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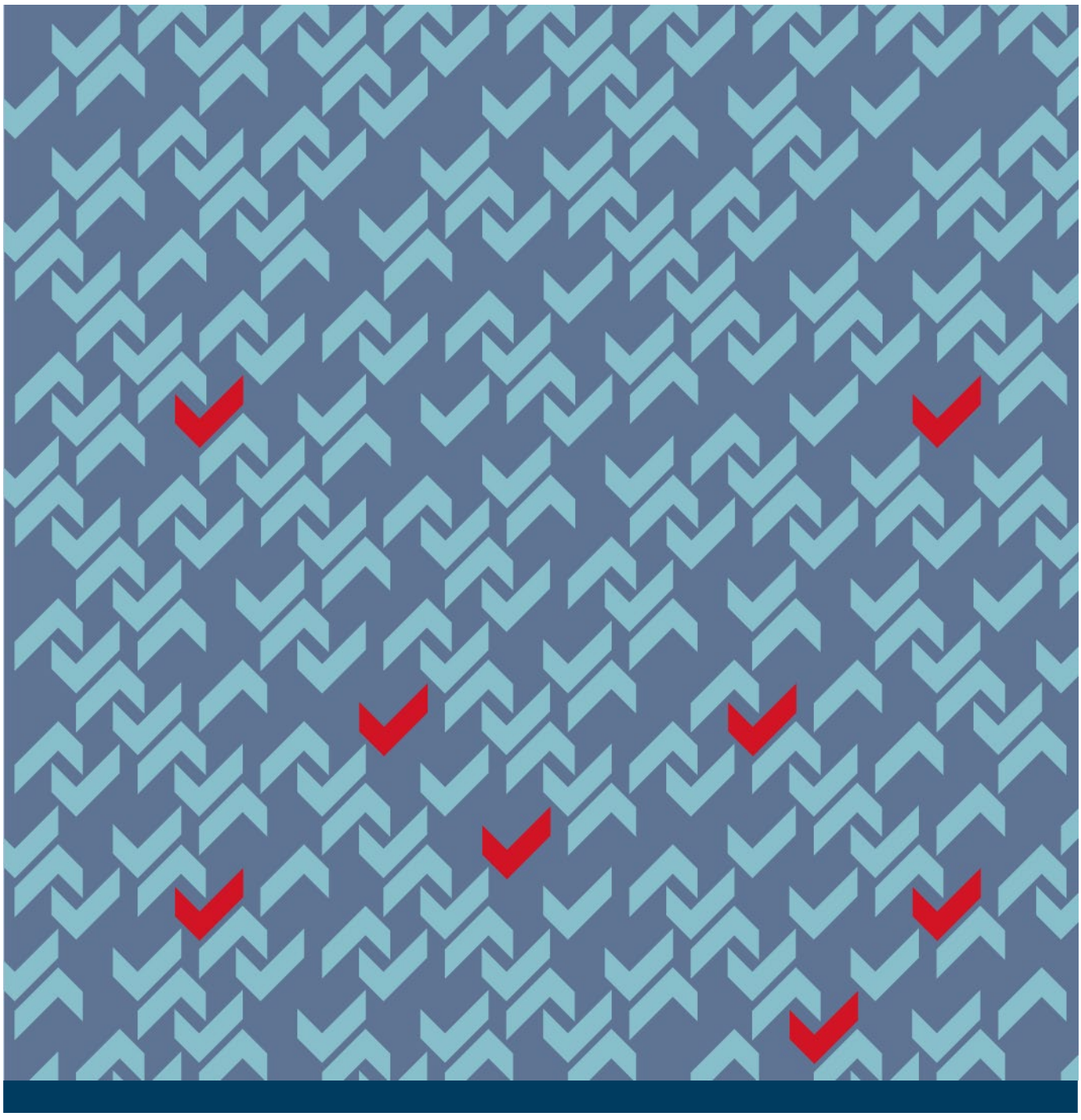
Report

Assessment Universal Basic Income Pilot Project

Recommendations for an Evaluable Design



Assessment
Universal Basic Income Pilot Project
Recommendations for an Evaluable Design

Final report
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Executive summary

Introduction

Universal Basic Income (UBI) is a cash benefit of sufficient amount that is paid to all citizens of a territory in a fully universal, individual and unconditional manner.

In 2021, the Government of Catalonia set up the Office of the Pilot Project to Implement Universal Basic Income with the task of designing and implementing a pilot project trial to test the effects of a UBI in Catalonia. Throughout 2022, **Ivàlua advised this Office to ensure that the pilot project is designed and implemented in an evaluable manner.** This document sets out the **main recommendations made by Ivàlua.**

Scope and objective of pilot project

The basic income pilot project of Catalonia will consist of granting a UBI to a total of 5,000 people throughout Catalonia for 24 months.

The UBI to be granted under this pilot project will be 800 euros per month for people over 18 years of age and 300 euros per month for people under 18 years of age. It will be a monthly payment and there will be no restrictions on the amount spent with the UBI. Those eligible to participate are people registered in Catalonia at least on 1 July 2022, who have an income of 45,000 euros gross or less and who did not declare a wealth tax in 2021.

The aim of the pilot project is to fill some of the knowledge gaps about its effects. On the one hand, about the effects of two UBI components that have been less studied, at least in the context of developed countries: its basic characteristic and its universal characteristic; on the other hand, about aspects of interest on which the evidence is scarcer or less conclusive (see knowledge gaps and literature review in Borrell-Porta, de Quintana & Segura, 2023).

Methodological proposal for evaluation

Based on the scope and objectives of the pilot project, the **proposal is to evaluate the impact of a UBI by means of a double pilot project made up of a randomised controlled trial at household level and a synthetic trial at municipality level.**

With regard to the randomised controlled trial, the proposal is to grant a UBI to 2,500 people randomly selected among all those eligible in Catalonia and to compare these results with a further 2,500 people, also randomly selected, who will not receive a UBI. This first component of the study should help to understand what occurs at an individual and household level when receiving a UBI; in other words, what decisions about work, education, family and so on individuals make as a result of receiving an unconditional monthly transfer for two years.

In terms of the synthetic trial, the proposal is to grant a UBI to all registered eligible persons in two municipalities in Catalonia comprising 1,200 to 1,400 inhabitants, with the idea that a UBI will eventually be received by some 2,500 more people. In this case, the results will be compared with the results of a number of municipalities (between 3 and 5) whose inhabitants will not receive a UBI. This second component will help to understand what occurs at an aggregate level when all the people in a given territory receive a UBI, primarily in terms of the use and functioning of public services, civic participation, economic activity and so on.

Opting for dual pilot project thus has the virtue of making it possible to draw conclusions at the level of Catalonia on the effects of a basic, individual and unconditional income thanks to the randomised trial, and at the same time to test the effects of universality thanks to the synthetic trial in the two small municipalities.

Data collection

In order to reduce the amount of information to be asked of participants through surveys, as well as to avoid as much as possible the problems arising from non-response, the **recommendation is to prioritise data collection through administrative registers whenever possible.** In this regard, the suggestion is that efforts should be made to obtain access to the following register data:

- Data on health status, consumption of medicines and use of health services available in the administrative records of the Department of Health.
- Data on the use of social services from the administrative registers of the Department of Social Rights.
- Data on educational performance, dropout rates and absenteeism available from the administrative registers of the Department of Education.

- Data on income, revenue and wealth available in the records of the Tax Agency.
- Data on job participation and conditions as well as entrepreneurship available from Contrat@, Catalonia's Labour and Production Model Observatory and Catalonia's Department of Business and Labour.

This list may be extended if other information sources offering relevant data are detected in the future.

To complement the data that can be obtained from administrative records, the **recommendation is that all persons over the age of sixteen participating in the pilot project are surveyed.**

The recommendation in both cases is that information should be collected from participants at three points in time: 1) before granting a UBI; 2) one year after first payment; 2) two years after first payment.

Procedure

In the case of the randomised trial, selecting the sample of potential participants will be made from the census with the collaboration of Idescat. The selection will be made at the household level from among all households in Catalonia, with all persons eligible who were registered in one of the selected households on 1 April 2022. For the synthetic trial, selecting the sample of potential municipalities to participate is based on Idescat's list of municipalities with between 1,200 and 1,400 inhabitants (so that together they do not add up to more than approximately 2,500 inhabitants).

In order for the evaluation to provide thorough information on the effects of the pilot project, it is important to take measures during implementation of the pilot project to ensure that the results of the studies are accurate and unbiased. It is therefore **necessary to have a sufficiently large sample size** to allow for adequate accuracy of the results to be obtained. The following is recommended in order to maximise the sample size:

- To have a sufficiently large pool of potential participants, so that, as the sample is reduced in number (people who do not receive the communication, people who do not apply, people who are not eligible and so on), it can be expanded by means of new invitations for people to participate. The recommendation is therefore that the initial sample requested from Idescat for the randomised trial

should be at least 6,600 households. And a representative sample of the population of each municipality should be available for the synthetic trial surveys, in addition to a pool of substitutes to be surveyed to compensate for the non-response of people in the initial pool.

- In the case of the randomised trial, participation in the pilot project should be dependent on having completed the baseline survey, because having people who are part of the pilot project but who do not respond to the surveys is a problem as they cease to form part of the evaluation.

- That authorisation to access the administrative data of the participants is included as part of the application to participate in the pilot project, as this will allow information to be available for all participants in it, thereby maximising both the statistical power of the study and its internal validity at least in relation to the variables of interest that are built from administrative data.

The other important issue is to ensure that the baseline survey information captures the situation of the various groups in the study before the effects of a UBI emerge. It is therefore important that the following issues are taken into account:

- In the case of the randomised trial, it is imperative that the baseline is passed before participants learn whether they are part of the control or treatment group, otherwise it may bias their responses. This should ideally be done even before the draw. It is therefore recommended that every effort is made to ensure that the baseline survey takes place before the outcome of the draw is known and, if necessary, that the start of implementing the pilot project is adjusted in order to enable this.

- In the case of the synthetic trial, the recommendation is that it be done as close as possible to the announcement of the treatment municipalities. An ideal scenario would be that it is done even before people in the treatment municipalities learn that they would be beneficiaries of the UBI to avoid it capturing pre-emption effects. Nonetheless, realising that this is most likely logistically unfeasible, the recommendation is to do this as close as possible to the public announcement.

Conclusions

The recommendations presented here aim to ensure that the pilot project provides useful, thorough information on the individual and aggregate effects of implementing a UBI in Catalonia. The proposed methodological design therefore has the virtue of allowing to draw conclusions on the effects of a UBI on individual and household-level decisions for Catalonia as a whole, while helping to test the collective effects deriving from universality in two municipalities representative of rural areas, towns and intermediate density areas in Catalonia. Procedural considerations are also aimed at ensuring that the selection of participants, the implementation of the pilot project and the collection of information are conducted in manner that ensures the conclusions drawn are accurate and unbiased.

The recommendations submitted serve the Office, which is the final decision-maker, in order to enable it to adapt the design of the pilot project to make it evaluable if it deems this necessary. There has been agreement in most cases between the Office's judgement and that of the evaluation team and these recommendations have been incorporated.

The recommendations are based on the information available at the time of writing. The arrival of new relevant information may cause Ivàlua's position to change on some of the issues discussed.

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1. Introduction

Universal Basic Income (UBI) is a cash benefit of sufficient amount that is paid to all citizens of a territory in a fully universal, individual and unconditional manner. The justification for this policy is based on many normative philosophical reasons, although it has come to the forefront in recent years as an innovative solution in terms of social policy to respond to the growing inability of states to address poverty and inequalities through the usual redistributive mechanisms, as well as to address factors such as unemployment, job insecurity, gender inequality, psychological discomfort and stress arising from financial instability.

The interest and centrality of this proposal on the political agenda has been increasing significantly over the past twenty years and several countries have implemented pilot projects to examine its feasibility and effects. The Government of Catalonia set up the Office of the Pilot Project to Implement Universal Basic Income in 2021 with the task of designing and implementing a pilot project trial to test the effects of a UBI in Catalonia before assessing its full deployment.

Against this background, the Office of the Pilot Project contacted Ivàlua to work on a proposal to design, implement and evaluate this pilot project. The approved proposal is divided into three distinct phases: the design phase of the pilot project, the implementation phase and the evaluation phase. **It has been agreed that Ivàlua's role is to advise the Office of the Pilot Project during the first two phases, while the evaluation phase is to be carried out entirely by Ivàlua.**

This document contains the position and recommendations that Ivàlua made within its assessment framework during the design phase of the pilot project. These recommendations are based on joint work carried out by the Office of the Pilot Project and Ivàlua to ensure that the pilot project design can extract relevant and thorough information on the **effect of a universal basic income (UBI) in Catalonia.** They have therefore been structured based on contributions from both teams. Nonetheless, the positions set out in this document are those preferred and defended by Ivàlua's evaluation team in accordance with what would be most desirable from an evaluation perspective. As a result of this joint work, these coincide in most cases with the options preferred by the Office of the Pilot Project, and they have guided decisions that have been made so far on the pilot project design. Nonetheless, there have been discrepancies in criteria between the two teams on certain specific points,

implementation challenges or constraints that have meant some of the recommendations set out below have not been finally included in the pilot project design (the pilot project design can be consulted at the Office of the Pilot Project and Ivàlua 2023).

This document has been structured as follows: section 2 describes the intervention and objective of the pilot project. Sections 3, 4 and 5 detail the recommendations stemming from the advice provided by Ivàlua in three areas: recommendations on intervention, on evaluation methodology and on implementation procedure. With regard to the recommendations on intervention and implementation, **Ivàlua has only advised on those aspects that affected the evaluability of the pilot project.** Similarly, and in mutual agreement with the Office of the Pilot Project, **design decisions without implications for the evaluability of the pilot were left to the Office.** Section 6 is a conclusion.

This report does not present a review of the literature and theory of change. This conceptual exercise can be found in a companion document entitled "Review of Evidence: Universal Basic Income Pilot Project. Review of Literature and Theories of Change" (Borrell-Porta, de Quintana & Segura, 2023).

2. Description of pilot project

2.1 Intervention: Universal Basic Income (UBI)

According to the Basic Income Earth Network (BIEN), a Universal Basic Income (UBI) is a cash benefit for every full member of society, paid unconditionally by the state without having to prove any conditions or meet any requirements.

A UBI has six main characteristics:

1. Basic: sufficient minimum amount to guarantee a dignified life.[1]

[1] No specific proposal exists to define a *basic* UBI. One of the dimensions of debate in the literature on UBI specifically revolves around how generous it should be and how to define and set an amount that helps to meet the basic needs of recipients. The difficulty of defining this characteristic is tied to the fact

2. Individual: it is an individual right, not for households, families or cohabitation units.

3. Stable: it is stable in nature and paid at regular periodic intervals, usually every month.

4. Cash: it is a cash transfer, not in kind or in exchange coupons, so it can therefore be used freely.

5. Universal: it is universal, in the sense that the entire population is entitled to it, without exclusions.

6. Unconditional: its payment is not conditional on any requirement or condition (looking for a job, participating in activation activities and so on).

Given these characteristics, a UBI is therefore expected to:

- Empower people receiving it to allow them to choose their future in a full, free and autonomous manner.
- Avoid paternalism and stigmatising controls on people receiving it.
- Avoid the administrative pitfalls and bottlenecks of other benefits.
- Avoid the problems arising from targeting, lack of coverage of other benefits and problem associated with non-applications.

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that life conditions depend on a variety of factors, differing greatly between countries and fluctuating markedly over time, so it is therefore difficult to make a specific recommendation. The literature on poverty and income security offers various approaches to measuring poverty that can help to better define this issue. For example, relative poverty measures based on income can serve as a reference for defining a UBI amount, and other proposals that focus more on consumer goods or needs can also be useful. Some examples for the case of Catalonia are the Income Sufficiency Index of Catalonia (IRSC) or the proposal of the metropolitan reference wage (Sánchez-Vidal et al., 2022). The at-risk-of-poverty indicator in Europe is the main tool for measuring individuals and households in poverty. This indicator defines a relative poverty threshold according to which all people with income equivalents below 60% of the median in the territory in question are poor. Following the logic of this indicator, a UBI of an amount equal to or close to the threshold would satisfy basic needs and therefore be a basic UBI.

- Eliminate poverty traps by being compatible with other sources of income (for example, not accepting an employment contract for fear of losing a benefit).

The UBI to be analysed in the pilot project will be for the amount of **800 euros per month for people over 18 years of age and 300 euros per month for people under 18 years of age.** It will be received by bank transfer to the beneficiary's current account. The transfer will be guaranteed as individual in the case of adults, while in the case of minors who do not have their own bank account, their families or legal guardians will receive and/or administer their UBI. The payment will be made monthly. In order for the UBI to allow for financial planning, the amount will be regular throughout the pilot project and the amount will be cumulative month by month. Finally, it should be noted that there will be no restrictions whatsoever on how the UBI is spent.

The Office of the Pilot Project has opted for these characteristics with the aim of making the intervention as close as possible to an UBI, bearing in mind the considerations in the preceding paragraphs, as well as budgetary and time considerations. The result is that the income paid is very similar to the characteristics of a basic income. First, it is a *basic* income, the amount of which takes the poverty threshold as a reference in order to almost cover the material needs considered to be basic.[2] Second, it is an *individual* income, paid to each individual, and not per household. Third, it is *cash* income, not paid in kind. Fourth, it is *regular*, paid periodically – in this case monthly. And fifth, it is *unconditional*; in other words, it is not subject to any conditions (such as looking for work and so on). However, the eligibility criteria indicate that one cannot *strictly* speak of a universal UBI, although neither can it be said that we are dealing with a case of *means-tested* transfers, as the excluded individuals are only those in the 10th decile of the income distribution.

[2] The poverty threshold is currently 941 euros per month, or 11,295 euros per year, for single-person households according to data from the latest Life Conditions Survey (National Statistics Institute, 2021). If the poverty threshold criterion were strictly followed, the amount per adult person should vary according to household composition to ensure that economies of scale are taken into account (following the modified OECD equivalence scale or variant thereof). Nonetheless, for budgetary and legal complexity reasons, it has been decided to grant a somewhat lower amount per adult person than would be the case for a single adult person according to the equivalence scale (947 euros), and higher than would be the case for an adult person living with more adults (700 euros). An amount of 800 euros has been chosen for all adults because of budgetary reasons and to avoid further legal complexities.

The Office of the Pilot Project has also deemed it appropriate to establish some eligibility criteria for receiving the UBI:

- **Residence criterion:** all persons registered in Catalonia on 1 July 2022 and on the date of submitting the application to participate in the pilot project, with the aim of determining a specific macrocosm for the pilot project and ensuring that the recipients reside in Catalonia.[3]
- **Income criterion:** people with incomes above 45,000 euros gross are excluded, with the aim of simulating the fiscal integration and redistributive logic of a UBI (Office of the Pilot Project to Implement Universal Basic Income in Catalonia, 2022).
- **Wealth criterion:** people who have had to file a wealth tax return for the year 2021 in accordance with the regulations governing wealth tax are excluded, once again with the aim of making a UBI consistent with the redistributive proposal advocated by the Office (Office of the Pilot Project to Implement Universal Basic Income in Catalonia, 2022).

The Office has moreover established the following conditions for updating the amount and eligibility:

- **Non-integration of births:** persons born to parents receiving a UBI during the duration of the pilot project are excluded from it.
- **Integration of age changes:** the UBI amount will be modified for those persons who reach legal age during the pilot project.

It should be noted that **there are several aspects of a UBI that the pilot project will not be able to consider.** The concept of *universal basic income* is used to refer to a number of proposals that meet the characteristics described in section 2.1, but which can be detailed in various ways depending on aspects related to its design and implementation, especially with regard to how a UBI will be financed and how it will be integrated into the existing welfare and/or financial and social benefits system in

[3] The literature on UBI is not free of debate on how to determine the recipient macrocosm of the benefit, e.g. if citizenship is taken as a criterion, immigrants are left out. This is an unresolved issue that needs to be determined to make the implementation of the pilot project feasible.

the context in which it is applied. Implementation of a UBI in the case of Catalonia would imply a major tax reform and substantial modification of its current benefit system. The Office is working to be able to simulate and integrate some of these aspects; for example, how a UBI will be taxed or how to minimise the effect it may have on people receiving financial benefits. Nonetheless, there are regulatory and legal constraints that cannot be influenced and the pilot project will therefore not be able to consider these.

3. Intervention recommendations

The Office of the Pilot Project has defined all the issues associated with designing a UBI; for example, the eligibility criteria or aspects related to the characteristics of the intervention (amount, periodicity or allocation level of benefit). However, Ivàlua has made a number of recommendations on issues with methodological implications for evaluating impact, particularly in terms of **internal validity.** The following issues were specifically the subjects of intervention recommendations:

- Degree of intervention
- Eligibility criteria
- Updating UBI amount according to age during duration of pilot project
- Allocation of UBI to persons born during the pilot project

On the other hand, issues that had no direct implications on the evaluability of the pilot project, although relevant in terms of the intervention, were excluded from Ivàlua's advisory framework. Specifically:

- Issues related to fiscal integration and taxation of UBI
- Scalability and financing issues.

The recommendations made with regard to the intervention are described below.

3.1 Degree of intervention

The degree of intervention refers to the type of UBI allocation to be followed by the pilot project. **It is recommended that a UBI should be allocated on an individual basis regardless of cohabitation status of the recipients.** This proposal implies that all recipients will receive a UBI and that its amount will not vary according to the composition of the household in which they live. All adults will therefore receive the same UBI amount (800 euros), while all minors will receive 300 euros. The recommendation is based on the following considerations:

- **Remaining true to UBI characteristics.** As explained in section 2.1, "Intervention: Universal Basic Income (UBI)", one of the basic characteristics of a UBI is that it is a subjective, individual right. The recommendation is therefore to allocate the benefit at an individual rather than household level in order to ensure that the treatment remains true to UBI principles.

- **Make it feasible to analyse the effect of a UBI on intra-household relations and on empowerment and emancipation decisions.** Income benefits generally use a household as the unit of reference: both the calculation and allocation of the benefit take into account household composition and are allocated for the whole living unit. This scheme implicitly assumes that the distribution of resources and decision-making power within the household is equal and that preferences within it are homogeneous and unproblematic. Nonetheless, **the literature highlights that benefits allocated at a household level may hide inequalities in access to and control of financial resources** among various members. For example, feminist literature has highlighted that household benefits can make female poverty invisible (De la Fuente, 2016) and perpetuate situations of intra-household gender-based violence. Similarly, studies have also found that they can hide the existence of divergent preferences (Dema & Díaz, 2014) and hinder the emancipation of women. Feminist literature has consequently underlined the need to consider income and financial needs on an individualised basis in order to promote social and economic independence of women (Sainsbury, 1999). Similarly, the dynamics and distribution of resources and power within households can also affect the emancipation of young people. One of the goals of the pilot project is to examine the effect that a UBI may have on intra-household power relations, on the empowerment of women and on the

emancipation decisions of young people (see Table 1 and the Ivàlua review of evidence report – Borrell-Porta, de Quintana & Segura, 2023 – for more details). Allocating the benefit individually ensures that these factors will be less affected by intra-group dynamics, thereby allowing us to observe whether they are modified in cases where the benefit is an individual entitlement.

3.2 Eligibility criteria

As discussed in section 2.1, "Intervention: Universal Basic Income (UBI)", the Office of the Pilot Project has deemed it appropriate within the framework of the pilot project to establish eligibility requirements based on residence, income and wealth. The recommendations that Ivàlua has made on these eligibility criteria, in addition to decisions regarding the updating of the amounts and eligibility by extension of children of beneficiaries born during the pilot project, are set out below.

Residence criterion

In order for the intervention to remain as true as possible to the idea of universality, Ivàlua recommended that the **residence criterion should be registration in Catalonia, as this is the most accessible type of common residence criterion.** At the same time, Ivàlua agreed with the Office of the Pilot Project on the need to require a few months of registration to make implementation of the pilot project viable: to have a specific reference period for making data requests and to avoid strategic decisions and fraud once the pilot project has commenced.

Income and wealth criteria

Ivàlua recommended not to apply any exclusion criteria in the pilot project with regard to income and wealth. As stressed by the Office of the Pilot Project in the draft document (Office of the Pilot Project to Implement Universal Basic Income in Catalonia, 2022), the purpose of these exclusion criteria is to simulate the fiscal and redistributive effects of a UBI as well as to exclude from the pilot project anyone who in a real scenario would be *net contributors*; in other words, they would finance a major part of the intervention.

Nonetheless, Ivàlua believes that this proposal has some problems. As has already been mentioned, it is unfeasible to simulate in the pilot project all the fiscal and

redistributive implications that a UBI's effective implementation would have: this would require a truly exhaustive income test to simulate all of a UBI's fiscal effects and to use this information to make the transfer progressive for all recipients. It would also be necessary to carry out a periodic update in order to adapt a UBI amount to changes in income and wealth that may occur over the duration of the pilot project. Applying these issues is infeasible in terms of implementation.

Consequently, Ivàlua deems it inappropriate to try to simulate this scenario based solely on two fixed, absolute criteria of income and wealth. It is important to note that this exercise may be unfair in cases where changes in income levels occur during the duration of the pilot project or for people who are ineligible based on wealth but are eligible based on income. On the other hand, because the pilot project cannot exhaustively reproduce a UBI's fiscal and redistributive effects, the proposed treatment (a fixed amount for all eligible persons) has de facto the logic of a *social dividend*, a form of a UBI that meets the six characteristics described in section 2, but which is financed with the dividends stemming from financial or social resources and is of a fixed amount for all citizens. Ivàlua therefore believes that not applying any exclusion criteria by income and wealth would make the treatment of the pilot project more faithful to a social dividend, and the results would be interpretable according to this logic.

Allocation of UBI to persons born during pilot project

Allocating a UBI on an individual basis for each household member ensures that the UBI amount will be basic for each member, even if they share expenses. There is a slight risk of the benefit losing its basic status in cases where births occur in recipient households because it will cover the needs of more people. **Ivàlua has recommended allocating a UBI to children born in UBI beneficiary families during the pilot project.**

4. Recommendations on evaluation methodology

4.1 Background

Throughout the process of designing the pilot project for a UBI in Catalonia, and as explained in section 2, the Office has prioritised examining the effects of a *basic* and *universal* UBI on micro factors (e.g. health and mental health, well-being, income and

poverty, and participation) as well as on meso and macro aspects, especially the use and functioning of public services (health, social services and education), in addition to spillover and general equilibrium effects on the labour market, consumption and prices (see Borrell-Porta et al. 2023 for a detailed overview of the outcomes included in the pilot project). During the design advisory phase of the pilot project, Ivàlua has explored several designs to assess the impact of a UBI in order to choose the most appropriate, robust proposal. This exercise has been done by bearing in mind three elements that cannot be modified by Ivàlua:

- Amount of the benefit, established by the Office
- Duration of pilot project, established by the Office
- Budget available for pilot project

Following the instructions of the Office of the Pilot Project, **Ivàlua has explored and rejected various designs to test the effects of a basic and universal UBI by specifically considering what aspect of universality these can capture.**

Cluster randomised trial

A cluster randomised trial (CRT) is an experimental design in which randomisation of the intervention occurs at a cluster level (e.g. municipalities or schools), and these clusters are randomly assigned to the treatment and control group (Baird, Bohren, McIntosh & Özler 2018). All individuals in the cluster are treated in the treatment group clusters. This type of design is suitable for testing the effects of a UBI because it helps to simulate universality in a specific context.

Partial population experiment

A partial population experiment is a type of cluster experiment in which the total treatment population within a cluster is modified as part of the treatment. Two-stage randomisation occurs: first, the clusters are randomly assigned to a treatment and control group, and then, within each cluster, a fixed percentage of individuals are randomly assigned to receive the treatment (Baird et al. 2018). We would therefore have treated and untreated persons in the treatment clusters, and no treated persons in the "pure" control clusters. This is a truly interesting design for testing the effects of a UBI

because it would allow us to detect spillover effects, general equilibrium effects and *average beneficiary* effects.

Randomised saturation

A randomised saturation experiment is a type of cluster experiment that includes the following as part of the treatment: a) the total treatment population in a cluster is modified, and b) the percentage of individuals to be treated in the treatment clusters varies by cluster or cluster group, in such a way that there are pure control clusters and treatment clusters with various levels of saturation (Baird et al. 2018); for example, with 80% of treatment individuals or 20% of treatment individuals. This design is useful for studying the effect of a UBI, because apart from identifying spillover effects, general equilibrium effects and the effects of changing the identity of the average individual beneficiary, it would allow us to ascertain whether the intensity of treatment leads to the intensity of these effects to vary.

These designs have been discarded due to feasibility issues. The robustness of these designs depends on the number of available clusters. A sample of at least twenty municipalities was required for these designs to be feasible and to detect statistically significant effects. Given the characteristics of the pilot project (sample and size), this implied selecting very small municipalities with a maximum of 250 people. This option was discarded for two reasons. First, it would be very difficult in these municipalities to observe aspects related to the use and functioning of social, health and educational services, given that the service structure of these municipalities is minimal. Second, it was thought that these municipalities were not very representative of Catalonia as a whole, thereby greatly reducing the external validity of the pilot project.

4.2 Design chosen

In light of the limitations of the previous options, **Ivàlua proposes assessing the impact of a UBI through a dual pilot project consisting of a randomised controlled trial (RCT) at household level and a synthetic trial at municipality level.**

An RCT is an impact evaluation method in which eligible units (individuals, households, companies, schools and so on) are randomly assigned to either a group receiving the intervention, known as the *treatment group*, or a group not receiving the intervention, known as the *control group*. Given a sufficiently large sample, the

random assignment of units to one of the two groups helps us to obtain two equal groups, both in terms of observable and unobservable variables, thereby avoiding so-called *selection bias*. In other words, people receiving the intervention and those not receiving it have different characteristics that simultaneously condition their results. Consequently, it is impossible to distinguish whether the differences observed between the two groups are due to participation in the programme or policy or to the distinctive characteristics that both groups already possessed at the outset.

A synthetic control method is a statistical method for evaluating the effect of an intervention in comparative case studies. The effect of a policy or programme in the synthetic control method is estimated by comparing the evolution of the variable(s) of interest of the treatment unit(s) with the evolution of a synthetic control group. This group is created according to a weighted combination of control units chosen so as to minimise differences in the evolution of the variables of interest between the treatment units and the synthetic control in the pre-intervention period. The evolution of a synthetic control group in a synthetic control method therefore acts as an estimate of the counterfactual; in other words, what would have happened to the treatment units if the intervention had not taken place.

Approximately 10,000 people will participate in the UBI pilot project in Catalonia, of which 5,000 will receive a UBI. As part of the randomised controlled trial, **the proposal is to grant a UBI to approximately 2,500 persons randomly selected** among all eligible people in Catalonia, and to compare the results with another 2,500 also randomly selected persons who will not receive a UBI. As part of the synthetic trial, the proposal is to **grant a UBI in two Catalan municipalities of 1,200 to 1,400 inhabitants to all registered eligible persons,** with the idea that a UBI will eventually reach about 2,500 additional persons. The results in this case will be compared with the results of a number of municipalities (most likely between 3 and 5 municipalities) whose inhabitants will not receive a UBI.

This dual pilot project has the **virtue of making it possible to draw conclusions at the level of Catalonia on the effects of a basic, individual and unconditional income thanks to the randomised trial, while at the same time being able to test the effects of universality thanks to the synthetic trial in the two small municipalities.**

The trial should, on the one hand, help us to understand what happens at both an individual and household level when receiving a UBI; in other words, what decisions

at work, education, family and so on individuals make as a result of receiving an unconditional monthly transfer for two years. Table 1 summarises the main dimensions and sub-dimensions that we propose to explore.[4]

Table 1 – Dimensions of interest at individual and household level

Dimensions of interest	Sub-dimensions of interest
Material well-being	Income, expenditure and living conditions
Financial behaviour	Savings, debt and investment
Emotional well-being and autonomy	Life satisfaction, mood, cognitive ability, decision-making ability, freedom of choice, intra-marital affective relations, youth emancipation and time use
Labour market	Employment, job search and entrepreneurship
Health	Health status and use of health services
Gender and intra-household relations	Domestic and care work and resource distribution
Values and attitudes	Political participation and attitudes towards welfare state
Relationship with social services	Use of and satisfaction with social services

The synthetic trial, on the other hand, should allow us to understand what happens at an aggregate level when all people in a territory receive a UBI, primarily in terms of the use and functioning of public services, civic participation, economic activity and so on. The main dimensions of interest are listed in Table 2.

Table 2 – Dimensions of interest at municipal level

Dimensions of interest	Sub-dimensions of interest
Operation of public services	Functioning of health services, mental health, social services and education services
Local economy	Housing prices, inequality, economic activity, unemployment, business and cooperative fabric, etc.
Participation and social cohesion	Associative fabric, political participation, social conflict

Furthermore, subject to statistical criteria on the samples of both studies, the comparison of the respective results can help us to understand which are the direct effects of receiving an income and which are the general equilibrium effects resulting

[4] See the review of evidence report (Borrell-Porta et al., 2023) for a more detailed understanding of the choice of dimensions of interest.

from the fact that a whole community is receiving it.[5] In the case of the synthetic trial, the proposal is also therefore to study what occurs at a both an individual and household level in the municipalities receiving a UBI by using the same data collection instruments as in the randomised trial (survey).

The two studies are thus considered complementary, given that they respond to different questions that together provide answers to the main objectives of the pilot project.

Design limitations

The design as it stands fails to help us to know which would be the general equilibrium effects of granting a UBI to all people in Catalonia, because the aggregate effects that can be detected in municipalities of 1,200-1,400 inhabitants in the synthetic trial are unlikely to be extrapolated to large urban centres.

With the randomised trial, on the other hand, it will be possible to draw conclusions about the population of Catalonia as a whole. But the final size of the sample and its composition will determine for which population subgroups it is possible to conduct specific sub-analyses and for which it is not.

Distribution of the sample between the two pilot projects

One of the limitations of the pilot project is that a maximum of 5,000 UBIs can be granted; in other words, there can only be 5,000 beneficiaries at most, and this figure needs to be divided between the two studies because of the pilot project's dual design.

The larger the study sample in both cases, the better. A larger sample in the case of the randomised trial means higher statistical power. In other words, it increases the probability that any real effects produced by a UBI will be able to be detected. To explain this in another way, it reduces the minimum detectable effect; in other words, the effect size below which we cannot accurately distinguish a non-zero effect from a null effect, even if such an effect exists. For example, a minimum detectable effect of 10%

[5] The synthetic trial has been designed to be representative for non-urban municipalities in Catalonia. Its results will therefore be comparable to the results of the randomised controlled trial for the *subgroup of UBI beneficiaries of the randomised controlled trial living in non-urban municipalities* (i.e. excluding beneficiaries living in large towns). Making this comparison will help us to identify direct effects from general equilibrium effects.

implies that we may not be able to distinguish it from a null effect if an 8% effect exists.

In the case of the synthetic trial, a bigger sample implies that the treatment municipalities may be larger and thus have more scope for interesting general equilibrium effects in terms of labour market dynamics, functioning and use of public services and so on. There is therefore a trade-off between providing a bigger sample to one study or the other.

At the same time, it is deemed suitable in the case of the synthetic trial not to treat only one municipality, but instead to treat two. Although the disadvantage of dividing the study sample between two municipalities is a reduction in the size of the treatment municipalities, we believe that it has a greater advantage: treating two municipalities protects us from a situation in which one of the treatment municipalities receives an exogenous shock (such as a Christmas jackpot or installation of a large factory). It would therefore be impossible to distinguish between the impacts caused by a UBI and those caused by an exogenous shock, thereby making it impossible to robustly assess the impact of a UBI in the municipality in question.

To evaluate the advantages and disadvantages of providing a bigger sample to one study or the other, our premise is a situation where the **number of people benefiting from a UBI is divided equally between the two studies (scenario 1),** and comparing this situation with an alternative one where a bigger sample is provided in the synthetic trial in order to increase the size of the treatment municipalities, thereby reducing the sample of people benefiting from the randomised trial by half: 1,250 (scenario 2).

The following tables indicate the implications of moving from a randomised controlled trial with a total sample of 5,000 people in which half receive a UBI (scenario 1) to one with a total sample of 2,500 in which 1,250 receive a UBI (scenario 2), concerning the minimum detectable effect on a small selection of variables of interest.[6] The

[6] The table shows the minimum detectable effect with a statistical power of 80% and a statistical significance of 5%. Statistical power is the probability of making a Type II error: failing to reject the null hypothesis when it is false; in other words, concluding that there is no effect when in fact there is a non-zero effect. Statistical significance is the probability of incurring a Type I error: rejecting the null

calculations presented assume that approximately 15% of the beneficiaries will be under the age of sixteen and therefore will not be surveyed, and that all persons aged sixteen and over will respond to the survey. The available sample in scenario 1 is therefore 4,250 persons and 2,125 persons in scenario 2. The effects of non-response on the statistical power of the trial are discussed below.

hypothesis when it is true; in other words, concluding that there is a non-zero effect when there is no effect. The sample is assumed to be clustered at household level in the case of individual-level variables, with clusters of 2.5 persons on average.

Table 3 – Minimal detectable effects in randomised trial: individual-level variables[7]

			SCENARIO 1		SCENARIO 2	
VARIABLES	**Mean**	**Standard deviation**	**Minimal detectable effect (absolute)**	**Minimal detectable effect (relative)**	**Minimal detectable effect (absolute)**	**Minimal detectable effect (relative)**
Labour market						
Persons aged >16 in employment (%)	40.3%	0.49	5.33	13.2%	7.53	18.7%
Self-employed persons over 16 years of age (%)	4.4%	0.21	2.24	50.4%	3.17	71.3%
Dedication in main job (number of hours per week)	38.7	8.50	0.92	2.4%	1.31	3.4%
Persons aged >16 engaged in domestic and care work (%)	7.3%	0.26	2.83	38.7%	4.00	54.7%
Investment in human capital						
Persons aged 16-25 in education (%)	54.2%	0.50	5.41	10.0%	7.66	14.1%
Persons aged 26 to 65 in education (%)	5.6%	0.07	0.81	14.5%	1.14	20.6%
Material well-being						
Persons with unmet medical needs (%)	2.2%	0.15	1.59	73.1%	2.24	103.4%
Persons with unmet dental needs (%)	3.0%	0.17	1.86	61.8%	2.63	87.3%
Persons saving (%)	43.2%	0.50	5.38	12.5%	7.61	17.6%

[7] The mean and standard deviation values are calculated using the B-Mincome sample for the general life satisfaction variable. The source of information is the Life Conditions Survey 2020 for the other variables. The minimum detectable effect in absolute value in the case of variables expressed as proportions is expressed in percentage points, while for the other variables it is expressed in the units shown in brackets.

			SCENARIO 1		SCENARIO 2	
Persons spending a small amount of money on themselves each week (%)	25.7%	0.44	4.75	18.5%	6.72	26.1%
Subjective well-being						
Satisfaction with life (scale 0-10)	5.0	2.61	0.28	5.7%	0.40	8.0%
General state of health (% very good or good)	63.6%	0.48	5.23	8.2%	7.40	11.6%
Use of time						
People who regularly participate in leisure activities (%)	37.4%	0.48	5.26	14.1%	5.88	15.7%

Table 4 – Minimal detectable effects in the randomised experiment: household-level variables[8]

			SCENARIO 1		SCENARIO 2	
VARIABLES	**Mean**	**Standard deviation**	**Minimal detectable effect (absolute)**	**Minimal detectable effect (relative)**	**Minimal detectable effect (absolute)**	**Minimal detectable effect (relative)**
Household economy						
Expenditure on food (€/uc)	277.6	144.7	19.7	7.1%	22.0	7.9%
Expenditure on eating out (€/uc)	91.8	87.7	11.9	13.0%	13.3	14.5%
Expenditure on public transport (€/uc)	29.7	27.1	3.7	12.4%	4.1	13.9%
Expenditure on private transport (€/uc)	94.7	99.0	13.5	14.2%	15.0	15.9%
Debt repayment (€/month)	332.7	325.4	44.2	13.3%	49.5	14.9%
Risk of poverty						
Bill arrears (%)	8.9%	0.3	3.9	44.3%	4.4	49.5%
Households with material deprivation (%)	5.4%	0.2	3.1	57.2%	3.4	63.9%
Households with social welfare income (%)	3.3%	0.2	2.4	73.8%	2.7	82.5%
Social welfare income (€ per year)	78.5	546.9	74.4	94.8%	83.1	106.0%
Households with housing benefit (%)	4.4%	0.2	2.8	63.4%	3.1	70.8%

[8] The mean and standard deviation values are calculated using the B-Mincome sample for the general life satisfaction variable. The source of information is the Life Conditions Survey 2020 for the other variables. The minimum detectable effect in absolute value in the case of variables expressed as proportions is expressed in percentage points, while for the other variables it is expressed in the units shown in brackets.

			SCENARIO 1		SCENARIO 2	
Housing benefit (€/year)	127.9	900.5	122.4	95.7%	136.9	107.0%

As the tables above indicate, moving from scenario 1 to scenario 2 in the case of the individual variables implies increasing the minimum detectable effect (in relative terms) by between 1 percentage point (the minimum detectable effect on hours spent on main job increases from 2.4% to 3.4%) and 30.3 percentage points (the minimum detectable effect on the percentage of people with unmet medical needs increases from 73.1% to 103.4%), with an average effect of 10.3 percentage points. While the minimum detectable effect (in relative terms) in the case of the household-level variables increases between 2.9 percentage points (food expenditure per consumption unit) and 39.7 percentage points (amount received in housing allowances), with an average effect of 18.5 percentage points of increase.

In the synthetic trial, on the other hand, moving from scenario 1 to 2 means being able to treat two municipalities of approximately 2,000 inhabitants instead of two municipalities of around 1,300 inhabitants (taking into account that some people will not be eligible as they exceed the income and/or wealth thresholds), which is not a very significant leap in scale in terms of the general equilibrium effects that the municipalities may experience. Scenario 1 therefore appears to be the most favourable after weighing the pros and cons.

The third alternative would be to further increase the sample of the experiment to the detriment of the sample of the synthetic trial. Although this would mean further reducing the number of inhabitants of the treatment municipalities, thereby reducing the probability that these municipalities have public services of interest (in the fields of education, health and social services) and that interesting general equilibrium effects can be detected.

The recommendation is therefore to choose the scenario that distributes the sample evenly between the two pilot projects, including 2,500 treatment persons in each pilot project and 2,500 in the control group.

4.3 Randomised trial at registered address level

Objective

The objective of the randomised household trial is to respond to the following question: "What are the effects of providing an individual UBI on the behaviour, decisions and well-being of recipients and their households?" Table 1 contains a summary of the specific dimensions on which we wish to establish this impact.

Feasibility conditions

We need to have at least two comparable groups and information from both these groups to make a randomised controlled trial feasible and allow us to estimate the effect of an intervention.

Two comparable groups

Given a sufficiently large sample, randomly assigning eligible units to the control or treatment group ensures that the only factor determining participation in the programme or policy was chance, thus no other variables existed that differ systematically between the two groups. This ensures that if we observe differences between the two groups these can only be a consequence of their participation in the programme, given that it is the only factor differentiating them.

Therefore, **the assignment should be randomised and the groups resulting from the draw should be respected throughout the pilot project for the groups to be comparable.** In other words, all people assigned to the treatment group should be treated as such, regardless of whether they end up receiving a UBI or not, and the same applies for the control group.

Care should also be taken to ensure that there are no differences in the response rate of surveys administered throughout the pilot, as this is an indicator that comparability between the two groups is being broken.

Information from both groups

We need to have access to comparable information from both the treatment and control groups in order to be able to assess the impact of a UBI, whereby comparable is understood as the same information is being collected in the same way and for the same person profile (ideally for all participants) in both the treatment and control groups.

With regard to the information, it is essential to have:

- Data referring to the dimensions of interest upon which the impact is to be measured for moments following implementation of the intervention.

Similarly, it is also desirable to have the following in order to improve the precision of impact estimates, as well as to be able to conduct analyses by subgroups of interest:

- Data on the dimensions of interest upon which impact is to be measured for a point in time prior to implementing the intervention. These data would not only increase the precision of impact estimates, but also help to confirm that the two groups are indeed comparable prior to the intervention.

- Data on socio-demographic and economic variables that are determinants of the variables of interest for the time prior to implementing the intervention. These data would not only increase the precision of impact estimates, but also enable analyses by subgroups of interest.

Below are the recommendations made by Ivàlua during this evaluation to ensure that these conditions are met.

Randomised allocation at registered address level

An important decision when it comes to designing an RCT is at which level randomisation will be performed. In other words, at which level of aggregation are the treatment units chosen. Or in the case of this pilot project, at which level of aggregation are the persons to be offered a UBI chosen. Three alternative options have been considered in this case: 1) to randomise at household level; 2) to randomise at individual level; 3) to randomise at registered address level. The pros and cons of each are presented below.

Option 1 (to randomise at household level), although conceptually the most desirable, this was discarded from the outset as its implementation was unfeasible. We state that it would conceptually be the most desirable because, as explained in section 2, one of the main objectives of the pilot project is to test the effect of a basic amount UBI. In this sense, taking into account that a UBI is granted at individual level, to randomise at household level (in other words, defining a set of eligible households, assigning each household to either the control or treatment group, and offering a UBI to all members of the treatment households) would help us to ensure that all individuals in the household are chosen to receive a UBI. This is crucial to ensure that the intervention we analyse maintains the characteristic of a basic UBI; in other words, a sufficient amount for a dignified life. More specifically, it avoids that only one person in households with more than one member receives a UBI and decides to share it with the others members, which in practice would make the benefit granted neither individual nor basic, thereby diluting the intensity of the treatment.

This option in practice was nonetheless unfeasible for the following reasons. The database from which to select the sample of persons to be invited to participate in the UBI pilot project is the census register of Catalonia. This register of Catalonia is an individual-level record of all persons officially and habitually residing in a municipality in Catalonia and, therefore, of the entire macrocosm of persons potentially eligible to receive a UBI. It is also possible to group these people into registered addresses, thereby opening up the possibility of using the registered address instead of the individual as a sampling unit and, therefore, of randomisation. But it is **impossible to reconstruct households from the register,** so to use households as the unit of randomisation we would first need to select individuals and then contact them so that the same person could help us reconstruct their household. This forces upon us a number of implementation challenges that are outlined below:

- It would mean having to contact thousands of people for whom we only have their postal addresses as their only contact information to help us reconstruct their household. This would add an additional contact to the process and delay the implementation schedule of the pilot project.

- But more importantly, knowing that this is a pilot project under which a UBI will be granted, people would have unreasonable incentives to list persons as part of their household although not actually part of it. The Office of the Pilot Project would not be able to verify this information, thereby leaving room for fraud.

The second option considered was to randomise at individual level. The procedure in this case would comprise:

1. Start from individual-level registration in the census as the sampling framework.

2. Select a random sample of individuals and invite them to participate in the pilot project.

3. Apply the agreed steps and filters to obtain a group of individuals to participate in the draw.[9]

[9] The following sections discuss the recommendations on what conditions people in the initial sample should meet in order to be part of the draw.

4. Randomly assign each person to either the control or treatment group.

Table 5 summarises the main implications of randomising at individual level.

Table 5 – Implications of randomising at individual level

	Implications
Who participates in pilot project?	All persons participating in UBI draw.
What effect will we estimate at individual level?	What happens to a person when you give him/her a UBI but not the other members of his/her household.
Which households will we analyse?	Households formed by participants.
What effect will we estimate at household level?	What happens to a household when you give one of its members a UBI.
Treatment intensity at household level	**Low:** Except in the case of single-person households, the other households in the treatment group will only be partially treated (only one of the individuals in the household will receive a UBI). The intensity of treatment at household level decreases with the number of household members.
Main advantages	1. Eligibility is determined by objective, non-manipulable census information; there is therefore no room for fraud. 2. The treatment is homogeneous: the treatment for all participating persons is that an individual receives a UBI in a household in which no one else receives it.
Main disadvantage	**If the transfer is shared with other members of the household, the income ceases to be individual and basic** (dismantling of treatment), two of the key features of UBI.
Other considerations	There is no need to reconstruct households at any point, as there is only one beneficiary per household, and the only information source about the household is the individual.

The third option is to randomise at registered address level. The procedure in this case would comprise:

1. Start from the register at address level in the census, which Idescat itself can construct as a sample framework.

2. Select a random sample of addresses and invite all persons registered on a given date to participate in the pilot project.

3. Apply the agreed steps and filters to obtain a group of persons to participate in the draw.[10]

4. Add these people to the other persons at their address.

5. Randomly assign each address to either the control or treatment group.

6. Assign all persons in the control addresses to the control group, and persons in the treatment addresses to the treatment group.

Table 6 summarises the main implications of randomising at registered address level.

Table 6 – Implications of randomising at registered address level

	Implications
Who participates in pilot project?	All persons participating in UBI draw.
What effect will we estimate at individual level?	What happens to a person when you give him/her a UBI and a varying proportion of the other members of his/her household.[11]
Which households will we analyse?	Households formed by participating individuals.
What effect will we estimate at household level?	What happens to a household when you award UBI to a variant proportion of its members.
Treatment intensity at household level	**Variant:** There will be three types of households depending on intensity of treatment: - Fully treated households: households in which all members are effectively registered at the same address in the treatment group. - Partially treated households (type 1): households living together in a treatment group address, although where not all members are officially registered. - Partially treated households (type 2): households of persons who are registered at a treatment group address, although they no longer reside there but are part of another household whose members reside at an address that is not part of the treatment group.
Main advantages	1. The eligibility criterion is verifiable and non-manipulable, avoiding room for fraud. 2. It allows at least some of the participating households to have all members receiving a UBI.

[10] The following sections discuss the recommendations on what conditions people in the initial sample should meet in order to be part of the draw.

[11] The register is not a true reflection of people's cohabitation situation. Therefore, there may be households that live together, but do not have all members registered at that address, which would mean that part of the household is not eligible to receive a UBI.

	Implications
Main disadvantage	Resulting treatment is "heterogeneous": one individual receives a UBI in a household in which a varying proportion of other members receive it.
Other considerations	We can group eligible individuals into households at an initial contact, so that household module can respond to only one person per household.

Taking into account the pros and cons set out in Table 5 and Table 6, as well as the fact that one of the most important characteristics of this UBI pilot project is that the amount is basic, **Ivàlua recommends randomising at registered address level so that the intervention is diluted as little as possible at household level.**

Information gathering

Administrative data

In order to reduce the volume of information asked by surveying the participants, as well as to avoid as much as possible the problems stemming from high non-response rates or differential response rates between the two groups in the trial, the **recommendation is to prioritise whenever possible the collection of data through administrative records.** It is therefore suggested that efforts be made to obtain access to the following registry data:

1. Data on health status, consumption of medicines and use of health services available in the administrative records of the Department of Health.

2. Data on the use of social services from the administrative registers of the Department of Social Rights.

3. Data on educational performance, dropout rates and absenteeism available from the administrative registers of the Department of Education.

4. Data on income, revenue and wealth available in the records of the Tax Agency.

5. Data on job participation and conditions as well as entrepreneurship available from Contrat@, Catalonia's Labour and Production Model Observatory and Catalonia's Department of Business and Labour.

This list may be extended if other information sources offering relevant data are detected in the future.

Surveys

In order to complement the data that can be obtained from administrative records, **the recommendation is that surveys should be administered to all persons over the age of sixteen participating** in the pilot project. Not interviewing persons under the age of sixteen implies that:

- The only information that can be examined for this subset of the population will be that which is provided to the reference person from his/her household, as well as that which can be obtained through administrative records.
- If we estimate that 15% of the participants will be under the age of sixteen (if the response rate were 100%), we would obtain at most survey data for 85% of the participants.

The following is suggested as a first operational proposal on how to collect survey data:

- **An individual questionnaire:** to collect individual-level data from all persons over the age of sixteen participating in the trial.
- **A household questionnaire:** to collect household-level data for all households to which at least one person participating in the trial belongs.
- **A questionnaire for minors:** to collect individual-level data from all minors participating in the trial.

The proposals in terms of who answers each questionnaire are as follows:

- In single-person households, the single-person household members answer the individual and household questionnaires.
- In multi-person households with only one adult, the adult should answer all three questionnaires: questionnaires for individuals, households and minors (if any).
- In multi-person households with more than one adult, if there is only one adult beneficiary of a UBI, that person should be responsible for answering the three questionnaires (same protocol as for multi-person households with only one adult). Whereas if the household has more than one adult beneficiary, the

individual questionnaire is answered by each, although only one answers the questionnaire for households (and questionnaire for minors, if any).

In terms of the latter case, the recommendation to choose the person responsible for answering is as follows: in the first contact made with the potential participants, ask them to group the persons at their address into households and to mark those persons who can provide information on household level variables (expenditure on housing, food, debts and so on) and for minors (education, health and so on). The modules for households and minors should be activated at the time of data collection only for the first person in the household with the ability to respond to these modules that would respond to the baseline.[12]

In order to maximise the response rate to the surveys, **the recommendation is to limit the duration of the survey to a maximum of 30-45 minutes** and to prioritise obtaining information through the survey on dimensions and variables of interest that:

- Cannot be obtained from administrative records either because they do not exist, because their structure makes extraction difficult or because there is no agreement with the unit owning the data to be able to access these.
- Are closely linked to the theory of change after granting a UBI; in other words, for which we have defined a hypothesis that we want to test on how receiving a UBI may affect them.
- Enable us to fill in the gaps of the existing literature.

Along the same lines of **maximising survey response rates,** we recommend:

- **To make participation in the draw conditional on its completion** in the case of the baseline. And to use behavioural methods and offer cash incentives to those who will participate in the draw in order to motivate response if this option is ruled out.
- In the case of follow-up surveys, use behavioural methods to motivate the response of people in the treatment group and complement them with cash

[12] The best way to monitor and control this standard must be discussed with the survey company, and it will depend on the channel used to administer the surveys.

incentives in the case of the control group in order to compensate for the fact that, as they are not receiving a UBI, we expect their response rate to be lower to begin with.

In terms of when information should be collected, the proposal is to conduct surveys before payments begin (baseline), one year after the first payment (first follow-up) and two years after the first payment (second follow-up). For the follow-up surveys, it is important to allow sufficient time to elapse between the start of the intervention and the data collection points so that the variables of interest can change because of the intervention. In the case of the baseline, **it is imperative that this is done before participants learn if they are part of the control or treatment group,** given that this may bias their responses. This should ideally be done even before the draw so that not only the participants but also no one else (interviewer, Office, etc.) knows to which group the person being interviewed belongs. **Ivàlua therefore recommends that every effort should be made to ensure that the baseline survey takes place before the result of the draw is known, and in order to ensure this, adjust the beginning of the pilot project's implementation if deemed necessary to make this possible.**

Qualitative interviews

There are certain aspects that quantitative methods cannot capture, but which conversely are very useful in order to understand the functioning and effects of the intervention being evaluated. **Complementarity between quantitative and qualitative methodology is therefore recommended to ensure a better understanding of the deployment and effects of a UBI.** The purpose of the qualitative analysis will be twofold. On the one hand, it will focus on collecting the perceptions of beneficiaries about how a UBI has been developed, how it has functioned, potential difficulties encountered, coordination, effectiveness and so on. These perceptions will be very useful in order to understand how a UBI has worked in practice, as this is vital to explain the results it has had. On the other hand, qualitative analysis will seek to contextualise quantitative results and capture factors that have positively or negatively influenced achieving any outcomes of interest. The aim is to gather the perceptions of key actors about the effects of a UBI and these will also be useful to evaluate whether impacts vary or are valued differently among different recipients.

Although the qualitative sample will be defined in the near future as part of the project monitoring committee, the proposal is to collect information through focus groups and

interviews with people receiving benefits and so on. **The proposal is to conduct initial fieldwork at the mid-point of the intervention and at its conclusion** in order to evaluate the effects throughout the programmes.

4.4 Synthetic trial at municipality level[13]

Objective

The objective of the synthetic trial at municipality level is to respond to the following question: "What are the aggregate effects of granting an individual UBI to each member of a community on the local economy, participation and social cohesion, and the functioning of public services in this community, as well as on behaviour, decisions and well-being of the people living there?" The specific dimensions upon which the impact is to be established are summarised in Table 1 and Table 2.

Feasibility conditions

The basic conditions for a synthetic trial to be feasible are: 1) there is one or several units receiving the treatment, in this case municipalities receiving a UBI; 2) there is a group of municipalities not receiving the treatment that together represent a good counterfactual for the treatment units, and 3) there is aggregated historical information on the variables of interest and other variables that determine these.

Candidate list

The list of candidates in this pilot project refers to the list of municipalities that are candidates to receive a UBI. In order for the study to be extrapolated to a set of municipalities of interest, as in our case, there must be two municipalities to which a UBI can be granted that together can reproduce the average evolution of the variables of interest of the representative municipalities in the period prior to the intervention.

Donor pool

The donor pool refers to the set of candidate municipalities to form part of the control group. The basic condition to be met for a synthetic trial to be feasible is that it is possible to find a combination of non-treatment units among the donor pool that can

[13] The document by Vives-i-Bastida (2022) explains the technical details of the synthetic trial.

reproduce the evolution of the variables of interest of the treatment units in the period prior to the intervention. Or in our case, that there is a combination of municipalities not receiving a UBI capable of adequately reproducing the evolution of variables such as unemployment or the use of social services of the units that will receive a UBI.

Historical and monitoring information on candidates and donors

We need to have historical information on the variables of interest for both the list of candidates and the donor pool, as well as on the variables determining these aggregated at the level of municipality, basic health area or basic social services area, as it is from this information that we can evaluate whether there are valid candidates and donors: in the first case, evaluating the ability of the candidates to reproduce the average evolution of the target municipalities and, in the second case, the ability of the donors to reproduce the evolution of the treatment units. Therefore, the further back in time the information goes and the higher the frequency, the more valuable the information that will be used to feed the model, and consequently the better the choice of both treatment and control municipalities. Furthermore, in terms of the treatment and control municipalities, we need to have aggregated information at municipality level for the outcomes upon which the impact aims to be measured for the post-intervention periods.

Below are the investigations conducted by Ivàlua during this assessment to evaluate whether these conditions were met, as well as the resulting recommendations.

Candidates: list of possible treatment municipalities

As discussed above, it is considered desirable for the synthetic trial not to treat only one municipality, but to treat two in order to protect the pilot project from the possibility of one of the treatment municipalities receiving an exogenous shock that would prevent distinguishing between the impacts caused by a UBI and those caused by the exogenous shock, and thus the ability to draw conclusions about the effects of the universality of a UBI. For the same reason, **the proposal is that the two treatment municipalities are located far apart so that they are not subject to the same local shocks.**

Therefore, based on the recommendation that the synthetic trial should have 2,500 people benefiting from a UBI, this implies that both municipalities should together have a population of approximately 2,800 inhabitants, so that there are about 2,500

people eligible to receive a UBI (potential recipients) after excluding the highest incomes and people declaring wealth tax. This means **limiting the municipalities eligible to receive a UBI to those with a population of between 1,200 and 1,400 inhabitants.**

Donor pool: list of potential control municipalities

With regards to how to define the group of municipalities from which to draw the control municipalities to set up the synthetic control trial (which in turn will determine to which group of municipalities we can extrapolate the results obtained), Ivàlua's opinion is that **priority should be given to those municipalities for which it is credible that the social and economic dynamics can be similar to those found in the treatment municipalities,** which it must be remembered have a population of between 1,200 and 1,400 inhabitants.

Two options have been considered to be able to define these municipalities:

1. Use population of municipalities
2. Use degree of urbanisation

As for the population criterion, we at Ivàlua believe that under no circumstances should the main cities of Catalonia be included in the group of potential control municipalities. Although this implies that the results obtained in the synthetic trial cannot be extrapolated to these urban centres, we believe that a study using cities such as Barcelona, Tarragona or Rubí to simulate the evolution of a municipality of 1,300 inhabitants would not be very credible. It is therefore preferable to prioritise the credibility of the study over its capacity to be extrapolated to the whole of Catalonia. Nonetheless, what remains unclear is at what value the population threshold should be set so that the synthetic control trial we design is credible and the results can be simultaneously extrapolated to a population of interest, given the fact that there is no population threshold above which the characteristics of the municipalities change radically. Three thresholds have been considered to limit the donor pool: 1) municipalities with fewer than 50,000 inhabitants; 2) municipalities with fewer than 20,000 inhabitants; and 3) municipalities with fewer than 10,000 inhabitants.[14]

[14] The threshold of 10,000 inhabitants has traditionally been used to define urban municipalities.

The other criterion that has been considered, as recommended by Idescat, is classification according to the degree of urbanisation defined by Eurostat (Eurostat 2021). This classifies municipalities into three categories according to their size and population density: 1) cities; 2) towns and intermediate density areas; and 3) rural areas. The municipalities on the candidate list correspond to categories 2 and 3. Therefore, by following this criterion, the municipalities that would be excluded are those deemed to be cities, where we expect the dynamics to be different from those in rural areas, towns and intermediate density areas.

As Table 7 indicates, the two types of criteria display a certain, although imperfect mirroring. As far as overlaps are concerned, large cities (more than 50,000 inhabitants) are excluded with any of the criteria.

Conversely, it is interesting to note that, according to the degree of urbanisation criterion (excluding cities), municipalities are excluded that would be included if some of the proposed population thresholds were used:

- Eleven municipalities with between 20,000 and 50,000 inhabitants: Esplugues de Llobregat, Gavà, Igualada, El Masnou, Montcada i Reixac, Ripollet, Sant Adrià de Besòs, Sant Feliu de Llobregat, Sant Joan Despí, Sant Quirze del Vallès and Barberà del Vallès.

- Five municipalities with between 10,000 and 20,000 inhabitants: Montgat, Sant Just Desvern, Santa Margarida de Montbui, Vilanova del Camí and Badia del Vallès.

- Three municipalities with fewer than 10,000 inhabitants: Alella, Teià and Tiana.

Although if the criterion of fewer than 20,000 inhabitants were used, 33 municipalities considered towns or intermediate density areas would be excluded, to which a further 55 would be added if the threshold were lowered to 10,000 inhabitants.[15]

[15] Towns and intermediate density areas of more than 20,000 inhabitants: Castellar del Vallès, Esparreguera, Les Franqueses del Vallès, Manlleu, Martorell, Molins de Rei, Olesa de Montserrat, Pineda de Mar, Premià de Mar, Sant Andreu de la Barca, Vilassar de Mar, Sant Pere de Ribes, Santa Perpètua de Mogoda, Sant Vicenç dels Horts, Sitges, Vic, Vilafranca del Penedès, Banyoles, Blanes, Figueres, Lloret de Mar, Olot, Palafrugell, Salt, Sant Feliu de Guíxols, Amposta, Calafell, Cambrils, Tortosa, Valls, El Vendrell, Vila-seca and Salou.

Table 7 – Mirroring of population-based criteria and criterion based on degree of urbanisation

Size of municipality	Cities	Towns & intermediate density areas	Rural areas
More than 50,000 inhabitants	23	-	-
Between 50,000 and 20,000 inhabitants	11	33	-
Between 20,000 and 10,000 inhabitants	5	51	-
Fewer than 10,000 inhabitants	3	114	707

Table 8 contains the four criteria considered together with our evaluation of their level of coverage and credibility.

Table 8 – Coverage and credibility of different sets of control municipalities

Inclusion criterion	Coverage	Credibility
Municipalities with fewer than 50,000 inhabitants	**High:** Would allow us to extrapolate the results to 924 municipalities and 46% of the population of Catalonia.	**Low:** Would include large urban centres that are unlikely to have comparable dynamics in a municipality of 1,200-1,400 inhabitants.
Municipalities with fewer than 20,000 inhabitants	**Medium:** Would allow us to extrapolate the results to 880 municipalities and 29% of the population of Catalonia.	**Medium:** Medium-sized municipalities, such as Cardedeu, Balaguer and Arenys de Mar, traditionally considered urban centres, would be included.
Municipalities with fewer than 10,000 inhabitants	**Low:** Would allow us to extrapolate the results to 824 municipalities and 18% of the population of Catalonia.	**High:** This would include municipalities traditionally considered rural or semi-urban, such as municipalities on the candidate list.
Municipalities classified as rural areas, towns or intermediate density areas	**Medium-high:** Would allow us to extrapolate the results to 905 municipalities and 40% of the population of Catalonia.	**Medium:** Only municipalities with similar degrees of urbanisation to those on the candidate list would be included, although some with considerably higher populations.

Towns and intermediate density areas of between 10,000 and 20,000 inhabitants: Abrera, Arenys de Mar, Argentona, Berga, Caldes de Montbui, Calella, Canet de Mar, Canovelles, Cardedeu, Castellbisbal, Corbera de Llobregat, Cubelles, la Garriga, La Llagosta, Llinars del Vallès, Lliçà d'Amunt, Malgrat de Mar, Montornès del Vallès, Palau-solità i Plegamans, Pallejà, Parets del Vallès, Piera, La Roca del Vallès, Sant Andreu de Llavaneres, Sant Celoni, Sant Joan de Vilatorrada, Premià de Dalt, Sant Sadurní d'Anoia, Tordera, Torelló, Vallirana, La Bisbal d'Empordà, Calonge i Sant Antoni, Cassà de la Selva, Castelló d'Empúries, Castell-Platja d'Aro, L'Escala, Palamós, Ripoll, Roses, Santa Coloma de Farners, Torroella de Montgrí, Balaguer, Mollerussa, La Seu d'Urgell, Tàrrega, Cunit, Mont-roig del Camp, Sant Carles de la Ràpita, Torredembarra and Deltebre.

In light of the information presented, **the recommendation by Ivàlua is to use the degree of urbanisation criterion as the main criterion for inclusion,** and this could be complemented, if the Office deemed it appropriate in light of the information presented, with an additional criterion of a maximum number of inhabitants that would set this threshold at: either 20,000 inhabitants, a size above which municipalities have their own basic service areas, or 10,000 inhabitants, a threshold that has traditionally been used to distinguish between urban and non-urban municipalities.

Model for choosing treatment and control municipalities

In order to choose the treatment and control municipalities, **the recommendation is to use the methodology proposed by Abadie & Zhao (2021),** which selects them in such a way that:

1. The combination of the two municipalities chosen to receive a UBI is as representative as possible of the group of municipalities to which we want to extrapolate the results of the study, viewed as being able to reproduce the average evolution of this group of municipalities for a series of variables of interest in the period prior to the intervention.

2. The combination of the control municipalities provides a good counterfactual for the treatment municipalities, viewed as being able to reproduce the average evolution of the combination of the two treatment municipalities for a number of variables of interest in the period prior to the intervention.

And based on this methodology and the support of an expert reference person in the synthetic trial, a new synthetic control model has been developed to evaluate the effect of a UBI not only in one outcome, as has traditionally been done with this methodology, but in a number of outcomes covering the main dimensions of interest presented in Table 1 and Table 2.

In terms of the specific characteristics of the model to be used, it is proposed that:

- **The average of the donor pool to be approximated should not be weighted by population,** given that the eligibility criteria will have already reduced the variability of this variable and thus the importance of taking it into account.

- **That a penalised model** be applied in order to reduce the number of municipalities in the donor pool that end up in the control group, as this will help to facilitate the impact identification strategy and the interpretation of the results deriving from it, as well as facilitating data collection in the control municipalities.

- In addition to information on the main outcome, **information on determinants of other outcomes of interest should also be used,** as this can help to make the synthetic control trial suitable for a wider set of outcomes.

Information gathering

Administrative data

Apart from the administrative records proposed to be used at the individual level in the randomised trial, the recommendation in the case of the synthetic trial is also to explore the options of accessing other administrative data related to the dimensions of interest upon which the impact is to be measured at the level of municipality, basic health area (BHA) and basic social services area (BSSA). These include: data on the functioning of health services, data on the functioning of social services, absenteeism and dropout rates, unemployment, average income, income inequality, crime and municipal conflicts, electoral participation rates, job seekers and so on.

As far as possible, **the recommendation is that these data should be obtained not only for periods following the intervention, but also for as many earlier periods as possible.** This is because the choice of treatment municipalities and construction of the synthetic control will be made with the information that the Office of the Pilot Project has available at the moment. It will therefore not take into account all the outcome variables on which the impact of giving a UBI to all the people in a municipality is to be measured later.[16] So in order to assess the suitability of the synthetic control constructed with the subset of information available to simulate changes in these other outcomes, it will be important to assess the similarity of these

[16] Calibration of the model is being done by using as the main outcome to optimise the unemployment rate in the municipality and as covariates the average total population, territorial socio-economic index, percentage of foreign population, Gini index, average net income per person, average age of the population, percentage of population under 18, percentage of population aged 65 and over, average household size and percentage of single-person households.

outcomes between the treatment municipalities and the synthetic control in the periods prior to the intervention.

Finally, the higher the frequency of the data, the better, as this will help to obtain a more valuable snapshot of the evolution of the variables of interest in the post-intervention periods. It is therefore preferable to obtain monthly or quarterly data rather than annual data, as we would only have two observation points for the post-intervention period in the latter.

Surveys

To complement the data that can be obtained from administrative records, **the proposal is to survey a representative sample of treatment municipalities and synthetic control municipalities** by using the same survey and procedure as in the randomised trial.

In the case of the synthetic control, surveying a representative sample of the municipalities belonging to the study is important in order to understand the mechanisms that explain the aggregate-level effects that we can observe through the administrative data. At the same time, inasmuch as we can compare the individual effects we find in the synthetic trial and in the trial, **this will help us to explore which effects stem directly from receiving an income and which from spillovers and general equilibrium effects,** a question directly related to the universal characteristic of the UBI that we are aiming to study through this pilot project.

It is in any case worth noting that it will not be necessary to interview all the people registered in the municipalities to be studied. Table 9 shows the number of surveys that would be necessary within the framework of the synthetic trial, taking into account that there will be two treatment municipalities of about 1,300 inhabitants each, in addition to a reduced group of about five municipalities, the average size of which will depend on the criteria used to include municipalities in the donor pool.

Table 9 – Surveys required according to inclusion criterion[17]

Inclusion criterion of municipalities	Average size of municipalities	Total surveys needed
Municipalities with fewer than 50,000 inhabitants	3,878	2,341
Municipalities with fewer than 20,000 inhabitants	2,514	2,259
Municipalities with fewer than 10,000 inhabitants	1,713	2,162
Municipalities classified as rural areas, towns or intermediate density areas	3,409	2,319

Qualitative interviews

In order to evaluate the effects of a synthetic trial UBI at municipality level, **the proposal is also to combine qualitative and quantitative methodology.** Qualitative analysis will serve two general purposes. On the one hand, as in the case of the randomised trial, it will seek to collect perceptions and assessments of how the pilot project and its effects work in practice. Interviews and focus groups with recipients of the benefit have consequently been planned. On the other hand, specifically in the case of the synthetic control, we will seek to gather perceptions on the meso and macro effects of a UBI. The recommendation for the fieldwork is therefore also to conduct interviews with key local agents and local entities or institutions somehow involved with the pilot project, as well as focus groups.

5. Procedural recommendations

Samples to be requested from Idescat

In terms of the randomised trial, it is important to ensure that the sample reaches a minimum of 5,000 participants (2,500 treatment and 2,500 control), because as has been explained on several occasions, this will help us to acquire suitable statistical power. One important aspect to take into account is therefore that the sample of potential recipients is large enough to ensure that this sample size is reached once all

[17] Required sample size = $\frac{\frac{z^2 p(1-p)}{e^2}}{1+(\frac{z^2 p(1-p)}{e^2 N})}$. Sample size calculations are made assuming a margin of error of 5% (e = 0.05), a confidence level of 95% (z = 1.96) and under the criterion of maximum uncertainty (p= 0.5). The population (N) changes for each of the scenarios according to the average size of the municipalities included.

the required communications and checks have been made. It should be borne in mind for this question that the initial sample will diminish over the course of the process because:

- Not all persons will receive or open the initial communication letter.
- Not everyone opening it will accept the invitation, provide informed consent and request to participate in the pilot project.
- Not all persons applying will be eligible to receive it: a percentage of potential participants will be excluded because they do not meet the income and/or wealth requirements.
- Not all eligible persons will respond to the baseline survey.
- Not all persons assigned to the treatment group will eventually apply for a UBI.

Given the high incentive to participate in the pilot project (receiving a UBI for 24 months), it is likely that non-take-up will be moderate and reduced at each stage. This should therefore be kept in mind when requesting the initial sample, given that there are several points at which the sample may be depleted.

Assuming that answering the survey is a necessary condition for entering the draw, Table 10 contains a simulation of how the initial sample would be depleted in a pessimistic scenario in which only half of the people receive and open the letter, of which 75% complete the application to participate in the pilot project, 5% are rejected as ineligible, and 85% of the people with accepted applications complete the baseline survey.

Table 10 – Pessimistic scenario of depletion of initial sample during application process

Initial sample (in persons)	Letter opening rate (50%)	Application rate (75%)	Eligibility rate (95%)	Survey response rate (85%)
16,512	8,256	6,192	5,882	5,000

In line with Table 10, and given that each address has an average of approximately two and a half people living in it, **it is recommended that a sample of no fewer than 6,600 addresses** be requested from Idescat. If the costs of extraction by Idescat do not vary greatly depending on the size of the sample, it would be worth increasing this as much

as possible to ensure that it is not necessary to request an additional sample at a later date if the initial sample does not reach 5,000 participants, given that this would delay the entire process.

As far as the synthetic trial is concerned, we will have to wait until we have the treatment and control municipalities to calculate the size of the sample to be needed for each municipality. In any case, according to the calculations presented in Table 8, **the expectation is that between 300 and 400 people per municipality will need to be surveyed; in other words, between 150 and 200 households per municipality.** In this case, the only relevant depletion variable to consider is the response rate to the survey, but non-response in this case is expected to be much higher as it will in no case be linked to participation in the pilot project. **The recommendation is therefore to ask for one or two substitute addresses for each of the addresses to cover possible non-responses.**

Sending invitations to participate in randomised trial

Linked to the previous point, the challenge exists of deciding how to organise the sending of invitation letters to the randomised trial in order to reach at least 5,000 participants in the draw in the most efficient way possible; in other words, with the minimum amount of time and deviating as little as possible from this figure.

With regard to this question, we at Ivàlua believe that **the priority is not to limit the number of participants because, as has been explained several times, this affects the statistical power of the trial.** While a situation in which we have more than 5,000 people entering the draw is not problematic, for this will simply lead to an increase in the control group, thus not having any implication on the budget of the pilot project, it will conversely help to increase the statistical power of the trial.

Therefore, taking into account that we will have approximately 16,500 people (according to Table 10 above), **we suggest that the initial mailing of invitations should include 10,000 individual invitations and a reserve list of 6,500 people. The initial sample can alternatively be smaller (e.g. 8,000 people), with a larger reserve list** (e.g. 8,500 people).

In either case, the proposal is to carry out a second round of invitation mailings if the figure of 5,000 participants is not reached with the first mailing, and then adjust the

number of invitations according to what is needed to reach the target number of participants based on actual depletion data from the first round.

Conditioning participation in the draw on completion of baseline survey

As mentioned above, from an evaluation perspective, **Ivàlua considers it crucial to make participation in the draw conditional on having responded to the baseline survey.** The main reasons for this are set out below.

In order to be able to measure the impact that a UBI has had on recipients, it is essential to have follow-up information on all the people who have participated in the pilot project in both the treatment and control groups. **Having people who are part of the pilot project but do not respond to the surveys is therefore a problem, as they cannot be part of the evaluation sample.** As shown in Table 3 and Table 4, this has major implications in terms of statistical power; for example, going from a total sample of 4,250 people to a sample of 2,125 increases the minimum detectable effect on the probability of working by 5.5 percentage points, the minimum detectable effect on the percentage of people with unmet medical needs by 30.3 percentage points, or the minimum detectable effect on the percentage of households experiencing material deprivation by 23.7 percentage points.

Ivàlua believes that this situation of sample depletion (participants not responding to follow-up questionnaires) is more likely to occur in people who have not responded to the baseline survey than among those who have. Making participation in the pilot project conditional on having responded to the survey will therefore help to reduce depletion and consequently increase the statistical power of the trial.

In addition, having information from the baseline survey also helps to increase the precision of the estimates made for the post-intervention periods, as the baseline value of a variable is a good predictor of the future value of the same variable, and including this in the model will help us to reduce the unexplained variation of the variable of interest. But this is only possible for those persons and variables for which we have both baseline and follow-up information.

Include authorisation to access registry data in request for participation

For the same reasons stated in the previous point, Ivàlua recommends that authorisation to access the administrative data of the participants be included as part

of the application form. The reason for this is that it will allow us to obtain information on all the people participating in the pilot project, thereby maximising both the statistical power of the study and its internal validity, at least for the variables of interest that are constructed from administrative data.

Baseline survey in synthetic trial municipalities

With regard to the baseline survey in the synthetic trial municipalities, the recommendation is that it should be done as close to the announcement of the treatment municipalities as possible. In an ideal scenario, this would be done even before people in the treatment municipalities learn that they will be beneficiaries of a UBI to avoid it capturing anticipation effects; in other words, changes in people's behaviour resulting from the fact that they know that in a few weeks or months they will start receiving a UBI. Nonetheless, realising that this is most likely logistically unfeasible, the recommendation would be to do so as close to the public announcement as possible so as to minimise these anticipation effects.

6. Summary of recommendations

The table below provides a summary of the recommendations made, as well as information on whether the Office of the Pilot Project has agreed to follow these (at the date of publication of this report). It also provides an explanation of the reasons that have been communicated to Ivàlua for not following the criteria set out.

Table 11 – Summary of recommendations made by Ivàlua for Office of the Pilot Project

Recommendations	Recommendation followed by Office
Intervention recommendations	
Level of intervention:	
1. The recommendation is to allocate a UBI on an individual basis irrespective of cohabitation status of targeted persons.	✓
Eligibility criteria:	
2. *Residence criterion:* The recommendation is that the residence criterion should be registration as a resident in Catalonia.	✓
3. *Income and wealth criterion:* The recommendation is not to apply any exclusion criteria related to income and wealth in the pilot project.	✗[18]
4. *Other:* The recommendation is that children born in UBI beneficiary families during the pilot project should be assigned a UBI that corresponds to them	✗[19]
Recommendations on evaluation methodology	
Design chosen:	
5. The proposal is to evaluate the impact of a UBI through a dual pilot project involving a randomised controlled trial (RCT) at registered address level and a synthetic trial at municipality level.	✓
6. The recommendation is to distribute the sample of 5,000 persons evenly between the two pilot projects, with 2,500 in treatment group and 2,500 in control group in each pilot project.	✓
Randomised controlled trial	

[18] The Office of the Pilot Scheme deems it appropriate to apply a maximum income and wealth threshold as a requirement for participating in the pilot project. It has been set at 10% of the highest income earners and/or those who have had to declare wealth tax. This threshold is high enough to ensure the near universality of the intervention, and the Office has deemed it necessary because in the existing simulations of financing a UBI in Catalonia, people above this threshold would not receive the cash allocation of the public policy in a clean manner, so it would therefore not be logical for them to receive it either in a pilot project.

[19] The Office of the Pilot Scheme deems it appropriate not to include children born in UBI beneficiary families during the pilot project because births represent a non-controllable variable during its implementation, thereby making it difficult to control the pilot project's budget.

Recommendations	Recommendation followed by Office
7. *Feasibility conditions:* It is vital that the allocation is randomised and the resulting groups are respected throughout the pilot project. It is similarly imperative that the baseline survey is completed before participants are informed about if they are part of the control or treatment group.	✓
8. *Level of randomisation:* The recommendation is to randomise at household level (not at individual level) so that the intervention is diluted as little as possible at household level.	✓
9. *Data collection:* The recommendation is for complementarity between quantitative and qualitative methodology. With regard to quantitative methodology, we recommend prioritising the collection of data through administrative records and complementing this with surveys of all persons over the age of sixteen participating in the pilot project.	✓
Synthetic trial	
10. *Candidates, list of possible treatment municipalities:* The recommendation is that the two treatment municipalities should be located far apart from each other so that they are not subject to the same local shocks.	✓
11. *Donor pool, list of potential control municipalities:* The recommendation is to prioritise those municipalities for which it is credible that the social and economic dynamics can be similar to those in the treatment municipalities. The recommendation is therefore to use the criterion of degree of urbanisation as the main inclusion criterion.	✓
12. *Model for choosing treatment and control municipalities (I):* The recommendation is to do so according to the synthetic trial methodology.	✓
13. *Model for choosing treatment and control municipalities (II):* The recommendation is that the mean of the donor pool to be approximated should be a non-population weighted mean.	✓
14. *Model for choosing treatment and control municipalities (III):* The recommendation is that a penalised model should be applied to reduce the number of municipalities in the donor pool ending up in the control group.	✓
15. *Model for choosing treatment and control municipalities (IV):* The recommendation is that in addition to information on main outcome, information on determinants of other outcomes of interest should also be used.	✓

Recommendations	Recommendation followed by Office
16. *Data collection to build the model:* The recommendation is to collect data for as many periods prior to the intervention as possible.	✓
17. *Collection of information by the evaluation (I):* The proposal is to survey a representative sample of treatment municipalities and municipalities that make up the synthetic control.	✓
18. *Collection of information by the evaluation (II):* The recommendation is to combine qualitative and quantitative methodology	✓
Procedural recommendations	
Randomised controlled trial	
19. *Samples to request from Idescat:* The recommendation is that a sample of no fewer than 6,600 registered addresses should be requested from Idescat.	✓
20. *Maximise number of participants to approach the sample of 5,000 participants:* The recommendation is that the initial mailing of invitations plus the reservation list should include 16,500 people.	✓
21. *Maximise survey response rates:* The recommendation is that participation in the draw should be conditional on completion of baseline survey.	✗ [20]
22. *Maximise available information:* The recommendation is that authorisation to access administrative data of participants should be included as part of the application.	✓
Synthetic trial	
23. Samples to be requested from Idescat: The recommendation is that between 300-400 persons per municipality (around 150-200 addresses) are	✓

[20] The Office of the Pilot Scheme deems it appropriate not to make participation in the draw conditional on answering the baseline survey, as this could potentially exclude people from the draw who do not usually answer surveys or do not have sufficient digital skills to do so. Given that we did not want to limit the willingness to participate and also wanted to avoid certain profiles having a lower probability of participating, it was decided not to make entry conditional on more than the fulfilment of the participation criteria. Not making participation in the prize draw conditional on answering the survey also makes it possible to argue that the personal data collected by the survey are provided by the participants in a completely free, voluntary manner, thereby also promoting their veracity. Although there is no legal obligation to complete the survey, doing so otherwise would have been a *sine qua non* condition for receiving a UBI.

Recommendations	Recommendation followed by Office
surveyed and that one or two substitute addresses are requested for each of these to cover possible non-response.	
24. *Maximise available information:* The recommendation is that the authorisation to access administrative data of participants should be included as part of the application form.	✓
25. *Feasibility conditions:* The recommendation is that the baseline survey should be conducted as close as possible to the announcement of the treatment municipalities.	✓

7. Conclusions

This document has presented in summary form **the main recommendations that Ivàlua made to the Office of the Pilot Project** to Implement Universal Basic Income as part of the assessment framework to ensure that the pilot project to be implemented provides useful, thorough information on the effects at an individual and aggregate level of implementing a UBI in Catalonia.

The methodological design proposed in this paper has the virtue of **allowing conclusions to be drawn on the effects of a UBI on individual- and household-level decisions for Catalonia as a whole, while also allowing the collective effects of universality to be tested in two municipalities that are representative of rural municipalities,** towns or intermediate density areas of **Catalonia.** On the other hand, it does not allow us to ascertain what the aggregate effects of implementing a UBI in Catalonia as a whole would be, or to simulate the effects that would arise from tax reform and changes in the current system of benefits that would have to be made to implement a UBI in Catalonia as a whole.

The recommendations contained herein have been used by the Office of the Pilot Project, which is the final decision-maker, to **adapt its design and ensure that its impact can be rigorously assessed.** There has been agreement between the Office's judgement and that of the evaluation team in most cases, and the recommendations have been incorporated. On other occasions, there have been logistical, bureaucratic or political reasons that have led to preferred options from an evaluation perspective being discarded in favour of other preferred options from an implementation perspective.

Ivàlua's conclusions are as follows: first, **there has been agreement with the Office to follow up on most recommendations** for an evaluable design; second, the **recommendations that have not been followed up do not generally pose a high risk for evaluating the impact of the pilot project;** and third, **the following risks to the evaluability of the pilot are highlighted below:**

- *In the case of the uncontrolled randomised trial:* not having enough respondents to the survey because of the decision not to make participation in the draw conditional on completion of the baseline survey (recommendation 21). This

would imply less statistical power and the potential risk of not being able to perform some analyses.

To lessen this limitation, the Office has put in place measures such as providing a financial incentive to those groups that have less incentive to participate in the survey (control groups) in both the randomised trial and the synthetic control saturation trial.

- *In the case of the synthetic trial:* the lack or poor quality of necessary registry data. In order to reduce this risk, the Office of the Pilot Project has already started to work on the collection of registry data from the various official sources of information available to evaluate their quality and select those that meet robustness criteria suitable for the impact assessment of the pilot project.

Finally, it should be noted that the **recommendations presented are based on the information available at the time of writing the report.** This information comes primarily from the review of academic literature on a UBI, the knowledge of experts involved in designing the pilot project and investigations into what and how it is possible to carry out and implement a UBI pilot project. In this regard, the arrival of new relevant information may lead to Ivàlua's position to change in some of the aspects presented.

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