



## Claims That Can't Be Tested with Available Techniques

**Abstract** This chapter identifies several claims that are important to the public discussion and evaluation of Universal Basic Income (UBI) but that cannot be tested on an experimental scale. Unfortunately, for experimental research, these issues cannot be left out of the discussion of evidence about UBI. This chapter offers suggestions about how experimental reports should treat these questions to give people a good understanding of the meaning of experimental findings.

**Keywords** Basic income experiments • Negative Income Tax experiments • Social science experiments • Basic income • Universal Basic Income • Inequality • Poverty

This chapter discusses important empirical claims in the UBI discussion that are untestable or virtually untestable by the techniques available to potential UBI experiments, while Chap. 16 discusses claims that can be tested at least in some manner. That dichotomy is a simplification: in fact, there is a continuum between completely untestable and sufficiently testable claims, and it is a bit of a judgment call to determine which side of the line to put any particular claim. Tests will have *some* implications about most claims. The criteria that I've tried to use are whether the test can make some direct observations about the variable in question (as opposed

to being connected by theory alone) and whether the theory connecting observations to the final effect on the variable is fairly settled and tends to point in one direction.

Experiments are virtually unable to test the following claims:

- The exploitation claim
- The anti-exploitation claim
- The social-equality claim
- The capture claim
- The reduced-capture-corruption-and-bureaucracy claim(s)
- The labor-productivity claim
- The increased-innovation-and-entrepreneurship claim
- The structural-disadvantage claim
- The better-working-conditions claim
- The flexible-lifestyle claim
- The productive nonlabor claim
- The gender-role-reinforcement claim
- The degrowth claim
- The consumerism claim
- The self-destruction claim
- The economic-stimulus claim
- The economic-impediment claim
- The migration claim
- The shut-door claim
- The increased-support-for-redistribution claim
- The increased-overall-disadvantage claim
- The politically-enabled-proletarian claim
- The bought-off-proletarian claim
- The dynamic-efficiency claim

**The anti-exploitation claim** and **the exploitation claim** are not polar opposites. The anti-exploitation claim involves UBI's suspected ability to reduce exploitation of workers by employers. The exploitation claim involves UBI's suspected ability to enable nonworking recipients to exploit workers. Depending on how exploitation is defined, it is possible for both claims to be true at the same time. A UBI could make workers less vulnerable to exploitation by employers while making them more vulnerable to exploitation for the benefit of nonworking net recipients. Similarly, the exploitation claim is distinct from the harm-to-workers claim. The exploitation claim

focuses only on the effect of taxes. It is possible that some workers pay higher taxes under a UBI system, and so are exploited in the sense used, but are better off overall because of better wages and working conditions, as well as other community effects (see Chap. 16).

The concept of exploitation is so controversial and so morally loaded that researchers can't hope to say much about it directly, but it is so important that they should not ignore it either. They need to address other issues, such as the welfare claim, the benefit-to-workers claim, the better-working-conditions claim, and the harm-to-workers claim in the context of the exploitation debates. Unfortunately, these are difficult to address as well, as discussed in Chap. 16.

Despite the importance of **the social-equality claim**, experiments can say very little about it because it is inherently a community effect. Experiments will not directly reveal whether UBI net beneficiaries are less likely to be stigmatized than recipients of other redistributive programs. They won't observe housing segregation. Experimenters can ask people whether they feel socially isolated, but any relief from isolation is likely to be much larger in a long-run nationwide program. Even a very large saturation study might only pick up a small portion of this effect.

**The capture claim and the reduced-capture-corruption-and-bureaucracy claim(s)** cannot be tested in an experiment because they involve market reactions and/or the internal workings of a potential future government administration. The bureaucratic structure needed to run a small-scale, temporary experiment will provide no evidence about the bureaucratic structure needed for a large-scale, permanent national program or about the behavior of public employees within that structure. To the extent that these claims involve capture by private economic entities such as employers and landlords, an RCT will provide no direct evidence and a saturation study will provide very little. Labor markets are primarily national. The effect of geographically dispersed, randomly selected individuals will be nonexistent. The effect of geographically concentrated subjects in a saturation study will probably be much smaller than the national response, and how large it is will depend on how isolated the community is.

**The labor-productivity claim, the increased-innovation-and-entrepreneurship claim, and the better-working-conditions claim** are extremely hard to observe because they depend on the long-term reactions of both recipients and employers. Researchers can examine whether people in a short-term experiment seek training or education, whether they are healthier, and so on, but they will be unable to observe

whether and how any gains in these areas will eventually affect workers' productivity, entrepreneurship, and mobility. A major part of the argument for increased labor productivity and improved working conditions is through employers: a decline in labor effort gives employers incentive to increase wages, improve working conditions, and introduce higher productivity techniques. Because RCTs are unable to observe employer responses, they cannot observe whether this path actually leads to higher productivity or better working conditions. The best they can do to approach employer reaction is to observe whether the UBI trial leads to a decline in labor-market participation, which is only the first step in the chain expected to lead to these results.

Similarly, researchers can observe part of the first step of **the structural-disadvantage claim** and **the dynamic-efficiency claim** (does it improve education, childhood health and nutrition, entrepreneurship, and so on). A major part of the first step is true by definition: that UBI can reduce poverty. A great deal of theory and empirical evidence indicate that people who grow up and live with a reduced threat of poverty are much better able to succeed in ways that benefit themselves and others. The majority of claims on the UBI supporters' list are closely tied by theory and past observations to the structural-disadvantage claim. Unfortunately, experiments cannot directly observe whether these first steps toward reducing structural disadvantages do in fact lead to the dynamic process needed to produce greater efficiency or reduced disadvantage.

Yet these issues, especially the structural-disadvantage claim, cannot be left out of the discussion. The elimination of structural disadvantage is an important concern for any country that endorses the principle of equality before the law. It would be an enormous example of the streetlight effect if people involved in the discussion got distracted by quantitative comparison of how much the control and experimental groups work or drink from the important question of whether experimental evidence is connected by theory to good reasons to believe that UBI will have a significant effect on the structural causes of poverty, inequality, and other forms of disadvantage.

**The flexible-lifestyle claim, the productive nonlabor claim, the gender-role-reinforcement claim, the degrowth claim, the consumerism claim, and the self-destruction claim**, all share two problems. They require observing behavior that is not easy to observe and making subjective and/or normative judgments about that behavior. For example, researchers can observe whether parents use their UBI to spend more time with children and whether women do this more often than men, but they will not be able to

observe whether this reaction should be seen as reflecting increased flexibility in lifestyles or as reinforcement of gender roles. It will be very difficult to observe whether test subjects react in ways that lead to more or less growth and consumerism. Even if researchers are able to observe what subjects do with increased available nonlabor time, researchers would have to make controversial moral judgements to label that time “productive,” “unproductive,” or “self-destructive.”

Yet, researchers will need to find some nonjudgmental way to make findings about subjects’ behavior relevant for these debates. For example, although they should avoid making moral judgments, they should not avoid estimating whether UBI is correlated with alcohol or drug abuse.

In addition, most of these variables depend heavily on long-term and community effects. For example, the ability of a person using a UBI to adopt a more flexible lifestyle is likely to depend on factors such as whether the UBI is permanent and whether it affects the market and culture in ways that make flexible lifestyles more feasible and attractive. Any short-run observations of people in a small-scale experiment are likely to give little indication of the long-run reaction to a national UBI for any of these possible effects.

**The economic-stimulus claim, the economic-impediment claim,** and associated subclaims involve market reaction to UBI, which RCTs cannot observe at all and saturation studies can observe only partly. Some of the potential effects involved are macroeconomic, operating at the national and—in the Eurozone—at the supranational level. A small-scale experiment can say nothing about them. Evidence has to be gathered from other sources.

**The shut-door claim, the increased-support-for-redistribution claim, and the decreased-overall-redistribution claim** involve the way voters and policymakers feel about and respond to UBI at the national level over time. Experiments provide no evidence about them.

**The migration claim** fits largely into this category as well. If immigrants are eligible for a substantial UBI shortly after they arrive, it’s reasonable to think more immigrants will want to come. But most countries control their immigration and the eligibility rules for immigrants. So, they can choose whether and when immigrants become eligible and whether or not to allow increased immigration. Regional polities that do not control their migration from other parts of the country and are required by national rules to extend eligibility to migrants might face this issue, as might countries, such as European Union members, that have signed international agreements allowing free migration across national borders and prescribing when and whether

immigrants from treaty countries are eligible for redistributive programs. But whether a UBI increases immigration to these countries is not something a UBI experiment (which has to have fixed control and experimental groups) can test.

Although **the politically-enabled-proletarian claim** and **the bought-off-proletarian claim** are potentially observable in an experiment by comparing the political behavior of people in the experimental and control groups, there are at least four reasons to believe it is beyond the reasonable capability of an experiment. First, political behavior is extremely difficult to observe and hard to quantify. Second, community effects are likely to be substantial. The way one person behaves politically affects their fellow citizens' behavior. Third, once a national UBI is in place, it would change the political dialogue and political behavior in unpredictable ways. Fourth, the long-term political response after years of activity and discussion in a national policy setting is likely to be very different from the initial reaction of study subjects.

Nevertheless, researchers should be aware that these claims affect how people interpret the other experimental results. Suppose the experimental group works fewer hours than the control group. This result could be a good thing because it is the first step in a process consistent with the anti-exploitation claim, the better-working-conditions claim, the reduced-capture claim, the labor-productivity claim, the productive nonlabor claim, the degrowth claim, the capture claim, the consumerism claim, and the politically-enabled-proletarian claim. But this result could be a bad thing because it is the first step in a process consistent with the exploitation claim, the gender-role-reinforcement claim, and the economic-impediment claim. People who feel strongly about these issues are likely to see confirmation in the results, glossing over the distance between the first step that might be confirmed by the experiment and the final step required for their theory to produce the result they expect. Keeping people from making this leap is a difficult challenge for anyone writing about experimental findings.

The difficulty of relating the trial findings to the issues being debated might tempt researchers to report experimental results on their own terms without any comment on what they indicate for all these different debates, but as past experience shows, ignoring these debates makes it easier for people to spin the results one way or another.