experiments, as long as they are discussed, will always mean different things to different people. Either side can spin the results, but that's not how science should be used. It is better to understand that the NIT experiments were able to shed a small amount of light on the positive issues that affect this normative debate. They we able to indicate only that a basic income guarantee is financially feasible at a cost of certain side effects that people with differing political beliefs may take to be desirable or disastrous. To claim more would be to overstate the evidence.

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[13] I'm sure I missed some. There is some repetition of papers published both as journal articles and as book chapters, and there was some subjectivity in the judgment of what constitutes "largely" and "published"—my apologies for any omissions. In addition to the published articles, there are at least 200 more unpublished memorandums, reports, discussion papers, and other unpublished works on the experiments as well. Many (but not all) of the unpublished articles were simply early version of later published works. For a bibliography including many of the unpublished articles on the NIT experiments, see the working paper version of this article: USBIG Discussion Paper No. 38, "A Failure to Communication: The Labor Market Findings of the Negative Income Tax Experiments and their Effects on Policy and Public Opinion" at http://www.usbig.net.

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Table 1: Summary of the Negative Income Tax Experiments in the U.S. & Canada

Name	**Location(s)**	**Data collection**	**Sample size: Initial (final)**	**Sample Characteristics**	**G***	**t****
The New Jersey Graduated Work Incentive Experiment (NJ)	New Jersey & Pennsylvania	1968-1972	1,216 (983)	Black, white, and Latino, 2-parent families in urban areas with a male head aged 18-58 and income below 150% of the poverty line.	0.5 0.75 1.00 1.25	0.3 0.5 0.7
The Rural Income-Maintenance Experiment (RIME)	Iowa & North Carolina	1970-1972	809 (729)	Both 2-parent families and female-headed households in rural areas with income below 150% of poverty line.	0.5 0.75 1.00	0.3 0.5 0.7
The Seattle/Denver Income-Maintenance Experiments (SIME/DIME)	Seattle & Denver	1970-1976, (some to 1980)	4,800	Black, white, and Latino families with at least one dependant and incomes below $11,00 for single parents, $13,000 for two parent families.	0.75, 1.26, 1.48	0.5 0.7, 0.7-.025y, 08-.025y
The Gary, Indiana Experiment (Gary)	Gary, Indiana	1971-1974	1,799 (967)	Black households, primarily female-headed, head 18-58, income below 240% of poverty line.	0.75 1.0	0.4 0.6
The Manitoba Basic Annual Income Experiment (Mincome)	Winnipeg and Dauphin, Manitoba	1975-1978	1,300	Families with, head younger than 58 and income below $13,000 for a family of four.	C$3,800 C$4,800 C$5,800	0.35 0.5 0.75

* G = the Guarantee level.
** t = the marginal tax rate
Sources: Robins et al (1980), Feber and Hirsch (1978), Hum and Simpson (1993a)

Table 2: Summary of findings of work reduction effect

Study	Data Source	Work reduction* in hours per year ** and percent			Comments and Caveats
		Husbands	Wives	SFH	
Robins (1985)	4 U.S.	-89 -5%	-117 -21.1%	-123 -13.2%	Study of studies that does not assess the methodology of the studies but simply combines their estimates. Finds large consistency throughout, and "In no case is there evidence of a massive withdrawal from the labor force." No assessment of whether the work response is large or small or its effect on cost. Estimates apply to a poverty-line guarantee rate with a marginal tax rate of 50%.
Burtless (1986)	4 U.S.	-119 -7%	-93 -17%	-79 -7%	Average of results of the four US experiments weighted by sample size, except for the SFH estimates, which are a weighted average of the SIME/DIME and Gary results only.
Keeley (1981)	4 U.S.	-7.9%			A simple average of the estimates of 16 studies of the four U.S. experiments
Robins and West (1980a)	SIME/ DIME	-128.9 -7%	-165.9 -25%	-147.1 -15%	Estimates "labor supply effects." It goes without saying that this is different from "labor market effects."
Robins and West (1980b)	SIME/ DIME	-9%	-20%	-25%	Recipients take 2.4 years to fully adjust their behavior to the new program.
Cain et al (1974)	NJ	-	-50 -20%	-	Includes caveats about the limited duration of the test and the representativeness of the sample. Notes that the evidence shows a smaller effect than nonexperimental studies.
Watts et al (1974)	NJ	-1.4% to -6.6%	-	-	Depending on size of G and t
Rees and Watts (1976)	NJ	-1.5 hpw** -0.5%	-0.61%	-	Found anomalous positive effect on hours and earnings of blacks.
Ashenfelter (1978)	RIME	-8%	-27%	-	"There must be serious doubt about the implications of the experimental results for the adoption of any permanent negative income tax program."
Moffitt (1979a)	Gary	-3% to -6%	0%	-26% to -30%	No caveat about missing demand, but careful not to imply the results mean more than they do.
Hum and Simpson (1993a)	Mincome	-17 -1%	-15 -3%	-133 -17%	Smaller response to the Canadian experiment was not surprising because of the make-up of the sample and the treatments offered.

* The negative signs indicate that the change in work effort is a reduction
** Hours per year except where indicated "hpw," hours per week.
NJ = New Jersey Graduated Work Incentive Experiment
SIME/DIME = Seattle / Denver Income Maintenance Experiment
Gary = Gary Income Maintenance Experiment
RIME = Rural Income Maintenance Experiment
Mincome = Manitoba Income Maintenance Experiment
SFH = Single Female "head of household."

Table 3: Labor market findings other than simple work-effort reduction

Study	Data Source	Findings	Comments and Caveats
Robins, Tuma, and Yaeger (1980)	SIME/ DIME	Increase in length of spells out of employment: Husbands: 9.4 weeks, 27% Wives: 50 weeks, 42% Single Females: 56 weeks, 60%	The experimental group was somewhat more likely to leave employment and substantially more like to remain nonemployed for longer spells than the control group.
Tuma and Robins (1980)	SIME/ DIME	Change in rate of entering employment: Husbands: -22.2 Wives: -39.6 Single female heads: -35.4	Conditional having become nonemployed. This reflects the fact that the labor-hours reductions were attributable more to longer spells of unemployment than to reductions in weekly hours of work.
Hall (1976)	NJ	Opt out rate: 125-50 plan: 13% 100-50 plan: 25% 50-50 plan: 94%	These are the percentages of participants in the study who received no benefits. But the results depend substantially on the participants pre-experimental income.
Robins (1984)	SIME/ DIME	Does not find evidence that 3-year and 5-year studies were biased relative to the response of the 20-year treatment group.	The available evidence is limited.
Cogan (1983)	NJ	Husbands reduce labor effort by -5 to -7 hours per week, conditional on participation	This estimate was only for the sub-sample of that actually received payments and so is not directly comparable to the estimates of labor response in table 2.
Moffitt (1979b)	Gary	Eligible low income population: -4.5% Total population: -1.6% The effect of an NIT on labor supply could be offset by unemployed workers if there is sufficient slack in the labor market.	Simulation model, does not take demand into account, but warns, "Assuming the labor-supply curve is forward-sloping, which it probably is at low age rates, the experimental estimates over-state the final impact on employment (due to a demand response)."
Keeley et al (1978b)	SIME/ DIME	Predicted labor supply response of a national program: Husbands: -5.3% Wives: -22.0% SFH: -11.2%	Applies the experimental parameters for labor supply functions to a national data base to obtain estimates of the nationwide aggregate labor effect and so these findings are not directly comparable to those in Table 2. Finds that the results vary wide with the generosity of the program.
Greenberg (1983)	SIME/ DIME	Response of the demand for labor had a small mitigating effect on hours.	Results depended on assumptions on the level of unemployment and the elasticities of demand and supply of labor and the substitutability and availability of workers making similar wages to those eligible for NIT.
Keeley et al (1978a)	SIME/ DIME	Labor Supply response accounts for 23% to 55% of programs with a positive net cost. That is, cost before labor supply response is 45% to 77% of total cost.	Range depends on the size of G and t. Justifies the assumption of perfectly elastic demand on employers' ability to substitute high-wage, high-skilled workers for workers who are likely to be affected by an NIT.
Robins (1980a)	SIME/ DIME	Replacement of the 1974 welfare system with an NIT would have cost an additional $2.2 billion to $30 billion ($55 to $97 in 2004 dollars). The work-effort response would add $0.2 to $7.0 billion ($0.6 to $23 in 2004 dollars) to cost.	Range of responses depends on the size of G and t. Demand response not included.
Rees and Watts (1976)	NJ	Increase tax cost due to supply response: 5 to 10%	Demand response not included.
Ashenfelter (1978)	RIME	Estimates that the cost before the labor supply response would only 78% of the cost after the labor supply response.	Demand response not included. Findings could be restated to say that the work-effort response adds 28% to the transfer cost.
Burtless (1986)	4 U.S.	$3 in transfers raises the income of recipients by only $1. Poverty among all families with children could be eliminated for an additional cost of $61 billion ($98 in 2004 dollars).	Demand response not included.
Maxfield	SIME/ DIME	Labor supply response is highly correlated to the generosity of the NIT program.	Demand response not included.
Bishop (1979)	SIME/ DIME	"Reduction in labor supply produced by these programs does tend to raise low-skill wages, and this improves transfer efficiency."	General equilibrium model focusing on efficiency effects, and so results are not directly comparable to those focusing on tax cost. Results are sensitive to assumptions

Figure 1: The vertical axis shows the wage (W), the horizontal axis shows the hours worked (H). The work disincentive effect causes the supply among the experimental group to shift from S_0 to S_1. Because the experimental group is small in comparison to the size of the market, the results would reflect a fixed-wage shift in hours worked (at W_A) from point A to point B, which involves only a decline in hours and no increase in the wage. If all workers in the market received the NIT, there would be a movement along the supply curve. The market outcome would go from A to C instead of A to B, increasing the wage to W_C and partially offsetting the decrease in hours worked by difference between H_C and H_B.

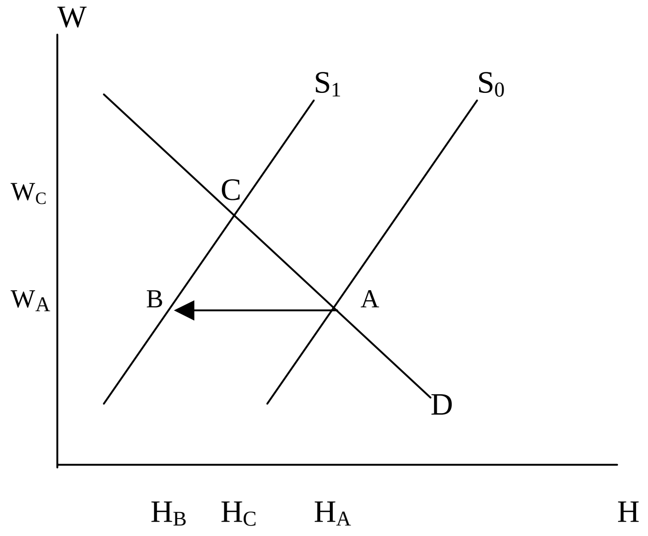


Figure 2: If Demand is completely inelastic, there is no equilibrium reduction in work hours.

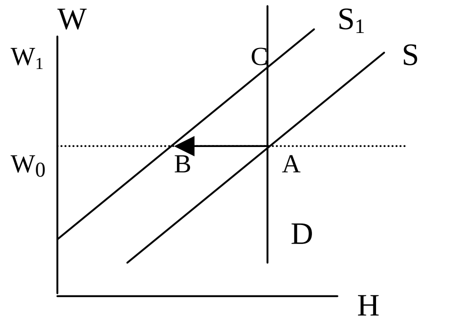


Figure 3: If demand is completely elastic, there is no change in the wage, and the full reduction in work hours in the experiments would occur in the market.

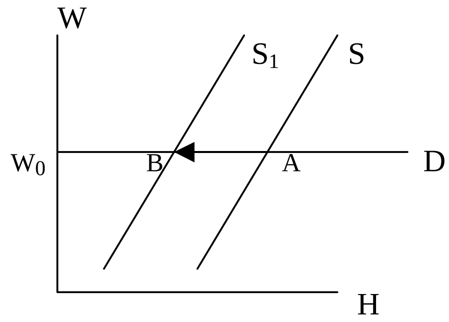
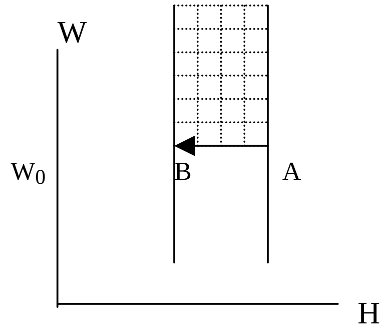


Figure 4: The range of possible market responses to a given horizontal shift in the supply of labor.



KATHOLIEKE
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WIN FOR LIFE
What, if anything, happens after the introduction of a Basic Income? [1]

Paper prepared for

ESF-workshop 'Towards a European Basic Income' (17/9/2004 – Barcelona)

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[1] Earlier versions of this paper were presented at a seminar at the sociology department of the K.U.Leuven (20/5/2004), at Midis intimes de la Chaire Hoover (Louvain-la-Neuve - 25/5/2004), at the ESF-workshop 'Towards a European Basic Income' (Barcelona - 18/9/2004) and at the 10th BIEN congress (Barcelona – 20/9/2004). We are very grateful to all the participants and especially to Jos Berghman, Albert Martens, Gert Verschraegen, Gijs Dekkers, Kristine Nijs, Annelies Debels, Koen Vleminckx, Ann van den Troost, Axel Gosseries, Yannick Vanderborght, Philippe van Parijs, Ilka Virjo, Sean Healy, Robert van der Veen, Loek Groot, Jurgen Dewispelaere, Robinson Hollister, Manos Matsaganis and Guido Jacxsens for valuable comments or information. We also like to thank Nadia Fadil, Sakura Yamasaki and Margot Van Baelen for translating the Dutch questionnaire in French and Carla Ons for preparing and encoding the surveys. Special thanks further to the Belgian National Lottery, and especially An Lammens, for supporting this pilot-project and coordinating the distribution of the questionnaires.

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INTRODUCTION

In an interesting book "*Seeing Like a State. How Certain Schemes to improve the Human Condition have failed",* James Scott (Scott, 1998) warned against implementing Big Ideas without thoughtful empirical consideration or experimentation. Local diversity and unintended consequences, among other things, made theoretical ideas for societal improvement go astray.

Although the cases analysed by Scott were mostly technical engineering cases – building cities, increasing agricultural productivity, etc. – his message can be extended to other Big Ideas. One such idea that has received increasing attention is a Universal Basic Income (hereafter Basic Income). "A Basic Income [is] (...) an income paid by a government, at a uniform level and at regular intervals, to each adult member of society. The grant is paid, and its level is fixed, irrespective of whether the person is rich or poor, lives alone or with others, is willing to work or not (Van Parijs, 2003, p. 5)". A Basic Income is defended on various grounds. "Liberty and equality, efficiency and community, common ownership of the earth and equal sharing in the benefits of technical progress, the flexibility of the labour market and dignity of the poor, the fight against unemployment and inhumane working conditions, against the desertification of the countryside and interregional inequalities, the viability of co-operatives and the promotion of adult education, autonomy from bosses, husbands and bureaucrats – all have been invoked in favour of a (…) Basic Income (Van Parijs, 1992, p. 3)".[2] Thus, like most Big Ideas, a Basic Income might change society profoundly. Indeed, as Brian Barry (in Groot, forthcoming), stipulates "A subsistence-level basic income would face people with an entirely different set of opportunities and incentives from those facing them now".

Whether and to what extent this different set of opportunities and incentives will result in significant behavioural changes, is an empirical question. Indeed, hypothetically, the introduction of a Basic Income could result in many different micro behavioural changes with distinct macro implications. This has been argued by both proponents and opponents of a Basic Income. In general, several socio-economic and sociological changes can occur due to the introduction of a Basic Income. In this paper we concentrate on changes in labour market behaviour. For example, the introduction of Basic Income might provide an incentive to reduce the amount of time spent on the labour market or even withdraw from the labour market (micro changes). This might result in the abolition or reduction of unemployment since the amount of work will be redistributed over a greater number of people (more people work less). However, when a significant number of people decide to withdraw from the labour market it may create massive shortages on the labour market which can result in economic decline.

[2] For a comprehensive overview of what a basic income is, why we need it and whether it is affordable or not, see (Van Parijs, 2004).

Given these unresolved questions, empirical research into the behavioural consequences of introducing a is of obvious importance. It should be noted however that these empirical questions do not affect all arguments for a Basic Income in an equal way. As indicated by Brian Barry (1996) it is useful to distinguish between pragmatic and principled arguments about a Basic Income. "Pragmatists are those who assume that social policy should serve certain ends. (…) [They] suggest that the introduction of basic income would be the most effective way of reforming existing welfare states. (…) In contrast (…) the principled argument seeks to show that the case for basic income can be derived directly from the concept of social justice (Barry, 1996, p. 243)". Evidently, empirical arguments are more important to pragmatics than they are to defenders of principled arguments.[3]

Since empirical arguments are important in the Basic Income debate the question remains on how to proceed with research into this counterfactual phenomenon. True, in Alaska a Basic Income has been introduced, but this is hardly a representative case. For one, the amount of the dividend is too low in comparison to most proposals for a Basic Income. For another, as anyone who visited Alaska will know, the external validity of research in Alaska to any, for example, urban setting is very difficult. Alaska is geographically, but also socially, quite a unique place.[4]

In the absence of the actual introduction of a Basic Income, second-best solutions for empirical research must be considered. A key-challenge for such research is to design a research project which enables researchers to make valid inferences. An "inference is the process of understanding an unobserved phenomenon on the basis of a set of observations (King, Keohane & Verba, 1994, p. 55)". In other words, to what extent do research results enable us to draw valid conclusions about what might happen when a Basic Income is introduced?

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[3] Of course, the dividing line between pragmatic and principled arguments is not always that clear cut. As Van Parijs (1992, p. 29) argues "The importance of such arguments [that derive basic income from an explicit formulation of the ideal of a free, equal or good society] does not make more limited efficiency arguments irrelevant, (…) because many of these fit, as partial components, into arguments of the more ambitious sort (…)" Furthermore, utilitarian inspired defenses of a Basic Income (for instance most green arguments for a basic income) can be wedded to a certain conception of justice but entirely depend on pragmatic arguments.

[4] Another possible interesting actual implementation of a Basic Income might result from the recent June 2003 reforms of the Common Agricultural Policy (CAP) of the EU which includes a shift from production-based subsidies to direct payments to farmers which will provide them with a guaranteed minimum level of income that is not linked to production. The basic idea is to replace most of the direct subsidy payments for farmers by a single farm payment. The European Commission states that "A major aim of the single payment is to allow farmers to become more market-oriented and to release their entrepreneurial potential. Management decisions that in the past have been influenced by what the CAP offered in subsidies can now be taken on the basis of market requirements. Where a particular production activity is profitable farmers will continue to follow it. The reformed CAP is designed so that farmers take advantage of such opportunities (European Commission, 2004)". The member states will have to decide on the specific implementation of the reform. It is however important to note that the amount of the payment will be calculated on the basis of the direct subsidies farmers received in a reference period (2000 to 2002). In addition, the payment is not unconditional but conditional on the fact that beneficiaries of direct payments will be obliged to keep their land in good agricultural and environmental condition. Even though a single farm payment is not equal to a Basic Income, it might still constitute an interesting case.

There are limitations for any research project which cannot be overcome due to the nature of the proposal. For example, the introduction of a Basic Income and a related partial deregulation of the labour market, will clearly influence the demand side of a labour market which can result in different wages, the emergence of new types of previously undervalued jobs, etc. Since this will affect the entire labour market one cannot empirically assess the impact on the demand side before the effective introduction of a Basic Income.[5]

However, questions related to human behaviour are open for empirical investigation. Preferences in relation to willingness to work are assumed not to be that different before and after the introduction of a Basic Income.[6] The Basic Income might influence the capability to implement preferences (for example maximising free time) but not necessarily the preferences as such. In other words, the claim that people would retreat from the labour market once a Basic Income is introduced is open for empirical investigation.

Since many factors influence labour market behaviour the challenge for research is to design a project which takes into account the complexity of this behaviour. To this end two possible research designs can be thought of which both rely on the logic of an experiment. [7] As Groot (forthcoming) has argued "There

[5] For a discussion on the hypothesized effects of a Basic Income on the demand side of the labour market, see (Widerquist, forthcoming).

[6] It could be argued that it is sheer impossible to conduct research since the political and normative context in which a Basic Income will be implemented will be significantly different to any existing situation. This change in context might legitimise behaviour which is now regarded as politically and socially 'unacceptable' such as voluntary unemployment. The introduction of a Basic Income founded on clear normative principles for societal ordering and development supported by a clear political majority will imply a transformation of the concept of work and contribution to society which cannot be compared to any existing situation. As a consequence, empirical research is bound to be impossible. This argument, however, could result in a Catch-22 with regard to the effective implementation of a Basic Income since empirical arguments are clearly important in the political discussion of a Basic Income. A Catch-22 is an impossible situation where one is prevented from doing one thing (empirical research) until one has done another thing (introducing a Basic Income), but one cannot do the other thing (introducing a Basic Income) until one has done the first thing (empirical research). For example, Pels and van der Veen (1995) report in the case of the Netherlands, that many arguments of the opponents of a Basic Income concern the negative effects of a Basic Income on human behaviour. These are empirical arguments about how an unconditional income will influence human behaviour. The Catch-22 then consists out of the following paradox: the argument that it is impossible to do empirical research before one introduces a Basic Income will result in the impossibility of implementing a Basic Income since one needs empirical arguments to make a valid political case. An insight – via empirical research - in what happens when people receive an unconditional income might break the Catch-22.

[7] No doubt, other research-designs may exist. First of all, one could re-analyse existing socio-economic databases. In this case, one could argue that since introducing a Basic Income has mainly to do with income-effects, one can rely on existing survey material and official statistics to analyse the effect of increases in income on several parameters such as labour supply, entrepreneurship, etc. However, this approach is limited since existing datasets hardly ever contain information about significant exogenous non-earned incomes (Imbens et al., 2001, p. 779). Furthermore, existing databases do not contain any information about periodically paid exogenous non-earned income similar to a Basic Income. This makes it almost impossible to make any inferences from such databases to a Basic Income situation. Secondly, one could survey people and ask them what their attitude is towards a Basic Income and what they *might* do under Basic Income conditions (for instance, see (Késenne & Van Durne, 1989)). However, the results of such a research-strategy are hard to interpret since there is an important difference between attitudes (what people say) and behavior (what people do). Although one could argue that attitudes influence behavior (opinion and attitude research in sociology), the relationship can also be reversed (see for example cognitive dissonance theory in

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." The beauty of an experiment is that it allows researchers to put people in different possible worlds.

One possibility is to conduct a genuine experiment. An experiment is a research design in which an 'independent' variable is manipulated under controlled conditions. As such, an experiment consists of two essential elements, namely the manipulation of a causal factor and the control – mainly via random selection – of all factors that might plausibly affect the causal relationship of interest (Gerring, 2001; Orr, 1999). Via an experiment – and the effective creation of a Basic Income situation – one would be able to monitor what will happen in the experimental group and how this differs from a control group. A Basic Income experiment has never been implemented but has recently been proposed by Groot (forthcoming).

However, an experiment has some limitations with regard to making valid inferences which, at least on theoretical grounds, might be challenged and need further assessment. The most important limitations for Basic Income research, mainly resulting from financial barriers, relate to the difficulty of including variation in Basic Income design in order to analyse variation in behavioural outcomes, running the experiment over a sufficient amount of time and expanding the experimental (and control) groups in order to take institutional effects into account.

Hence, in order to make valid inferences an experiment should be complemented with other types of research. It is only by complementing experimental research with other empirical research that insight into the consequences of introducing a Basic Income might be gained. This can be achieved by making use of natural experiments, such as cases where people receive windfall gains.[8] In a natural experiment the change in the causal factor is provided by contingencies, such as natural occurring phenomena or social interventions, which are independent of the research-project. Promising natural experiments in this context are lotteries. Indeed, lotteries organise interesting games for Basic Income researchers. Some games – such as Win for Life, Lifetime Spectacular, Lifetime Riches, Weekly Bonus, Fun for Life, Lucky for Life, etc. - grant a periodically unconditional lifelong income to winners (cf. annuity games). In this way, they constitute a natural Basic Income experiment and can generate significant insights into the possible consequences of introducing a Basic Income. The strength of this type of natural experiment is

psychology). At this point there is no consensus in the social sciences on how to draw inferences from the measurement of attitude to behaviour. The problems for interpretation are worsened by the fact that one investigates a counterfactual phenomenon.

[8] For more references on this type of research see (Imbens et al., 2001).

that it can include variation in Basic Income design, is possibly unlimited in time and can take into account different institutional settings. A major drawback is that the attribution of people to the experimental and control group is not ad random and selection bias might hamper generalization.

The aim of this paper is twofold. First of all, the paper discusses why, how, and to what extent, natural experiments such as lotteries can contribute to research which empirically explores possible social consequences of the introduction of a Basic Income. The second aim is to focus on the question of what, if anything, happens after the introduction of a Basic Income.

The paper is structured in three parts. The first part of the paper addresses the question of why natural experiments constitute an interesting research-strategy. Via a comparison with a genuine experiment a theoretical case is made to conduct lottery research, which has some distinctive strengths vis-à-vis an experiment.

The second part of the paper discusses an ongoing pilot-project which investigates the consequences of winning the Belgian lottery game Win for Life, which grants every winner an unconditional lifelong monthly allowance of 1.000 euro. It is assessed to what extent this game represents a good proxy for a Basic Income and what conclusions can be drawn from it.

In a third part, a proposal for the extension of lottery research is suggested. A genuine research-program based on this natural experiment should cover multiple countries and different types of Basic Income design. Such a research-program will allow for comparison across institutional settings and can contribute to the debate on the behavioural consequences of a Basic Income versus stakeholder grants. The ultimate aim of the research-project is to build a large panel dataset (including several experimental and control groups) which allows for this type of comparison.

1. WHY NATURAL EXPERIMENTS? STRENGTHENING THE CASE FOR LOTTERY RESEARCH

An investigation into the behavioural consequences of a Basic Income could be done via an experiment. Groot recently elaborated a proposal for a Basic Income experiment. This experiment "*would involve (4) a limited group of people in (3) a limited area who would, during (2) a limited time, receive (1) a basic income*". However, several limitations affect the possibility to make valid inferences based on such an experiment. It is furthermore argued that lottery research has some distinctive strengths to address these limitations. This first part elaborates on these limitations and assesses in what way lottery research can complement a Basic Income experiment.[9]

The most important limitations of an experiment, and hence, challenges for another research-design relate to the following aspects:

(1) *A Basic Income*: The experiment does not take into account differences in Basic Income design. However, different types of Basic Income design might result in different behavioural consequences. This implies that a challenge for additional research is to analyse different consequences of different designs.

(2) *A limited time:* The experiment attaches limited importance to time. However, behavioural changes might spread out unevenly over time which might result in biased results when an experiment is conducted over a limited period of time. The challenge then becomes to design a genuine longitudinal research-project.

(3) *A limited area*: The experiment is confined to a limited area (one country) and therefore neglects the importance of institutions. It might be argued that different institutional contexts might generate different outcomes. A third challenge then becomes to design a multi-institutional (multi-country) design.

(4) *A limited group of people:* The experiment only considers the consequences of introducing a Basic Income for social assistance recipients, workers who would earn the same amount of money before and after the introduction of a Basic Income, and prospective entrepreneurs. Because these groups only form a subset of existing socio-economic groups a challenge for additional research is to analyse the effect of introducing a Basic Income for a more representative sample of the population.

[9] It should be noted that the discussed limitations are not necessarily important, but that one should not assume they are unimportant.

Each of these topics will be elaborated upon.

1.1. A BASIC INCOME

A first limitation concerns the fact that an experiment typically would test the effect of only one Basic Income design. However, one could easily hypothesise that a divergent Basic Income design will differently influence labour market behaviour.[10] In other words, variation in Basic Income design can generate variation in outcomes.

Two issues are important: the level of the awarded income and the frequency of payment. First of all, different amounts of Basic Income will have different behavioural consequences, including effects on labour supply. A high Basic Income will provide more incentives to reduce working time than a low Basic Income. Since no previous empirical research has been conducted on the consequences of granting varying unconditional benefits at equal time intervals, the different incentives this would bring along remains a highly theoretical undertaking. Existing research on winners of regular (non-annuity) lottery games, however, points to the inverse relationship between the amount won and the probability of changing working behaviour. Thus Kaplan (1985) found that there was a significant association between the amount a person won and his or her working behaviour. As the size of the winning increased, so too did the number of changes in working behaviour. "Nearly twice as many winners and spouses in the under- $10.000 category kept working as winners of $30.000 (Kaplan, 1985, p. 90)." In addition, Imbens, Rubin and Sacerdote (2001) found that unearned income, resulting from winning the lottery, reduces labour supply with larger effects for individuals between 55 and 65. Similarly, research into the labour market effects of inheritances suggests the existence of an inverse relationship between large inheritances and a person's labour force participation (Holtz-Eakin, Joulfaian & Rosen, 1993).

[10] There is possibly an additional issue at stake in this context. In an experiment one does not grant a Basic Income but an experimental grant. The extent to which an experimental grant equals a Basic Income in behavioural consequences is a question of 'fungibility'. Fungibility is the premise according to which all instances of a given commodity that meet certain standards are considered interchangeable. Since money is assumed to be the ultimate objectifier (Zelizer, 1989), it is assumed that amounts of money are interchangeable. However, several streams of research have questioned this assumption and argue that depending on the source of money people behave differently (Thaler, 1992). Therefore, one needs to address the question to what extent an income received by participating in an experiment is a good proxy for a Basic Income. 500 euro granted as an experimental grant might not be interchangeable – in terms of behavioural consequences – with a 500 euro granted as a Basic Income. This effect might be reinforced by the fact that only a very small proportion of people receive an experimental income. This will set the group apart from the rest of society, which could possibly result in attaching a very special meaning to the received money. It is hard to assess the importance of this fact, but the possible bias should be taken into account in interpreting research results. A comparison between results of lottery research and experimental research might be very instructive in this context. A careful comparison of cases from the experimental group and the lottery group might generate insight into the behavioural effects of experimental and lottery grants.

In addition, a Basic Income design can vary according to frequency of payments. A Basic Income can be paid weekly, monthly, yearly or as a lump sum (in case it is most often called a 'stakeholder grant').[11] This choice is possibly not without implications. It might be argued that people will behave differently under different frequencies of payments due to different mental accounting processes which refer to the fact that people develop different preferences when a similar amount of money is offered under different conditions (Langer & Weber, 2001; Zelizer, 1989). These different conditions might include different time frames. As a result, the design of a basic income in terms of frequency might not be neutral. Indeed a trade-off seems to be at stake. As Van Parijs (1995, p. 48) points out "The shorter the period, the better the real freedom (…) of later stages is protected against irresponsible conduct at earlier stages, but the more restrictions on the time scale of the commitments one is empowered to make".

Lottery research can address design limitations because several interesting variants of games exist both in terms of frequency as well as in terms of amount of money. Some games award a one time lump sum payment while others offer a lifelong grant on periodical time intervals. Within these 'lifetime games' different variations exist. Some games – such as Lucky for Life - guarantee a weekly grant. Other games -– such as Win for Life – pay a lifelong monthly income. Still other games – for instance Fun for Life - pay a yearly income. Besides these differences regarding frequency of payment, different modalities of most games exist in terms of level of payment. This makes it possible to compare different levels of payments. All this makes lottery research especially interesting to compare the consequences of a Basic Income with a stakeholder grant.[12]

1.2. A LIMITED TIME

A second limitation for valid inferences of an experimental design concerns the importance of time. Two issues are of importance in this context. First of all, the time frame in which people will change their behaviour is unknown. However, there is no theoretical reason to assume that behavioural changes will reflect any 'general linear reality' (cf. Abbott, 2001). Secondly, a limited time period might bias the answers on behavioural changes resulting from the experiment.

[11] Proposals regarding frequency of payment often coincide with different national traditions in organising social security benefit payments. Most proponents of a Basic Income in the UK propose a weekly payment, while in Belgium a monthly payment is proposed (Van Trier, 1995).

[12] It should be noted that experimental research as such can easily incorporate variation in Basic Income design. Different levels of grants could be given to different experimental groups. The same holds for granting the same amount at different frequencies. These different experimental groups could subsequently be compared with one control group. However, even though theoretically an experimental research design could take various differences in Basic Income design into account, this would in practise become very costly.

1.2.1. General Linear Reality

Introducing a Basic Income might be labelled an innovation. Research in several different areas has shown that the diffusion of an innovation - and behavioural adaptations to this innovation – is among other things a function of time. As a consequence, behavioural effects of introducing a Basic Income will be time-dependent. There is no reason to assume that introducing a Basic Income will have some kind of tornado-effect (short causes – short outcomes) where you can directly observe the behavioural consequences of introducing such a scheme. Introducing a Basic Income can be more akin to an ecological adaptation process where you have a short time-horizon on the side of the cause but a very long time horizon on the side of the outcomes (Pierson, 2003). More in general, the development of time-dependent modes of (statistical) analysis such as event-history analysis clearly emphasize the importance of time on behavioural adaptations (Tuma & Hannan, 1984, pp. 187-264). Therefore, time poses severe challenges for empirical research into the behavioural effects of introducing a Basic Income. It would necessitate a long period of observation which would dramatically increase the cost of an experiment. By contrast, lottery research, when designed longitudinally, could eventually generate insights into time-effects.[13]

1.2.2. Bias in Behaviour

A second issue concerns the possibilities of biased results due to a limit in the time-frame in which the experiment is conducted. Widerquist (forthcoming), commenting on the Negative Income Tax experiments of the '60s and '70s in the United States, notes that the limited time frame of the experiment might result in biased results, because experiments run the risk of measuring only short time responses to a policy change. He notes, for example, that participants in the experimental group might, on the one hand, face a great incentive to trade working time for leisure time since they now have the financial capabilities to do so. On the other hand, since people have to return to work after the experiment it might provide an incentive to stay in a job in order not to loose it. In other words, experiments might over- or underestimate behavioural consequences due to time constraints (Widerquist, forthcoming). As noted before, lottery research has an advantage in this respect because some annuity grants are unlimited in time

[13] Closely related to the issue of time is the issue of social influence which determines behavioral change. Time and social influence will interact to produce behavioral changes. For example, threshold models have been developed to show that in many cases a critical threshold (cf. tipping point) has to be reached before a significant number of people will change behavior. This line of research has recently gained much momentum with the focus on social networks. The adoption of innovation or the imitation of behavior mainly occurs via networks which transfer information (Gladwell, 2000; Granovetter, 1978; Schelling, 1978). In relation to introducing a Basic Income, the above might imply that at first few or insignificant changes in labour market behaviour will occur, but as time goes on and a certain threshold is reached, many others will follow. For instance, once a few people shift from full-time to part-time work and can still afford a decent life, more people will start to do the same. In many cases these developments are non-linear and extremely hard to model. The crucial issue here is that behavioural effects are not only a result of rational decisions, but also of social contagion which is time-dependent. This contagion mechanism might take a very long time to become effective. The latter can hardly be empirically assessed since it requires a high number which create a sufficiently dense network in which contagion can occur.

and in this way do not provide specific time-related incentives or disincentives to change labour-market behaviour.

1.3. A LIMITED AREA

Some Basic Income proponents argue for the introduction of a Basic Income in multiple countries at the same time. Hence, research into behavioural effects should take into account the differences between countries that might result in different behavioural changes. The social science literature on structural and cultural differences between countries is significant and points to the fact that differences between nations are pronounced.[14] It is therefore difficult to generalize from one country to another.

In other words, the introduction of a Basic Income will not occur in a vacuum. It is hypothesised that the willingness and possibilities to change labour-market behaviour is a function of the societal context.[15] First of all, institutions matter for preference formation and willingness to change preferences. Cultural institutions such as norms and expectations regarding work ethic might influence a change of working time for leisure time or care time in a given society.

Secondly, institutions matter in relation to the possibilities they provide for implementing preferences. Several authors link the institutional structure of labour markets (structure of decision making, institutions for collective bargaining, laws, etc.) to outcomes on the labour market such as participation on the labour market, unemployment rates, spread of labour market activity over a lifetime, etc. (Madsen, Madsen & Langhoff-Roos, 2003; Hall & Soskice, 2001; Wallerstein, 1999). Clearly, countries differ regarding institutional structure of labour markets. This may interact with the introduction of a Basic Income. For example, in a country in which part-time work is easy to obtain or is institutionalised, it is easier to change labour market behaviour in comparison to countries where labour markets are more rigid. These interactions could be very considerable since, in our understanding, Basic Income proponents do not propose a complete abolishment of the institutional fabric of the labour market. Although proposals for a Basic Income imply a deregulation of the labour market, they do not suggest a complete abolishment of the institutional fabric of the labour market. Many institutions will continue to play an important part.

One should therefore not assume that the behavioural effects of the introduction of a Basic Income in different institutional settings will be the same in every country. Moreover, research into the interaction

[14] The importance of cultural differences is highlighted among others by (Inglehart, 1990; Inglehart, 1998). Structural differences are emphasized by authors such as (Esping-Andersen, 1990; Hall & Soskice, 2001; Kitschelt, Lange, Marks & Stephens, 1999; Madsen et al., 2003).

[15] Context is defined as different institutional characteristics of the country in which a scheme is implemented. Institutions include laws, rules, norms, values co-ordinating organisations, etc. which provide incentives and disincentives to change behaviour (Ostrom, 1990).

between a Basic Income and different institutional settings might generate insights in which labour markets or economic development policies best complement Basic Income schemes.

As a result, research should take institutional variation into account when designing a research-project. Experiments which are confined to one country cannot consider institutional and cultural effects. Experiments can be conducted in several countries. However, this will dramatically increase the cost of conducting such experiments. Lottery research, on the other hand, can take into account institutional and cultural variation since similar games are played in different countries.

1.4. A LIMITED GROUP OF PEOPLE

Different socio-economic groups will react differently to the introduction of a Basic Income. Therefore, it is important to empirically address this issue. The proposed experiment is limited in this regard. Due to financial constraints one has to focus mainly on those groups whose income before and after the introduction of a Basic Income would be quite similar. These groups are, first, the social assistance recipients because they already receive a substantial income without performing work. Second, also those workers can be included in the experiment whose incomes are around the break-even level. At the break-even point the net income one receives under the current conditional income scheme and the Basic Income scheme is exactly the same because the unconditionally granted income is entirely offset by the higher level of taxes that have to be paid to finance the Basic Income (cf. infra). The income of a third group, the prospective entrepreneurs, is different before and after the introduction of a Basic Income, but this group would be included because of their theoretical importance (Groot, forthcoming).

Because the three mentioned groups only form a small and biased sample of the general population it is interesting to supplement an experiment with lottery research whose sample includes many different kind of socio-economic groups.

An additional problem with regard to experiments concerns the Hawthorne-effect, this is the fact that people – possibly under media influence - will adopt their behaviour in favour of the experiment (Gillespie, 1993). It will be very hard to exclude the experimental group from information on expected behavioral outcomes of the experiment. Once this information is available, the experimental group may act accordingly. Regarding this Hawthorne-effect lottery research is preferable to experimental research. Even though in lottery research a similar Hawthorne-effect might occur, it should be noted that in case of lotteries the money is independently provided by state lotteries and not by the experimenters. This puts the research-population in a more independent (non-reciprocity) relationship to researchers which will generate less pro-active behaviour by the experimental group. Lottery winners have fewer incentives to be grateful to the researchers and behave in a socially desirable way.

2. WHAT, IF ANYTHING, HAPPENS AFTER THE INTRODUCTION OF A FULL BASIC INCOME?

The theoretical case in favour of lottery research led to the start up of a pilot project with lottery winners of Win for Life (hereafter W4L). Winners of W4L receive an unconditional monthly grant of 1.000 euro for the rest of their lives.

The pilot project has three aims. First of all, the project wants to explore the *practical* possibilities and constraints of lottery research. Secondly, the project aims to gather information on the impact of winning W4L on the life of the winners. In other words, the project wants to start with an exploration of a possible answer to the question of 'What, if anything, will happen after the introduction of a Basic Income?' Finally, the pilot project aims to create a starting point for future research which will be able to discount some of the challenges discussed above. More specifically, as emphasised in the first part, time plays a crucial role in behavioural changes. The third aim of the project therefore is to build a panel which can be followed through time and will allow for longitudinal research into behavioural consequences of winning W4L.

Even though the similarity between W4L and a Basic Income is striking - both are granted unconditionally, as a monthly instalment until death – the comparability is not straightforward. Therefore, before the pilot-project is presented a further defence of W4L as a valid case for Basic Income researchers is made.

2.1. WHAT CAN WE LEARN FROM W4L-RESEARCH?

The proposal for a Basic Income is not to give everyone a winning lottery ticket. Hence, the question to what extent W4L is a valid case for investigating a possible Basic Income situation needs to be addressed. In order to compare a Basic Income and a W4L situation special attention has to be paid to the difference in tax regime between both situations and the fact that a W4L grant is not adjusted to inflation while a Basic Income is. Furthermore, a distinction has to be made between singles and couples.

2.1.1. Tax regimes and inflation

A first difference between a Basic Income and a W4L situation concerns a difference in tax-regime which will influence the net incomes of singles and couples under a Basic Income and W4L situation. Figures 1, 2 and 3 present the relation between the gross and net income situation of both Basic Income recipients and W4L winners.[16] Figure 1 represents a full Basic Income regime financed with a flat tax (hereafter UBI). Figure 2 represents the case of a *single* W4L winner under the conventional guaranteed minimum income scheme (hereafter GMI/W4L). Figure 3 represents the conventional guaranteed minimum income scheme for one of the two partners in a *couple* which equally divide the winning W4L grant between each other (hereafter GMI/W4L). On the X-axes gross income is presented, on the Y-axes net income. The 45° dotted line represents a situation where no taxes are paid. In this case gross and net income are the same. Note that in Figure 1 two dotted lines are presented. The left line represents the situation where every one receives a Basic Income but no taxes are paid. G represents the subsistence minimum for a single person. The Basic Income is set at the level of this subsistence minimum, irrespective of the household situation, as in most proposals for a full Basic Income. G is set at 580 euro since this is the level of Belgian social assistance for a single (situation 1/1/2004).[17] The line indicated by W4L depicts the W4L grant of 1000 euro.

The difference between the income line (before W4L) and the dotted line points to the amount of taxes that have to be paid. Comparing Figure 1 to Figures 2 and 3, it becomes clear that the amount of taxes that have to be paid under UBI is higher than those paid under GMI/W4L (compare °UBI with °W4L). This seems to be a valid assumption mainly because more people (for instance students and housewives) will receive an income under UBI while they do not have an income under GMI.[18] Combine this higher tax rate with the fact that a W4L grant is not taxed[19], and a first clear difference between UBI and GMI/W4L becomes clear. The consequence is that the net income of a single person winning W4L is clearly higher than the net income this person would have under UBI (compare Figures 1 and 2). The same partially holds for couples, but the difference is less pronounced. For those earning nothing, the situation under W4L and UBI will be the same because W4L<G and hence the net income will be raised to the level of G (under the assumptions made in note 17). However, from a certain gross income level onwards, the net income under GMI/W4L will be higher than under UBI (compare Figures 1 and 3).

[16] Figure 1 is a modified copy of the Basic Income regime as presented by (Van Parijs, 2004, p. 32). Figures 2 and 3 are based on (Van Parijs, 2004, p. 29) but adjusted to the W4L situation. Note that the figures make strong simplifying assumptions. Most important, it is assumed that there is only one flat tax rate, in contrast to the existing progressive tax rate. Furthermore, Figures 2 and 3 assume that social assistance is the only existing transfer income. Finally, Figure 3 presupposes that the situation of one partner in a two person household is the same regarding taxes and transfers (the level of G) as the situation of a single (apart from W4L). Some of these assumptions are discussed in (Van Parijs, 1996). Of course, different Basic Income regimes are possible but the 'Basic Income combined with flat tax' seems to be the most common proposed. For a discussion of different Basic Income designs and possible differences in tax regime see (Van Parijs, 2004).

[17] This amount is comparable with the proposal of the Belgian political party VIVANT which proposes a Basic Income of 540 euro a month for every adult between 18 and 65 (VIVANT, 2004). For a discussion of VIVANT see (Vandenborght, 2000).

[18] Another reason could be the following: because the effective marginal tax rate of those earning less than G is obviously much higher (100 percent!) under GMI than under UBI, the loss of this tax revenue has to be compensated for. See (Van Parijs, 2004, p. 29) for a further clarification.

[19] Belgian lottery winners do not have to pay any taxes on the amount won in the lottery. For a discussion, see (Vernat, 2003).

Figure 1. UBI

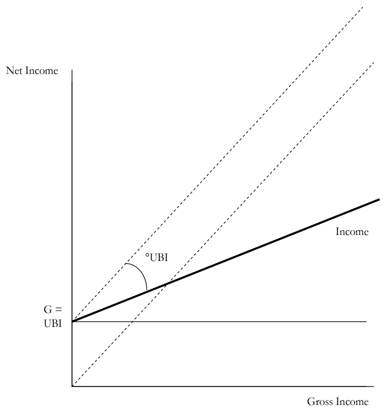


Figure 2. GMI/W4L (single situation)[20]

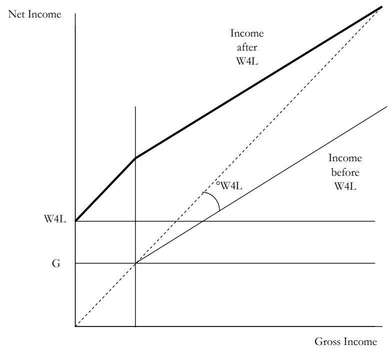
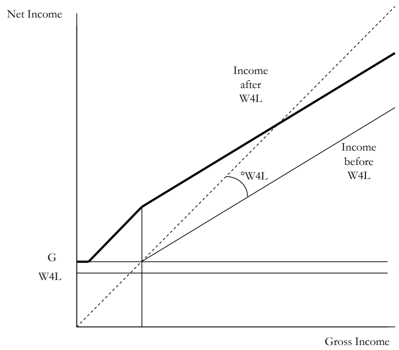


Figure 3. GMI/W4L (one partner in a couple situation)



A second difference concerns the fact that W4L is not price related, while a Basic Income, under every serious proposal, would have to be adjusted for inflation. Assuming a yearly inflation of 2%[21] (as in Figures 4 and 5) this would mean that while someone who has won W4L in 2000 will still receive 1000 euro in 2030, the real value of the grant will have been diminished to 545 euro. The real value of a Basic Income will at that time still be 580 euro. As becomes clear from Figure 4 for singles this mean that the W4L grant will for a very large part of their lives be higher than the Basic Income but at some point the two grants will have the same value (in this example this will be in 2028), and after that point the Basic Income grant will be higher than the W4L grant. For one partner in a couple situation this means that the difference between a W4L grant and the Basic Income (with UBI > W4L, see Figure 5) will become larger as time passes.

Figure 4. Evolution W4L versus Basic Income grant (single situation)

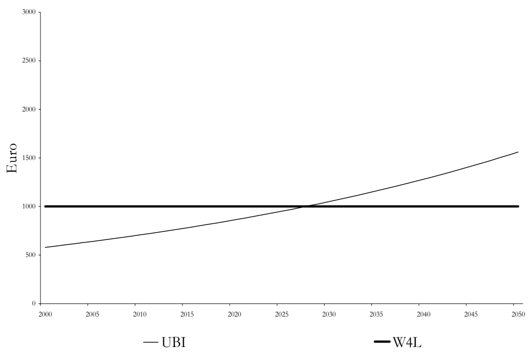
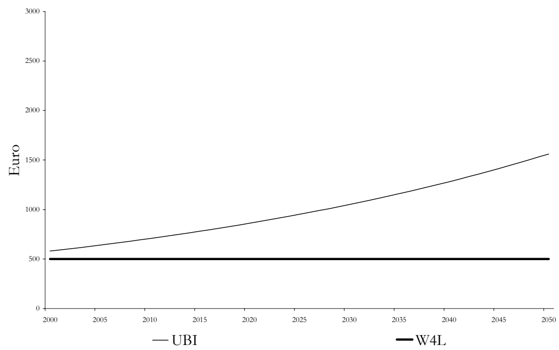


Figure 5. Evolution W4L versus Basic Income grant (one partner in a couple situation)



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20 It is assumed that no income tax has to be paid until someone earns a gross income of G. Hence, the angle representing the tax rate of W4L winners with a gross income from work below G equals 45%.

21 The figures are purely illustrative. However, 2% inflation seems to be a realistic estimate. According to World Bank figures average inflation (calculations based on consumer prices) in Belgium for 1990-2002 was 2.1%.

As will become clear in the next section Figures 4 and 5 are important. However, notice that not only the level of the grant but also the tax regime will be different under GMI/W4L and UBI (cf. supra). Recall that the tax rate necessary to finance a full Basic Income will be higher than the current tax rate. Thus in comparing a Basic Income recipient and a W4L winner one should take into account these different tax regimes. How this influences the difference between the net income situation of W4L winners versus Basic Income recipients will depend on the level of the tax increase and the gross income one earns (see Figures 1 to 3). Assume however that the tax rate under the existing regime is 50 percent and that this has to be raised to 60 percent to finance the Basic Income. In that case Figure 6 compares the net income situation of someone with a gross income of 2500 euro over time. From this figure it becomes clear that the real difference between UBI and GMI/W4L will be bigger than one would expect on the basis of figure 4. The same holds for couples (compare Figures 5 and 7).

Figure 6. Evolution net income under UBI versus GMI/W4L (single situation)

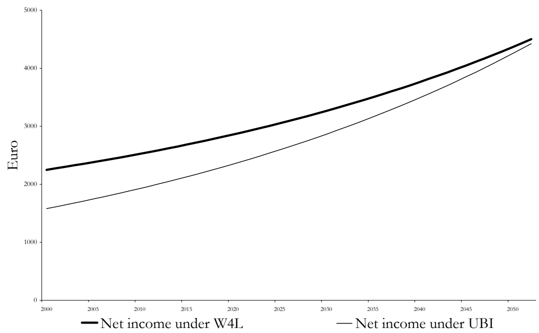
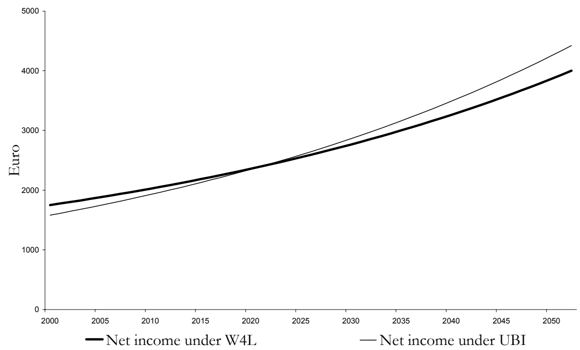


Figure 7. Evolution net income under UBI versus GMI/W4L (one partner in a couple situation)



In order to illustrate the figures and hypothesise how W4L compares to UBI regarding possible behavioural consequences, a hypothetical example is used. In the next section we will look at the case of a single who wins W4L. Afterwards, the couple situation will be discussed (cf. 2.1.3.)

2.1.2. Extreme, not absurd: Carla wins W4L

Consider Carla. She works fulltime at a university and earns a gross income of 2.500 euro per month. She pays a 50% tax and hence receives a net income of 1.250 euro a month. Every once in a while Carla buys a lottery ticket on her way home. She is lucky and wins W4L. A 1.000 untaxed euro extra for the rest of her life! She now earns 2.250 euro per month (an 80% increase in income). What will Carla do? With regard to her position on the labour market she has three options, she can decide to start-up her own business, she can stop working or she can decide to work less. We consider the three options one by one.

Suppose Carla has always dreamt of opening her own boutique. She has always been willing to use some of her savings for this purpose but as a shop needs a few years to become profitable and the first few years are very costly, she has never taken the risk. After winning W4L prospects look very different. Even if the shop is not successful in the beginning and hence cannot make enough profit to live off, she always has her unconditional monthly W4L grant as a security. For Carla, W4L makes her dream come true.

Will Carla have started her boutique under UBI? Maybe she would, but not necessarily. It could be that the 580 euro is a sufficient incentive for Carla to start her shop. However it could also be that after she has made all the calculations she decides that the minimum she needs is more than 580 euro. What seems to be clear then is that if Carla does not decide to open her boutique under GMI/W4L, she will most probably not do so under UBI.

Furthermore, if Carla is planning to start her boutique, we should be able to observe this in a relatively short time period. Remember that the W4L grant is not inflation related. As a consequence, people will still receive a 1.000 euro in 30 years time. The real value of W4L however has by that time decreased significantly (see Figure 4). Hence, it is best to start a business in a relatively short time period after winning W4L because the grant guarantees the highest standard of living close to the winning date. As time goes by opportunity costs start to change. In the future, the possible opportunity cost of starting up a boutique will be higher because the real value of W4L decreases over time.

The above example makes clear that W4L is an extreme but not absurd case. It is extreme because the granted amount clearly exceeds a full Basic Income (1000 euro as compared to 580 euro). As a result, people's incentives to change their behaviour are bigger under GMI/W4L than under UBI. Therefore, if people do not change their behaviour under GMI/W4L they will most probably not do so under UBI. However, the unconditional income provided by W4L is not absurdly high. Not everyone is willing to substitute a job for the risk of a possible successful boutique. Remember, Carla earned 1.250 euro before winning W4L. Starting-up a shop implies she will loose 250 euro a month during the first few years. Hence, in this case W4L research can inform us on two issues. First of all, if people do not start up a business under GMI/W4L they will most probably not do so under UBI (extreme case). Secondly, if they do start up a business, one cannot conclude that they will do so under UBI because of the difference between GMI/W4L and UBI (see Figure 2 and 4). However, the information that they will start-up a business indicates whether people are willing to start-up a business given sufficient – not absurd - financial incentives to do so. In order words, it can inform us on the presence of preferences of starting-up a business. Under UBI not everybody who switches from a job to self-employment due to W4L will make the same switch. This will depend on the individual elasticity to do so. However, some of them will.

Consider Carla's second option: stop working. Suppose in this case that Carla just works at university out of necessity. Her big passion is surfing and she wants to substitute everything to maximize the possibility to surf. Will she continue to work at university after W4L? After all, W4L provides her with enough income to stay alive and keep on surfing (surfing is not such an expensive sport). Again W4L is an extreme, but not absurd case. It is extreme because the W4L grant exceeds a full Basic Income by a significant amount. If one does not stop working under GMI/W4L one will most probably not do so under UBI. However, the case is not absurd as most of us will consider it impossible to live a comfortable life with just a 1.000 non-indexed euro. By contrast, a similar observation as in the case of self-employment holds for quitting work. If people stop working after winning W4L we are not able to conclude that they will do so under UBI because of the differences in amount. However, it gives us an indication of the presences to stop working.[22]

Finally, suppose that Carla is not such an enterprising person nor the 'lazy' type we supposed she was in the previous paragraph. Instead, Carla enjoys working at university. But she has always found it very difficult and stressful to combine her fulltime job with her extensive circle of friends and her love for playing the piano. What will she do after winning W4L? Carla, rationally as she is, starts to make calculations. If she would work less, she would obviously earn less. Recalculating her income under the assumption of a part time job of four days a week she ends up with the following sum: 2.000 (income 4/5) - 1000 (tax rate of 50%) = 1000 euro + 1.000 (W4L grant) = 2.000 euro per month. With foregoing 12.5 percent of her income she buys a day off per week and still earns 750 euro more than before W4L. Due to the lottery game Carla faces very strong incentives to reduce work. Even more pronounced than in the 'boutique' and 'stop working' examples, we can say that if Carla does not reduce working time under GMI/W4L she will most probably not do so under UBI. The possible observation of reduced working time under W4L might indicate a preference to do so, given sufficient financial incentives.

To conclude, if people with a high annual additional tax-free W4L income do not withdraw from the labour market, reduce working time by a significant amount or start up a business, the probability is (very) low that this will happen under UBI. Some of the pragmatic criticism against the introduction of Basic Income resolves around this specific issue since some opponents argue that the introduction of a Basic Income will provide significant disincentives to work and hence create labour market problems. Investigating these claims via an extreme but not absurd case is a valid research strategy which could verify or falsify this claim.[23]

What if Carla does not have a job when she buys her winning W4L ticket? Proponents of a Basic Income point out that a Basic Income abolishes the 'unemployment trap'. They argue that under a Guaranteed Minimum Income (GMI) unemployed people are not encouraged to return to work due to the low

[22] However, in this case the preference to stop working does not necessarily imply a lifelong preference for not working. W4L can provide a strong incentive to maximize surfing over working for a certain amount of time since it is now financially possible. However, this does not necessarily imply that Carla will surf for the rest of her life. After a few years surfing she may return to the labour market. Hence, there might be different behavioural changes as time proceeds (see 2.2.).

[23] In the above examples it is assumed that Carla earns more than G (see Figures 1 and 2). As becomes clear from looking at the Figures, the situation is different in case Carla earns less than G before winning W4L. However, since this is very exceptional and we do not have these cases in our dataset we will not go into the matter any further.

marginal difference in disposable income between accepting a job and staying unemployed. A Basic Income in contrast is given unconditional and would therefore abolish the unemployment trap, because going to work will always result in a significantly higher disposable income. What can W4L research suggest with regard to the unemployment situation? Are people really trapped or do they just not want to work?

In order to explore this issue a distinction has to be made between unemployed people who receive a social assistance benefit (someone who earns less than G in Figure 2) and those who receive an unemployment benefit (not represented in Figure 2).

Suppose Carla receives a social assistance benefit. In this case her social assistance income, which is means-tested, will be replaced by an unconditional W4L grant, which is substantially higher. If she decides to start working under these conditions she most probably will do so under UBI because she is obviously willing to work, but was prevented to work because of the unemployment trap. This willingness to work signal is strengthened by the fact that a substitution of a guaranteed minimum income by a W4L grant constitutes a significant increase in disposable income which might provide a disincentive to work and enjoy the 'better' life. Hence, going to work clearly signals a preference to work.

This is even more so the case when she receives an unemployment benefit. In this case she can combine her unemployment benefit with a W4L grant which combined generates a significant disposable income. If she decides to start working, at least part-time, in this case she will most probably do so under UBI.

2.1.3. Carla and John

Imagine Carla is married to John. Carla has met John at university and both have the same job. Now Carla wins W4L. What will they do? Carla and John could decide that Carla (or John) gets all the money and can do whatever he/she wants with it. In this case we are back to the extreme but not absurd Carla case. However, they could also decide to share the money equally between them. Now they have several options of which the three most important are discussed: they can start-up their own business, they can quit working or they can both reduce working time.

With regard to starting up a business and quitting work the couple example most probably constitutes a baseline scenario for UBI since it is slightly below or just at the subsistence level (compare Figures 1 and 3) and the gap with the subsistence level increasingly widens (see Figures 5 and 7). Under GMI/W4L they have an unconditional income of 1.000 euro (not price related) while under UBI in Belgian standards this will be 1.160 euro (price related).With regard to the two mentioned options different conclusions might be drawn from the research-findings. In a nutshell, the following observations of GMI/W4L might generate insights into what might happen under UBI. If Carla and John decide to stop working and start up their boutique under GMI/W4L they will most probably do so under UBI. The same holds for quitting and surfing. If they decide to quit working after one of the two has won W4L, they will probably do so when they would receive a Basic Income.

Finally, what about reducing working time? In this case GMI/W4L still seems to be a case which would provide couples with more net income than under UBI. In a way, it is also an extreme case, but less to than the Carla case. This is illustrated in the following brief example. Both Carla and John work at university. Together they earn a gross income of 5.000 euro. Both have to pay 50% taxes and hence together they receive a net income from work of 2.500 euro. After W4L they now have a 3.500 euro a month, or 1.750 euro each. What if they both decide to work four days a week? Together they will earn a net income of 3.000 euro (2.000 from work and 1.000 W4L). What would they have left under UBI? Working five days a week, and again assuming a 60% tax rate, they would have a net income from work of 2.000 euro. Add to this a Basic Income of 580 euro each and together they will have an income of 3.160 euro. If they both decide to work four days a week, they would still have their Basic Income of 1.160 euro, and a net income from work of 1.600 euro. Together this is 2.760 euro. In this option, in contrast to the two other options, UBI provides less net income than GMI/W4L. Hence, GMI/W4L does not provide a perfect match for UBI. In addition, it should be noted that the case becomes more extreme as the earned income of the couple increases. However, it should also be noted that since W4L is not inflation related this difference might disappear over the years.[24]

2.2. DESIGN OF THE PILOT PROJECT

Having analyzed how GMI/W4L compares with UBI, the next section focuses on the empirical results. Before that, however, in this section the design of the pilot project is discussed.

Since anonymity is secured by the Belgian National Lottery it was not possible to contact the Lottery winners directly to ask them for co-operation.[25] This introduced an extra hurdle not to co-operate since people can stay anonymous.

Given this difficulty, it was decided to conduct the research via a two-step approach. In a first step all winners receive a short mail questionnaire which aimed to identify some major socio-economic effects of winning W4L and asked them to co-operate with an extended face-to-face interview. In a second step, all winners who are willing to co-operate further with the research-project are contacted for a more

[24] The Carla and Carla and John cases should be considered 'ideal' cases. In reality it is of course possible that one partner of a household (for instance the winner) feels free to use the greatest part of grant but not all of it. In this case neither the Carla nor the Carla and John case applies. These cases will therefore be in between the Carla and Carla and John case.

[25] It should be noted that this problem is only confined to a number of countries. In other countries the names of lottery winners are publicly known.

elaborated structured interview on the effect of winning W4L. This interview aims to gain a fuller picture of changes in life patterns and to bind the respondents to the research team in order to set up a more longitudinal design.[26]

The design of the mail questionnaire had to discount two limitations, namely the questionnaire should not be too long and should contain questions on the effects of winning not only for the winner but also for the partner.

Although the relationship between the length of a questionnaire and a response rate is also influenced by factors such as the respondents' interest in the research topic and the presence of incentives (such as money) there exists an inverse relationship between the length of the questionnaire and the response rate (Bogen, 2004; Smith, Olah, Hansen & Cumbo, 2003). The longer a questionnaire the less the response. A rule of thumb is that a questionnaire should not be longer than 20 minutes. After the 20-minute threshold response rates begin to decline. It is a specific objective of the pilot project to generate a maximum response in order to start with the construction of a panel research population which can be followed through time.

Since households can bargain on the use of time and money it was decided for most part to investigate both the winner of the lottery as well as the spouse. Existing lottery research mainly focuses on the winner. This, however, might bias assessments since decisions are often made on the level of the household. For example, it is possible that the winner of W4L does not reduce working-time but that his partner does.

The interaction between a limit on the length of the survey and the desirability to extend the survey to the winner's spouse clearly limited the number of questions included in the mail questionnaire. Considering these limitations it was decided to draft a mail questionnaire which could be filled in less than 15 minutes and which contained information for the household on several topics. The questionnaire was structured using mostly closed answer categories.[27] At the end a general open question invited respondents to share any information they considered relevant in the context of the research-project.

The topics discussed in the questionnaire were:

- Labour market position of winner and partner, before and after W4L. This includes questions on type of job, number of working days per week, average number of hours worked. In case of a job change or possible job change in the future, the respondents are asked for their motives.
- Entrepreneurship of winner and partner, before and after W4L.

[26] This paper only addresses the results of the survey. The qualitative interviews are not discussed.
[27] The questionnaires are available on request in Dutch or French

- Active participation in associations of winner and partner, before and after W4L.
- Volunteering of winner and partner, before and after W4L. This includes questions on the type of volunteering and the hours per week or month spent on voluntary work. In case of a change in voluntary behaviour, the respondents are asked for their motives.
- Background topics as age, education and lottery behaviour.

The questionnaires were sent to 189 W4L winners. Of these, 82 responded, 19 surveys returned due to changes in the address of the winners. As a result, 48% of the winners who received the questionnaire participated with the survey. In general this constitutes a high response-rate for mail surveys.

2.3. RESULTS

In this section the results of the survey are presented, in light of the comparisons developed before (cf. 2.2.). The cases will be presented in a descriptive way, making use of several tables to summarize the results. Afterwards, using Qualitative Comparative Analyses (QCA), a sub-sample will more formally be analyzed in order to formulate some hypotheses. First, the representativeness of the sample is discussed.

2.3.1. Representativeness of the sample

A crucial question for any research which aims to make inferences concerns representativeness. To what degree is the W4L sample representative for the wider Belgian population? It should be noted that given the small sample (only 82 cases) it is impossible to claim representativeness for the total Belgian population. However, one can assess if there are obvious misrepresentations which enable researchers to put the results into context.

In order to assess the representativeness of the sample a comparison between the respondents (cases) and the general population (population) could be made on four meaningful background characteristics: age, sex, household type and education (see Tables 1 to 4).[28] First, in relation to age (cf. Table 1) it can be observed that the 25-49 year old are overrepresented in comparison to the general population, while the younger and older categories are underrepresented. Second, regarding sex both the research-sample and population of the winners corresponds with the general population (cf. Table 2). Thirdly, in relation to education (cf. Table 3) it can be observed that people with a higher education outside university are more represented among the cases, while people holding no degree or only a primary education degree are underrepresented. Finally, couples with two or more children are clearly over represented in the W4L

[28] On the crucial variables, such as inclination to change labour-market behaviour, start-up a business, volunteering, etc. no assessment can be made since no indicators are available for the population.

sample (see Table 4). Singles and couples with no children are underrepresented. This might indicate that people with a family play the lottery to overcome financial difficulties.[29]

As a result, some categories are slightly over represented while others are underrepresented. It should however be stressed that W4L winners do not constitute a closed homogenous group on age, education and household type. All categories are represented under the W4L-winners. Since many people play the lottery this could be expected. In this way the group is not very distinct from the general population and constitutes an interesting sample to explore the consequences of introducing a Basic Income.

However, this seemingly 'representative' sample could be the result of a double bias. First the initial W4L-sample (winners) can biased vis-à-vis the total population. Secondly, the response might be biased vis-à-vis the winners. The latter is referred to as non-response bias which means that only people who changed or intend to change their working behaviour participated in the survey because of the research topic. The double bias occurs when the sample (winners) is unrepresentative of the total population (bias 1) but due to the answering behaviour of the respondents (bias 2) the final set of cases becomes seemingly representative again.

These two biases might be significantly related to changes in labour market behaviour (the outcome to be analyzed) and hence it is important to detect a possible double bias. In order to assess the double bias a comparison was made on two variables for which data of the winners is available: age and sex (see Tables 1 and 2) On the basis of this comparison there is no indication that a double bias occurred.[30]

[29] In the sample and population statistics different definitions are used in order to determine whether one is single or part of a couple. The population data are gathered by the Belgian National Institute of Statistics (NIS) who defines a single as someone who is married. A couple is defined as two partners who are married. In the sample, however, marital status is not questioned. Therefore, someone is considered as part of a couple if that person is in a steady partner relationship and someone is defined single if he/she is not part of such a relationship. Considerations of marital status are in this definition of no importance. This difference in definition might explain why the singles with children seem to be underrepresented in our sample as compared to the population data of the NIS.

[30] A non-response analysis could not be conducted because the researchers were not able to gather additional information by phone since the survey was anonymous.

Table 1. Comparison of cases, winners and total population on age.

Age	Cases		Winners		Population	
	#	%	#	%	#	%
18-24	6	7.5	13	7.1	628.741*	8.0
25-49	45	56.9	102	53.9	3.761.322	48.1
+50	28	35.4	74	39.1	3.429.057	43.8
Total	79	100	189	100	7.819.120	100

* 20-24 years. Source: (Nationaal Instituut voor de Statistiek (NIS), 2001)

Table 2. Comparison of cases, winners and total population on sex.

Sex	Cases		Winners		Population	
	#	%	#	%	#	%
Man	36	47	93	47	4.082.724	48.4
Women	40	53	105	53	4.351.575	51.6
Total	76	100	198	100	8.434.299	100

Source: (Nationaal Instituut voor de Statistiek (NIS), 2001)

Table 3. Comparison of cases, winners and total population on educational level.

Educational Level	Cases						Population	
	Singles		Couples					
			Winner		Partner			
	#	%	#	%	#	%	#	%
Lower	3	17.6	10	16.6	7	11.8	2.215363	26.2
Secondary	9	52.9	34	56.6	35	59.3	4.484.204	53.1
Higher	4	23.5	14	23.3	15	25.4	1.173.622	13.9
University	1	5.8	2	3.3	2	3.3	561.000	6.6
Total	17	100	60	100	59	100	8.434.299	100

Source: (Nationaal Instituut voor de Statistiek (NIS), 2001)

Table 4. Comparison of cases, winners and total population on household type.

Type of Household	Cases		Population	
	#	%	#	%
Singles	15	18.7	1.412.786	34.1
Singles + 1 or more children	3	3.7	533.849	12.8
Couples	12	15.0	967.670	23.3
Couples + 1 child	13	16.2	497.361	12.0
Couples + 2 children	21	26.2	488.291	11.7
Couples + 3 or more children	16	20.0	238.442	5.7
Total	80	100	4.138.399	100

Source: (Nationaal Instituut voor de Statistiek (NIS), 2001)

2.3.2. Changes in working behaviour: a qualitative descriptive analysis

This section describes the changes in working behaviour (stop or start working, reallocate working time and change in entrepreneurship) occurring after winning W4L. To systematise the description of the changes, first, Table 5 summarizes the changing behaviour. Secondly, the occurrence or absence of changes is described in relation to the background characteristics occupation, hours worked, sex, number of children, year of winning, secrecy regarding the winning of W4L and civic engagement (cf. Tables 6 and 7). The variable occupation is reclassified from the original data using the International Standard Classification of Occupations (see Appendix).

A distinction is made between singles and couples because both clearly constitute separate units of analysis (cf. 3.2.). In addition, changes of winners and partners within a couple are distinguished because in some couples there might be a difference in the legitimacy of spending the W4L grant between winner and partner. Furthermore, in the tables a distinction is made between effectively changed behaviour and the intention to change behaviour. The latter is represented by numbers between brackets.

This section is structured as follows. First, working behavioural changes will be limited to stop or start working or the reallocation of working time. After that, changes regarding starting up a business are looked at.

From Tables 5 and 6 it becomes clear that most *singles* work both before and after W4L and that most job reclassifications are represented. The Carla case made clear that GMI/W4L constitutes an extreme situation. Therefore, the probability is very low that those singles would change working behaviour under UBI. Only one single changed his working behaviour after W4L. This person was a 48 year old blue collar worker who quit working after winning W4L. However, as discussed before, this does not mean that this person would have quit working under UBI.[31]

Table 5. Changes in working behaviour after W4L, singles and couples (winner and partner).

Working before W4L	No change or change in working behaviour (quit/start working, reallocate working time)					
	Singles		Couples			
			Winner		Partner	
	No Change	Change	No Change	Change	No Change	Change
Yes	12(10)	1(3)	38(40)	7(9)	41(39)	4(6)
No	2	1	19	2	13(12)	1(2)

[31] Three singles were not working before W4L. These were all student at the time of winning. Because they do not constitute interesting cases for Basic Income research, they will not be discussed any further.

Apart from those changing their working behaviour, Tables 5 and 6 also present those who have the intention to change working behaviour. Two persons intend to do so. Both are female service workers who point out that they want to work less in the future.

As with the singles, most *winners* (i. e. those winning W4L and having a steady partner relationship) did not change their working behaviour. Only 7 out of the 45 cases who were working before W4L and 2 out of 21 who were not working changed their behaviour (cf. Table 5). This is of great importance for Basic Income research

Table 6 makes clear that those working before W4L did so in very diverse sectors. Also those seven cases changing their working behaviour were working in diverse occupations. Furthermore, the changes are diverse: five quit working (three went on (early) retirement, one quit working because of illness, one took career interruption), two winners decided to work less. Of these two, one decided work 27 in stead of 38 hours a week, the other 24 hours in stead of 32.

Those not working before W4L were mainly pensioner or unemployed/pensioner (those unemployed aged between 55 and 65, in the current Belgian context it is debatable whether these are unemployed or on pension) (cf. Table 7). More interesting for Basic Income research are the four winners who were unemployed before W4L. Of these, one started working after W4L. The three remaining winners that stayed unemployed after W4L were all women with non adult children, two of whom explicitly relate their staying at home to having time to raise children. Going back to the Carla and John case, it can be stated that it is very likely that the person now working, but previously unemployed would also have started working under UBI. In contrast, regarding the three cases unemployed before and after W4L, it is possible but far from certain whether these would have remained unemployed under UBI.

Two sales workers have the intention to work less in the future. At this moment, both work more than 40 hours a week (cf. Table 7).

Table 5 also summarizes working behaviour of the *partners*. Only 4 out of 45 working before W4L change their working behaviour. Of these, one went on pension. Regarding the other three, one truck driver, working 75 hours a week before W4L, works 60 hours after W4L. The other two changed their working behaviour to take care of their children. Fourteen partners were not working before W4L. Again the great majority are pensioner, unemployed/pensioner or student and hence not very interesting to Basic Income research. One is unemployed both before and after W4L. *T*wo partners, working at this moment, have the intention to work less in the future.

These few cases notwithstanding, again the main conclusion is that most cases do not change their working behaviour. The interpretation is identical to the one given in the winner's situation.

Table 6. Changes in working behaviour after W4L by background characteristics, cases working before W4L, singles and couples (winners and partners).

	Single		Couple			
			Winner		Partner	
	No Change	Change	No Change	Change	No Change	Change
Occupation (reclassified)						
Teacher	0	0	2	0	3	1
Social worker	1	0	1	2	5(4)	0(1)
Civil servant	2	0	5	1	3	0
Clerical and related worker	3	0	10	0	11	0
Sales worker	0	0	4(2)	0(2)	3	1
Service worker	2(0)	0(2)	6	4	3(2)	1(2)
Blue collar worker	4	1	4	0	10	1
Self-employed	1	0	3	1	2	0
Hours worked						
< 40	7(6)	1(2)	24	7	29(27)	2(4)
≥ 40	1	0	11(9)	1(3)	10	1
Sex						
Woman	7(5)	0(2)	14	6	14(13)	2(3)
Man	5	1	19(17)	2(4)	24	2
Year of winning						
1999-2000	7(6)	1(2)	9(8)	5(6)	13(12)	1(2)
2001-2002	5(4)	0(1)	12	2	11(10)	3(4)
2003-2004	3	0	12	0	13	0
Number of children						
No children	8	0	3	1	6	0
1	1	0	9(8)	1(2)	11	0
2	1	0	15(14)	2(3)	12(11)	2(3)
≥ 3	1(0)	1(2)	8	4	12(11)	2(3)
Level of education						
Lower education	1	0	4(3)	0(1)	2	0
Secondary education	8(6)	0(2)	20(19)	4(5)	23	3
Higher education	3	0	13	3	14(12)	1(3)
Secrecy						
No one	1	0	9	4	20	1
Close relatives or friends	8(6)	0(2)	15	1	17(16)	1(2)
No secrecy	3	0	11(9)	3(5)	2(1)	1(2)
Civic engagement						
No	11	1	22	5	26(24)	2(4)
≥ 1	2	0	14(12)	3(5)	14	2

Table 7. Changes in working behaviour by background characteristics, cases not working before W4L, singles and couples (winners and partners).

	Single		Couple			
			Winner		Partner	
	No change	Change	No change	Change	No change	Change
Occupation (reclassified)						
Student	2	1	0	1	1(0)	1(2)
Unemployed	0	0	3	1	1	0
Unemployed / pensioner	0	0	5	0	3	0
Pensioner	0	0	9	0	9	0
Sex						
Woman	0	1	11	0	9(8)	1(2)
Man	2	0	5	1	4	0
Year of winning						
1999-2000	0	0	4	1	3	1
2001-2002	2	1	6	0	5	0
2003-2004	0	0	6	0	5(4)	0(1)
Number of children						
No children	1	0	4	0	2(1)	1(2)
1	0	0	2	1	5	0
2	0	0	4	0	3	0
≥ 3	1	0	7	0	4	0
Level of education						
Lower education	0	0	7	0	3	0
Secondary education	1	0	8	1	7	0
Higher education	1	1	2	0	3(2)	1(2)
Secrecy						
No one	0	0	5	0	6	0
No secrecy	2	1	4	1	2(1)	1(2)
Civic engagement						
No	1	1	11	1	9(8)	0(1)
≥ 1	0	0	7	0	4	0

So far changes in working behaviour regarding entrepreneurship were not presented. This is done in Tables 8 and 9, indicating whether the respondents (singles and couples) had started an enterprise before they won W4L and whether they have started a new enterprise after W4L.[32] First, the singles are discussed, then the couples.

[32] Regarding this item winners and partners were not questioned separately.

Table 8. Entrepreneurship, before and after W4L, singles.

Before		After: No	After: Yes
	No	15(14)	0(1)
	Yes	1	0

As becomes clear from Table 8, no actual changes in entrepreneurship occur in the single's sample. One single, however, reports to have the intention to start his own consultant business. Considering the fact that GMI/W4L gives extreme incentives, a conclusion seems to be that for these singles no mayor changes in entrepreneurship would follow the introduction of a Basic Income.

Table 9. Entrepreneurship, before and after W4L, couples.

Before		After: No	After: Yes
	No	47(45)	1(3)
	Yes	13	0

Of the sixty couples in the sample, 13 couples (or either winner or partner) have their own business, while 47 were not entrepreneurs at the time of winning W4L or before. Of these 47 one has started his own business, i. e. an insurance agency. Two more couples have the intention to be self-employed in the future. One couple in their early thirties thinks about taking over their parents business or starting a shop of children's clothes. One 25 year old salesman of mobile phones has the intention to become an estate agent. Again the conclusion that no mayor changes in entrepreneurship (at least within our sample) would occur under UBI seems warranted.

It could be objected that the extra monthly income could be invested in a business of a friend or relative and that therefore introducing a Basic Income would result in more changes than predicted on the basis of the above analysis. While this is indeed a probability, no actual evidence of such decisions was found in our sample. No one (either single or couple) has ever invested in the business of a friend or relative.

2.3.3. Qualitative Comparative Analysis

2.3.3.1. Methodology

In the previous part a descriptive analysis of all the cases was presented. In this part, a further exploration of the data is done using Qualitative Comparative Analyse (Hereafter QCA) which was developed by Charles C. Ragin for the analysis of a medium-sized number of cases (Ragin, 1987; Ragin, 1994; Ragin, 2000; Ragin, 2003). QCA is a research technique which enables researchers to systematically compare differences and similarities of configurations of variables between a set of cases and enables researchers to inductively explore data and develop explanatory models. In this paper, QCA is mainly used as a tool to summarize and explore data (De Meur & Rihoux, 2002; Rihoux, 2004).

QCA is a case-oriented approach. This implies that each individual case is considered a complex entity which needs to be comprehended as such. Different parts of each case are understood in relation to one another and in terms of the total picture that they form together. The organizing idea in such research is that the parts of a case do constitute a coherent whole and hence cases are regarded as configurations of variables (Ragin, 1987; Ragin, 2000). The essence of case analysis is to understand the configuration of variables and how that configuration is linked to a certain outcome. As such, this approach resembles more qualitative-oriented case research than quantitative-oriented variable research and hence can easily complement a qualitative description of cases. In other words, instead of analyzing relationships between variables (standard quantitative variable-oriented approach) QCA compares cases by comparing configurations of explanatory conditions with the presence or absence of an outcome. In this way it is truly comparative, in the sense that it explores similarities and differences across cases by comparing configurations. The goal is to unravel the different conditions connected to different outcomes. In this way it is a comparative exploration and examination of empirical diversity. As a result, QCA allows for *multiple conjunctural causation* (Ragin, 1987; Ragin, 2000; Rihoux, 2004). This means that the technique allows for the possibility that there may be several combinations that generate the same general outcome, can address complex and seemingly contradictory patterns of causation - a condition can be important in both its presence and absence – and that it eliminates irrelevant causes.

In order to explore and summarize data using QCA several basic analytic steps are required. In brief, the following seven steps can be identified (Becker, 1998, pp. 187-188; Ragin, 1994, p. 118).

1. Decide what outcome needs to be investigated and list the variables which might contribute to an explanation of the outcome.
2. Define the research population and select the cases for analysis (comparability requirement).
3. Define each variable and outcome as a categorical variable.

4. Construct a data matrix which is a table whose rows and columns provide cells for all the combinations of those variables.
5. Reformat the data matrix as a truth table that lists all possible logical combinations of the presence or absence of these attributes
6. Simplify the truth table. The simplification strategy follows the logic of an experiment. Only one condition at a time is allowed to vary (the "experimental" condition). If varying this condition has no discernible impact on the outcome, it can be eliminated as a factor. The rule of combining rows of the truth table as a way of simplifying them can be stated formally: If two rows of a truth table differ on only one causal condition yet result in the same outcome, then the causal condition that distinguishes the two rows can be considered irrelevant and can be removed to create a more concise combination of causal conditions.
7. Interpret the resulting equations

2.3.3.2 Model

The application of QCA to the W4L dataset aims at answering two main questions:

(1) Which conditions contribute to a change in labour market behaviour?
(2) Which conditions influence the decision not to change labour market behaviour?

Since the available information on the cases is limited it was decided to inductively explore answers to both questions. As a result, the information available in the questionnaire was transformed into several variables. For the transformation of the variables a dichotomous crisp set approach was chosen because of the explorative nature of the exercise. In a crisp set approach all variables are transformed in dichotomous variables (absence or presence of the variable). This implies a loss of information and nuance. An advantage of this approach is that it allows for the creation of 'sharp' typologies. In other words, it creates a black/white picture of reality which allows for the formulation of clear hypotheses.[33]

The exploration of the data was done by using 10 variables on which information was available through the questionnaires. The variables are presented below. For ease of presentation the variables are in italic to make a clear distinction with lowercase and uppercase letters which both have a special meaning in QCA. QCA uses lowercase notations of variables to indicate the absence of a variable and uppercase notations to indicate the presence of a variable.

The outcome to be explained is change in labour-market behaviour

[33] It should be noted that a QCA analysis can also be conducted with multi-value scales and fuzzy-sets. For more information see (Conqvist, 2003; Ragin, 2000; Ragin & Giesel, 2002).

Change: Presence or absence of effective or intention to change labour market behaviour due to W4L.[34]

The outcome will be explained by testing several models consisting out of all or some of the following nine explanatory conditions.

Kids: Presence or absence of one or more children
Kids2: Presence or absence of two or more children
Kids3: Presence or absence of three or more children
Civic: Presence or absence of active membership (on average more than 2 hours a week) of one or more associations
Hoursres: Winner works more (presence) or less (absence) than 40 hours a week
Hourspart: Partner of the winner works more (presence) or less (absence) than 40 hours a week
Edures: Presence or absence of a university or higher education degree for the winner
Edupart: Presence of absence of a university or higher education degree for the partner of the winner
Ageres: Winner is older (presence) or younger (absence) than 50
Agepart: Partner of winner is older (presence) or younger (absence) than 50
Couples: Both the winner and the partner works (presence) or either the winner or the partner works, but not both (absence).

The analysis was performed on a subset of cases because not all cases were suitable for the analysis. Three types of cases were not selected for further analysis. The first type are the single persons since they constitute a distinct unit of analysis in this research-project (see 3.2.1.). No separate analysis of the singles was conducted due to an insufficient number of observations. The second type of cases which were not included in the analysis consists out of students and pensioners since they were not (yet) active on the labour market. Finally, cases with significant item non-response on several variables were deleted because they could not be meaningfully analysed. In the end the subset of cases suitable for analysis contained 40 cases.

2.3.3.3. Analysis: Problems and a Partial Solution - Two-step approach

Analysing 40 cases with a model of 10 possible variables in QCA generates two problems. First there is the *problem of uniqueness*. If one uses all possible 10 variables, chances are considerable that each case is unique. Each case is described as a distinct configuration of 10 variables. This results in the fact that there is no summarisation of data and one does not generate any typologies. In other words, one ends up with 40 descriptions of cases and few possibilities to generate hypotheses.

[34] It is important to emphasise that in the QCA-analysis the dependent variable – change on the labour market – does not only include people who have effectively changed their behaviour, but also those who indicated that they had the intention to change their behaviour in the future. In this way, the *Change* variable is distinctively different from the change variable in the descriptive part which only included effective changes in labour-market behaviour.

A second problem related to a QCA-analysis is the *problem of contradictions*. This problem mainly occurs when one uses only a few of the 10 variables. It should be noted that QCA was mainly developed to comparatively analyze macro social entities and processes such as state-formation, revolutions, etc. for which much historical data is available and which allows for a constant dialogue between theory and data. It is only recently that QCA has been applied to individual level data (Britt, Risinger, Miller, Mans, Krivchenia & Evans, 2000). The application to individual level data generates problems for explaining *each* case. In case of individual level data the chances that similar configurations produce different outcomes increases dramatically. Individual contingent, idiosyncratic or non-modeled factors might influence the outcome. As a result, a QCA-analysis on individual level data can generate what is called contradictory results, i.e. the same configuration generates different outcomes. In addition, QCA was developed to generate explanatory models via a constant dialogue of theory and data. This implies a regular return to data and the gathering of additional data. If a return to the field to acquire new data is not possible it can be difficult to resolve contradictions in the analysis. In the pilot-project it was not possible to return to the field and hence an analysis could only be done on the basis of data available in questionnaires.

Moreover, there seems to exist a trade-off between the two problems. The smaller the models, the more contradictions, the more extensive the models, the less possibility to summarise data and create typologies. The two problems and the trade-off are illustrated by Table 10. The more extensive the model is (# variables) the more configurations and the less contradictions occur. A limited model, with a few variables, generates less configurations but more contradictions.

How to solve these two interrelated problems? In relation to the problem of uniqueness the only solution is to develop limited explanatory models which implies that the number of variables of an explanatory model should significantly be lower than the number of cases. Concerning the problem of uniqueness there are several ways to deal with contradictions. First, a new more homogenous and comparable research population can be constructed by including new cases or removing cases. Secondly, new variables could be included in the explanatory model. Thirdly, existing variables could be recoded. Since, it was not possible to return to the research population to gather additional information none of these three options were available to resolve contradictions. A final way to deal with contradictions is to include only those configurations which contain at least two cases since it is often the case that contradictions are generated because only one contradictory case occurs. These contradictions are disregarded when one specifies that at least two or more cases should be covered by a given configuration. The drawback of this decision is that it decreases the number of cases in an analysis and hence excludes possible relevant configurations. This is especially problematic when one works with (biased) samples and the aim is to explore data and generate hypotheses. Concerning the latter it is best to exclude as few cases as possible and hence proceed with an analysis of all possible cases.

As a result, the problem of contradictions is not easily overcome. In order to deal with these two interrelated problems a two-step approach was used for analysing an explanatory model. In a first step, (a) model(s) which best fits the data was identified. In a second step a further analysis of the selected model was performed.

The first step is time consuming and implies analysing the results of all possible and meaningful models. Table 10 lists a sample of tested models and presents the number of configurations which occur in the data on the basis of the model and the number of contradictions. If both numbers tend to go to 40 it means that either little reduction in data has occurred (each cases is unique) or that the model (almost) only generates contradictory results. Both are an indication that the model does not fit the data and provides little explanatory insight. For example, the first row presents an elaborate model with almost no contradictions (5%) but also very little simplification (34 configurations). Here one encounters the uniqueness problem. Each case is a unique combination of the presence and absence of each of the 10 variables. By contrast, the last row generates considerable simplification of the data but has a very high percentage of contradictions which means that the same explanatory model is inductively identified for both cases were labour-market change occurred and cases where this change did not occur.

The selection of the best fit model was based on balancing the number of contradictions and the number of configurations. The aim is to find a model with jointly the least configurations (reduction of data – creation of typologies) *and* the least contradictions. In other words, one does not select the model with the least configurations or the least number of contradictions, but the model which scores best on the two criteria combined. The model which best fits these two combined criteria is model 14 which was further analysed. As one can see in Table 10 this model consisting out of 6 variables generates 20 configurations and 8 contradictions. Model 14 is able to discriminate several types of cases in relation to making changes on the labour-market or not (problem of contradictions) and is able to capture some configurations which explain several cases at once (not each case is unique).

More formally, the model is selected on the basis of the following formula:

Best Fit Model (BFM) = MIN((#Contradictions + #Configurations)/2)

Table 10. Possible Models to Explain Working Behaviour

	Models	Variables	Configu-rations	Contra Dictions	BFM
	CHANGE =	#	#	#	
1	*Eduresp + Edupart + Civic + Kids + Hourresp + Hourspart + Couplesft + Ageres + Agepar*	10	34	2	18
2	*Eduresp + Edupart + Civic + Kids2 + Hourresp + Hourspart + Couplesft + Ageres + Agepar*	10	36	2	19
3	*Eduresp + Edupart + Civic + Kids3 + Hourresp + Hourspart + Couplesft + Ageres + Agepar*	10	35	0	17,5
4	*Kids + Eduresp + Edupart + Hourresp + Hourspart + Couplesft + Ageres + Agepar*	9	30	6	18
5	*Kids + Eduresp + Edupart + Civic + Hourresp + Hourspart + Ageres + Agepar*	9	34	2	18
6	*Eduresp + Edupart + Civic + Kids2 + Hourresp + Hourspart + Ageres + Agepar*	9	36	2	19
7	*Eduresp + Edupart + Civic + Kids3 + Hourresp + Hourspart + Ageres + Agepar*	9	35	0	17,5
8	*Kids + Civic + Hourresp + Hourspart + Couplesft + Ageres + Agepar*	8	23	12	17,5
9	*Kids + Eduresp + Edupart + Civic + Hourresp + Hourspart + Couplesft*	8	32	4	18
10	*Kids + Eduresp + Edupart + Hourresp + Hourspart + Ageres + Agepar*	8	33	4	18,5
11	*Eduresp + Edupart + Civic + Hourresp + Hourspart + Couplesft*	7	30	5	17,5
12	*Kids + Eduresp + Edupart + Hourresp + Hourspart + Couplesft*	7	26	14	20
13	*Kids + Eduresp + Edupart + Civic + Couplesft*	6	17	26	21,5
14	*Kids + Civic + Hourresp + Hourspart + Couplesft*	6	20	8	14
15	*Kids2 + Civic + Hourresp + Hourspart + Couplesft*	6	21	15	18
16	*Civic + Hourresp + Hourspart + Couplesft*	6	16	25	20,5
17	*Eduresp + Edupart + Civic + Couplesft*	5	15	27	21
18	*Kids + Eduresp + Edupart + Couplesft*	5	14	27	20,5
19	*Kids + Couplesft + Ageres + Agepar*	5	13	28	20,5
20	*Kids + Civic + Ageres + Agepar*	5	12	32	22
21	*Kids + Civic + Hourresp + Hourspart*	5	22	13	17,5
22	*Eduresp + Edupart + Civic + Couplesft*	5	15	27	21
23	*Hourresp + Hourspart + Couplesft*	4	11	31	21

The model with the lowest score is the best fit model for further exploring the data. This model states that changes and no changes in labour market behaviour is a function of the combined presence and absence of the following 5 variables: *Kids + Civic + Hourresp + Hourspart + Couplesft.*

In a second step the best fit model was used to conduct a QCA-analysis showing a description of the cases as a configuration of the presence and/or absence of each of the variables of the model. All the configurations which exist in the data are presented in Table 11.

Table 11. Overview of all configurations of the Best Fit Model

		# of cases	
		Change	**No Change**
1	KIDS civic hourresp hourspart COUPLES	0	10
2	KIDS civic HOURRESP hourspart COUPLES	4	1
3	KIDS CIVIC hourresp hourspart COUPLES	3	0
4	KIDS civic hourresp HOURSPART COUPLES	2	1
5	Kids CIVIC hourresp hourspart COUPLES	0	2
6	KIDS CIVIC hourresp HOURSPART COUPLES	0	2
7	KIDS civic HOURRESP HOURSPART COUPLES	0	2
8	Kids civic hourresp hourspart COUPLES	1	0
9	KIDS civic HOURSPART couples	1	0
10	Kids CIVIC Hourresp	1	0
11	KIDS CIVIC hourspart couples	1	0
12	Kids CIVIC HOURRESP hourspart COUPLES	1	0
13	Kids civic hourresp hourspart COUPLES	0	1
14	KIDS CIVIC HOURRESP couples	0	1
15	KIDS civic hourspart couples	0	1
16	KIDS civic Hourresp	0	1
17	KIDS civic hourresp HOURSPART couples	0	1
18	Kids CIVIC hourresp HOURSPART COUPLES	0	1
19	KIDS CIVIC HOURSPART couples	0	1
20	KIDS CIVIC HOURRESP HOURSPART COUPLES	0	1
	Total	**14**	**26**

Table 11 lists all the configurations which exist for the best-fit model. Uppercase notations indicate the presence of a variable, while lowercase notations indicate the absence of a variable. Most cases (67,5%)

are covered by the 7 first rows. The most important configurations in relation to change are row 2 and 3. The configuration of row 2 states that couples who are inclined to change can be described by the following characteristics: they both work, one of the partners is working more than average and they have kids. The configuration of row 3 indicates that couples who combine a busy personal life (kids and civic engagement) with the fact that the two partners are working are also more inclined to change. In other words, two paths lead to a change in labour-market behaviour. In contrast, the configuration which captures most non changes is row 1. This configuration indicates that couples with children where both partners work but none of the partners works more than average are less inclined to change. In addition, it should be noted that in several of these no change cases one of the partners is not working full-time but has a part-time job which makes a better combination between working life and family life possible. From row 8 onwards all cases are captured by a unique configuration of explanatory conditions

More in general, on the basis of these configurations one could develop the hypothesis that couples where the two partners work, one of the partners works more than average and which either have kids or are actively involved in civic activities are more inclined to change their labour-market behaviour. The contradictory cases to this hypothesis (mainly rows 5 and 6) can be explained by the fact that in these cases the couples are older than 50, which might indicate that the children are adults and hence require less attention. This suggests that balancing working-time and family time influences labour-market behaviour and that the provision of an exogenous non-earned income might in certain cases and under certain conditions change labour market behaviour. Obviously more data is required to assess this hypothesis.

3. DISCUSSION AND FUTURE RESEARCH

This study had two aims. First, to explore the advantages and drawbacks of empirical research into the working behavioural consequences of introducing a Basic Income. Secondly, to investigate the possibilities and outcomes of a research-strategy which uses an annuity game – the Belgian Win for Life (W4L) – as a natural experiment. These two goals were addressed in two distinct parts.

In the first part it was argued that a strong case can be made for the use of empirical research into the consequences of introducing a Basic Income. An argument was made to use natural experiments such as annuity games to this end because some of the limitations related to a Basic Income experiment can in this way be overcome. Thus, natural experiments allow researchers to build in relevant variation in different types of Basic Income design and institutional context and to design longitudinal research which captures different types of social dynamics in relation to introducing an exogenous unearned income.

Since, however, introducing a Basic Income is not the same as granting everyone a winning lottery ticket, the second part assessed to what degree W4L constitutes a good case for analysing the labour market related behavioural consequences of introducing a Basic Income. In a nutshell, the argument was made that W4L constitutes an extreme, but not absurd case.

In relation to singles it was argued that if people with a monthly additional tax-free W4L income do not withdraw from the labour market, reduce working time by a significant amount or start up their own business, the probability is (very) low that this will happen under a Basic Income regime (UBI). If, in contrast, they do change their working behaviour it is not possible to conclude that they will do so under UBI, even though it indicates a preference to do so. For couples the conclusions concerning stop working and starting up a business are similar than under the singles case. Interpreting a possible reduction in working time is more difficult however since the difference between receiving a Basic Income and W4L is less pronounced and dependent on the tax regime.

Taking into consideration these comparisons between UBI and the conventional guaranteed minimum income scheme adjusted for W4L recipients (GMI/W4L), a pilot project was set up to investigate Belgian W4L winners. The results were analyzed in two ways. First, by describing the cases making use of standard tables. Secondly, by investigating a subgroup of W4L winners more analytically making use of Qualitative Comparative Analysis (QCA).

Regarding changes in entrepreneurship, the descriptive analysis gave very little evidence that exogenous unearned income would stimulate entrepreneurship. Only one person started up a business after winning W4L and only 3 indicated that they have the intention to do so. Given the strong financial incentives under GMI/W4L it is hypothesised that the introduction of a Basic Income will have little effect on entrepreneurship. Of course these are very tentative conclusions. As became clear from the first part of the paper, while empirical research has certain strengths, a natural experiment can never capture the entire complexity of behavioural changes that a the introduction of a Basic Income would entail. Furthermore, only a very small minority of changes in our sample could constitute a substantial number when looked at on population level or could have profound macro consequences.

In relation to stop working or diminishing working time, only a very small proportion of people effectively changed their labour-market behaviour after winning W4L. There is therefore no indication for an immanent emergence of a 'lazy society' after introducing a Basic Income. The idea that people would retreat from the labour-market and would live off a Basic Income is not supported by the evidence. Only a handful of respondents changed their labour market behaviour. Even if one would include the people who indicated that they have the intention to change their behaviour in the future a majority of people would stay working under the same conditions they are working now (in our sample 85 out of 103). Furthermore, it has to be stressed that the 'intention cases' support the argument that people want to work less, not that they will stop working.

Whether these people will effectively change working behaviour in the future is still an unresolved question. In addition, as argued before, it is very well possible that even those cases that have made actual changes will not change their behaviour under a Basic Income. The comparative analysis of GMI/W4L and UBI made clear that one should not conclude that people who (might) change their labour market behaviour under W4L would also do this under a UBI. However, one can explore which factors influence labour market behaviour changes under conditions of an exogenous non-earned income. This exploration was done using QCA.

The QCA-analysis focused on couples winning W4L. It showed that couples with both partners in a full-time demanding job (work more than average) and with (up-growing) children are more inclined to change labour-market behaviour. Hence, a Basic Income might result in a different balance between working and family life for couples with a heavy time burden. Consequently, there is some evidence that supports the idea that a Basic Income will influence how people balance family and working life.

This conclusion may have important implications for the design of an experiment. If changes in labour-market behaviour occur they are likely to occur in 'normal – standard' households, i.e. working families with children. This implies that an experiment should include these types of households if it aims to make inferences about what might happen after the introduction of a Basic Income.

Furthermore, since having children plays a part in labour market decisions and in balancing family life and working life, the question of how a Basic Income will influence demographics becomes important. The pilot-project generated some anecdotic evidence that W4L provides an incentive to expand the family.[35] More evidence was found regarding the reduction of uncertainty about the future after winning W4L. Without asking any specific questions about it, many people indicate that W4L provides much more security which enables them to expand their choices and decisions. In other words, from an empirical point of view an argument can be made that introducing a Basic Income allows people to better plan their future life due to uncertainty reduction. How this would affect demographics is still an open question but a more systematic investigation of uncertainty reduction and its ramifications should be explored.

These are, of course, very tentative conclusions which require further investigation. Three major limitations of the pilot-project need to be overcome to generate more robust results. First, the number of cases should be increased, both in terms of lowering non-response as well as expanding the research-project to new winners. Secondly, data-collection on each of the cases needs to be improved significantly. Thirdly, future research should establish a control group among W4L-players who have not won the game. This is of quintessential importance in the design of a natural experimental research design. However, the formation of a control group is not straightforward and implies a significant investment of resources. In addition, four important caveats of the pilot-project which need to be addressed in the near future have to be stressed.

First, the pilot-project only provides a partial assessment of changes in labour-market behaviour. For example, a return to the labour-market from unemployment after winning W4L was only assessed in a very limited way due to very small amount of cases. It can very well be argued that winning W4L abolishes the unemployment trap (cf. infra) and would make people make more inclined to return to the labour market. By contrast an argument can be made that W4L plus an unemployment benefit provides a huge incentive to stay unemployed if people really do not want to work. The movement out of unemployment could not be assessed in the pilot-project. Hence, which hypothesis can be supported is still an unresolved question. In addition, besides focusing on changes in labour-market behaviour, non-changes due to an unearned exogenous income might also be important and should be analysed.

Secondly, the pilot-project observed the situation only at one moment in time. It is crucial to get an insight into the dynamics of introducing an unearned exogenous income and how effects play out over time. Several people indicated that they have the intention to change labour-market behaviour, but they have not yet done so. However, there is of course an important difference between what people say and what they effectively will do. A longitudinal research design is necessary to gain insight into the dynamics of changes.

[35] One respondent indicated they planned a second child after winning W4L because they can now afford it.

Thirdly, labour-market behaviour is in the present study narrowly defined as paid work and changes in labour-market position only refer to changes in relation to paid work. This is a much too restricted definition of work in the context of the Basic Income debate which explicitly aims to reward unpaid forms of work. In future research, a thorough conceptualisation of the concept of work, including volunteering and family work, should be done and included in the analysis of changes in working behaviour.

Finally, there is an important issue of representativeness and selection-bias. These were discussed before. Several selection biases can occur on which no definitive assessments can be made at this point. More research is needed to generate more robust results.

It is our intention to overcome these limitations and caveats by expanding and improving annuity games research. This will imply, among other things, an improvement in data-gathering techniques. Mail surveys are limited data collection tools due to (item) non-response and low reliability of data. In order to better capture changes it is advisable to construct longitudinal datasets based on face to face interviews. Apart from the better reliability, these interviews would have two more advantages. First of all, they should enable researchers to deepen the investigation into labour market consequences by tracking employment records and creating more elaborate working profiles in terms of several characteristics which might be relevant such as earnings, autonomy of decision in occupation, job fulfillment, etc. Secondly, face-to-face interviews should allow researchers to fully explore other relevant intended and unintended social consequences of an exogenous unearned income such as effects on family planning and other demographic factors.

In addition, a major challenge for future research is to expand W4L-research into other countries to allow for institutional variation and to assess the behavioural impacts of different designs of a Basic Income. Especially interesting in this respect is a comparison with the United States where many similar annuity games exist in different forms for some years. Thus, it could be illuminating to compare Belgian W4L with the W4L game organized in the state of Arizona, granting each winner a lifelong 1000 dollars a month, or with the Weekly Bonus game, organized in the state of Delaware, in which 250 dollars a week are granted. Similarly, it would be interesting to compare the Belgian W4L winners with the Belgian Fun for Life winners, who receive 25000 euro a year.

Furthermore, natural experiments such as lotteries can be used to analyze the behavioural effects of another big idea which is closely related to a Basic Income, namely the idea of stakeholder grants. The idea of a stakeholder grant is to give 'each (American) [as he/she] reaches maturity, [a] guaranteed … stake of eighty thousand dollars. [This would] point the way to a society that is more democratic, more productive, and more free (Ackerman & Alstott, 1999, p. 3).'

The potential of research into social consequences of introducing a stakeholder grant is significant since the potential research population is huge. A research-design could include all people who won approximately 80.000 euro/dollars on the ages of 18 to 25. Since almost every country has lottery games which grant a one-time sum of approx. 80.000 euro, the possible research population is very significant. This is interesting for two reasons. First, the initial research population will be big which will reduce possible selection biases and problems of representativeness. Although a further assessment of how representative lottery players are for the wider population should be conducted, it should also be stressed that the lottery populations are probably not as skewed as one would expect on the basis of common sense notions. The pilot-project showed that many different types of people play the lottery. Secondly, since many countries can be compared, the effect of institutional variation can be further assessed and conceptualized.

In a latter stage lottery based research on the behavioural consequences of un-earned exogenous income paid either as a lump sum (stakeholder grant) or as a weekly, monthly or yearly endowment (Basic Income) could be compared. This comparison could contribute to recent debates on the possible different advantages and disadvantages of Basic Income versus stakeholder grants. It is clear that this may constitute a future research agenda for Basic Income and stakeholder grant researchers.

APPENDIX. RECLASSIFICATION OF OCCUPATIONS

In the questionnaire all respondents were asked for their occupation before W4L. Because of the great diversity of jobs however, it was necessary to aggregate these to a higher level of abstraction. This was done using the International Standard Classification of Occupations (ISCO68), developed by the International Labour Organization (ILO). In a first instance, the jobs were classified on the most detailed level. Thus, depending on the information given by the respondent, a code is allocated between 1 or 5 digits. The categories student, unemployed, unemployed/pensioner (unemployed and between 50 and 60 years old) and pensioner were added. Next, the attributed classifications were reclassified. This classification was mainly based on the ISCO68 codes, lowering the level of detail. As can be seen by comparing the 'ISCO code'-column with the 'job classification reclassified'-column, some adjustments were necessary for the purpose of this study.

Table 12. Reclassification of occupations (before winning W4L).

Job classification	ISCO code	Job classification reclassified
Sport co-ordinator	1.3	Teacher
Teacher	1.3	
Teacher	1.3	
Teacher Secondary Education	1.32.00	
Language Teacher Secondary Education	1.32.15	
Instructor company	1.39	
Nurse hospital	0.71.10	Social workers
Social worker	1.93	
Social worker	1.93	
Social worker	1.93	
Social worker	1.93	
White collar employee social sector	1.93	
Caretaker old people's home	1.93	
Therapist in a centre of psychological health	1.93.40	
Social worker delinquency	1.93.40	
Judge	1.22.10	Civil Servant
Civil servant	3.00	
Civil Servant European Commission	3.00	
Civil servant	3.00	
Civil servant	3.00	
Translator in government administration	3.00	
Civil servant	3.00	
Civil servant	3.00	
Civil servant	3.00	

Civil servant	3.00	
Civil servant	3.00	
Computer programmer	0.84.20	Clerical and related workers
Book editor governmental organization	1.59.20	
Self-employed librarian	1.91.20	
Budgeting and accounting manager telecom sector	2.19.50	
White collar employee bank and insurances	3.31	
White collar employee bank and insurances	3.31	
White collar employee bank and insurances	3.31	
Accountant in a company	3.31.10	
Book keeping clerk	3.39	
Book keeping clerk	3.39	
Book keeping clerk	3.39	
White collar employee metal processing company	3.9	
Railway clerk	3.99.60	
White collar employee private company	3.	
White collar employee private company	3.	
White collar employee private company	3.	
White collar employee private company	3.	
White collar employee private company	3.	
White collar employee pharmaceutical company	3.	
White collar employee private company	3.	
Private guard	5.82.40	
Private guard	5.82.40	
Private guard	5.82.40	
White collar employee fish company	7.7	
Salesman in a company	4.31	Sales workers
Representative catering industry	4.32.00	
Worker in a supermarket	4.51	
Shop-assistant dress shop	4.51.25	
Salesman Cheese shop	4.51.25	
Salesperson retail trade	4.51.25	
Salesperson mobile phones	4.51.25	
Street vendor food	4.52	
Postman	3.70.30	Service worker
Postman	3.70.30	
Cook	5.31.00	
Cook hospital	5.31.30	
Snack-bar manager	5.32	
Waitress	5.32.10	
Building care taker	5.51.00	

Janitor	5.51.25	
Janitor	5.51.25	
Janitor	5.51.25	
Janitor hospital	5.51.25	
Fire fighter	5.81.10	
Policeman	5.82.20	
Soldier	5.83.40	
Soldier	5.83.40	
Travel attendant	5.111	
Drilling, machine operator	7.11.30	Blue collar worker
Blue collar worker sawmill	7.32	manufacturing
Blue collar worker sawmill	7.32	
Petroleum refinement worker	7.45	
Blue collar worker in a meat company	7.7	
Blue collar worker textile company	7.9	
Blue collar worker metal processing	7.2	
Blue collar worker private company	7-8-9	
Blue collar worker private company	7-8-9	
Automobile mechanic	8.43.20	
Automobile mechanic	8.43.20	
Automobile mechanic	8.43.20	
Blue collar worker electronic assembly company	8.5	
Plumber and pipe fitter	8.71	
Tile setter private company	9.51.50	
Warehouse man	9.71.45	
Warehouse man	9.71.45	
Lorry and van driver	9.85.55	
Lorry and van driver	9.85.55	
Lorry and van driver	9.85.55	
Construction worker	9.95.00	
Self-employed		Self-employed
Self-employed hairdresser	5.70.25	
Self-employed hairdresser	5.70.25	
Bicycle repairer	8.41.90	
Self-employed in machine construction	8.4	
Self-employed in machine construction	8.4	
Free lance express service T.V. production	9.85.00	
Student		Student
Student		
Student		
Student		

Student		
Unemployed		Unemployed
Unemployed		
Unemployed		
Unemployed		
Unemployed		
Unemployed/Pensioner		Unemployed/Pensioner
Unemployed/ Pensioner		
Unemployed/ Pensioner		
Unemployed/Pensioner		
Unemployed/Pensioner		
Unemployed/Pensioner		
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What would we like to learn from a European Basic Income Experiment?

I very much enjoyed being part of the one-day work workshop on a basic income experiment in Europe. About 20 of us spent the day discussing how basic income could be tested in a European social experiment. Toward the end of the workshop, it occurred to me that if there is going to be an experiment, we should start with a list of what we would like to learn from an experiment. With awareness that we will actually be able to test for much less than we would like to know, I think that an extensive wish list is the best place to start. With that we can consider how we could test each of these questions and that will help to determine which we should actually focus on, and how best to set up the test. I started a list at the workshop, hoping to bring it up in the discussion at the end. But time ran short, and so I would like to circulate that list now, and ask everyone else who participated to contribute their thoughts.

Many of the items on this list refer to the equation relating before tax to after tax income. This equation is the same whether the program tests a basic income (BI) or a negative income tax (NIT). The equation is: after tax income (Y_d) equals the basic income grant (G) plus private income (Y) minus income times the marginal tax rate (t):

$$Y_d = G + Y - tY$$

This formula can be simplified to:

$$Y_d = G + Y (1 - t)$$

I begin with a list of things that we might be able to learn, and then list some things that we would like to learn, but that cannot be learned, from an experiment.

<u>Labor Supply effects</u>: These were centrally important to the U.S. experiments in the 1970s, but they were not usually looked at as broadly as they should have been.

1. **What is the effect of various sizes of grant level (G) on labor supply?**
2. **What is the effect of various sized of marginal tax rate (t) on labor supply?**
3. **What is the efficiency loss created by basic income?** This is usually the “bottom line” question in any economic study, but it was largely ignored in the NIT experiments. Related to this question are two others (4 and 5)
4. **Does an equal amount of money spent on poverty reduction through basic income create a larger or smaller efficiency loss than spending on poverty reduction through the current, conditional welfare system?** Experiments cannot give a complete estimate to this question without being about to test for demand effects.
5. **Does an equal amount of poverty reduction through basic income have a larger or smaller efficiency loss than an equal amount of poverty reduction attained by increasing the current, conditional welfare system?** Experiments cannot give a complete estimate to this question without being able to test for demand effects, but the fact that it is a comparison to another system makes the interpretation a little easier. If any question is the “bottom line,” this is probably

the best candidate. It amounts to whether BI is a more effective and efficient way to fight poverty than the current conditional system. That is not the only reason one might choose one over the other, but it is an important reason.

6. **Would the amount of money spent on the current welfare state have a greater or lesser impact on poverty if it was spent as a negative income tax instead (or as the net cost of a BI)?** Experiments cannot give a complete estimate to this question without being about to test for demand effects.
7. **What is the overall effect of basic income (G and t) on the labor supply of recipients?** This was the "bottom line" question in the U.S. NIT experiments, but it is not necessarily the most important question the experiments can answer. It is, in fact, only a proxy for the relevant question of what is the equilibrium effect of a basic income, which experiments alone cannot answer. The question of the "overall effect" is further complicated by the question of which of the many different combinations of G and t is one that should be used to examine the overall effect.
8. **Will people respond to basic income by withdraw completely from the labor force?** This is the most commonly sighted argument against a basic income, despite the fact that the U.S. NIT experiments found no evidence of it. However, even a small number of labor market withdrawals would be considered by many to be a significant piece of evidence against basic income, depending on the reasons for the withdrawal. The reasons for withdrawal will be much more difficult to determine than the fact of withdrawal.
9. **Will the labor supply effects of a basic income be so large that the program becomes economically unsustainable?** This question can be restated, what is the highest sustainable level of basic income? Certainly a BI of 1 Euro per year will not have significant labor supply effects, at some level it will begin to show effects of reducing labor supply, and at some level the labor-supply-reduction would become so large that the program would simply be unsustainable. A hugely important question is how high BI (G and t) can be before the program becomes unsustainable. And an important corollary to this question is whether the sustainable level is high enough to have the desired effect on poverty.
10. **What is the Income elasticity of the supply of labor?**
11. **What is the substitution elasticity of the supply of labor?**
12. **Do basic income and a negative income tax affect behavior differently?** Although it is possible to study this question with an experiment, it would require running two experiments one testing BI the other testing NIT, greatly increasing the cost of the experiment.
13. **How do all of the above estimates compare to estimates from nonexperimental sources?**

<u>Non labor market effects</u>: It is important to remember that these are the *effects* of BI, and the labor market effects are merely the side effects.

14. **What is the effect of BI on the well being of the poor and others?**
15. **What is the effect of BI homelessness?**
16. **What is the effect of BI on health?**

17. **What is the effect of BI on housing?**
18. **What is the effect of BI on education?**
19. **What is the effect of BI on the divorce rate?** The interpretation of these findings is extremely important because it is not necessarily desirable to reduce the divorce rate by making women financially more dependent on men.
20. **What is the effect of BI on time use within the home?** Will encourage more equal labor sharing within the home, or will it encourage a more traditional male-female division of labor? This question will be very difficult to test because any such effects are likely to take a long time to take effect.

<u>Questions that an experiment can’t answer</u>: The first three of these questions (21, 22, and 23) cannot be answered through a basic income experiment, and without them it three of the most important questions we’d like answered (24, 25, and 26) can’t be determined. However, estimates for these answers can be obtained from other sources, and they can be combined with experimental data to give a nonexperimental estimate of the answers we want. This needs to be done and reported along with the direct results of the experiment.

21. **What is the demand response to BI? What is the price elasticity of the demand for labor?**
22. **What is the general equilibrium response to BI?**
23. **What are the macroeconomic effects of BI?** Most particularly, what is the effect of BI on the unemployment rate and the business cycle? But also, we would like to know how its macroeconomic affects feedback on the effects of BI on wages and hours worked.
24. **What are the equilibrium effects of G and t the wages (and total incomes) of low-income workers?** This is one of the most important things we want to know about BI, but it cannot be determined from an experiment that is incapable of testing the demand response to basic income. However, demand response can be estimated from other sources and the parameters from the experiments can be used in simulation models to produce a nonexperimental estimate of the answer to this question. Because of the importance of this question, the report on the findings of these experiments should include these kinds of estimates.
25. **What is the equilibrium effect of various levels of G and t on hours worked?** This is also a very important question that cannot be answered experimentally, but should be estimate using simulation models.
26. **What is the efficiency cost (and cost in terms of tax dollars) of eliminating poverty through BI? How does it compare to the cost of doing so through strengthening the current system?** The total effect of BI on poverty depends critically on the demand response, which in term will have an effect on the cost both in terms of efficiency and in terms of tax dollars, and again it will take a simulation model to produce any kind of estimate.
27. **Will BI have secondary effects on the price of housing and other basic commodities that will counteract its effects on the standard of living of the poor?**
28. **Will BI effect the average profit rate for businesses?**

29. **Will BI make the poor freer?** I don’t know how to test this or whether it is possible to test. The answer depends critically on how freedom is defined.

These are my initial thoughts with just a little bit of thinking after the one-day workshop. I’m sure there are important question I’ve left out and I think this is an important place to start. I don’t think that the answer to any one of these questions is a knockout question that determines whether BI should be implemented or not. Instead, experimenters should determine feasibility and compare the desirable effects to the undesirable effects, and produce a full report detailing the plan’s pros and cons.