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degree of Master of Science in Politieke wetenschappen

Political Lessons from the Frontline: The Causal impact of Policy Design on External Political Efficacy among socioeconomically vulnerable citizens

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1. INTRODUCTION

The extent to which political institutions respond to the interests of the represented constitutes a normative standard for assessing democratic quality (Celis, 2012; Pitkin, 1967). In an ideal-type democracy, inclusive political participation ensures that citizens help shape public policy and hold representatives accountable, thereby securing a degree of substantive representation. However, when political participation is structurally skewed along socioeconomic dimensions (Brady et al., 1995; Marx & Nguyen, 2016; Stolle & Hooghe, 2011), a precarious downward spiral may emerge. As socioeconomically vulnerable groups, characterized by lower income, lower education levels, or precarious labor market status, are more absent from the political arena, policy outcomes are less likely to reflect their specific needs (Bovens & Wille, 2010; Traber et al., 2022). This lack of responsiveness may, in turn, reinforce their political marginalization.

This representational deficit in measures of substantive representation, where elected officials act in a less responsive manner to the needs of vulnerable citizens, can be investigated through their reflection in individuals' subjective attitudes of political efficacy; more specifically, external political efficacy (EPE), which reports citizen's subjective feelings regarding the responsiveness of government to 'people like them' (Hayes & Bean, 1993). A robust sense of EPE can serve as a motivational bridge, bolstering political engagement even among those with fewer material resources. Conversely, its erosion can contribute to political withdrawal. Crucially, for vulnerable groups, low levels of EPE often represent a realistic assessment of a system they perceive as unresponsive. This necessitates an investigation into alternative sources of EPE within vulnerable groups (Shore, 2020).

Policy feedback theory offers a compelling framework for this investigation, positing that "policies, once enacted, restructure political processes" (Skocpol, 1992, p. 58). Rather than viewing political attitudes as the result of factors outside the political system, this perspective provides a theoretical basis for investigating these attitudes and behaviors partly as products of the political system's outputs (policy) (Kumlin & Rothstein, 2005). More specifically, for socioeconomically vulnerable citizens, frequent interactions with the welfare state bureaucracy (e.g., labor market mediation, social housing, social assistance) often constitute their most frequent and meaningful contact with the state, far more than the ballot box or elected officials (Soss, 1999). Through their design and administration, these policies serve as primary sites where citizens learn about their political standing and assess the state's responsiveness (Kumlin & Stadelmann-Steffen, 2020; Soss & Schram, 2007). Therefore, how these policies are designed is not merely a matter of bureaucratic efficiency; it can be a determinant of democratic inclusion.

Existing research has demonstrated that policy design can politically empower or disengage citizens (Campbell, 2012; Mettler & Sorelle, 2018; Soss, 1999). Universalistic programs, which deliver benefits with broad eligibility rules and high automaticity in benefit delivery, tend to bolster EPE (Kumlin & Haugsgjerd, 2017a; Soss, 1999). In contrast, means-tested programs, with restricted eligibility and where applicants have to “prove” their need, tend to be more stigmatizing and have a depleting effect on EPE (Bruch et al., 2010; Schneider & Ingram, 1993; Watson, 2015). The underlying mechanism suggests the same: the design and administration of policies can shape how individuals assess the responsiveness of government agencies, and citizens generalize these assessments of social policy as a proxy for the entire political system, which can be captured by EPE.

Despite these insights, current empirical evidence faces some methodological hurdles. First, most existing research relies on cross-sectional or qualitative data (e.g., Bruch et al., 2010; Kumlin, 2004; Shore & Tosun, 2019; Soss, 1999; Watson, 2015), leaving the question of selection bias unresolved. Does political alienation precede the receipt of means-tested benefits, or do specific elements within the policy design actively structure and generate this alienation? Second, because policy design structures rules and procedures for administering the policy, it is difficult to disentangle the effect of policy design from the impact of actual administrative experience.

To answer these questions, this research employs a randomized survey vignette experiment among a group of socioeconomically vulnerable citizens ($N=126$). Participants were randomly assigned to one of two treatment groups or a control group and exposed to a news vignette describing a proposed reform of the Brussels Rent Subsidy framed as either a universal or means-tested policy design. This allows us to isolate the direct effect of policy design on EPE.

Furthermore, this design allows us to investigate the conditional nature of policy feedback by testing the moderating effect of current experiences with welfare agencies, measured by their ‘perceived voice opportunities’. This allows us to determine whether the policy design acts as a source of stigma and affects EPE, and whether its impact depends on evaluations of one’s past interactions with agencies. Consequently, this research asks: *“To what extent does the social policy design, through universal or means-tested frameworks, shape the external political efficacy of socioeconomically vulnerable citizens?”*

The remainder of this paper is organized as follows. First, we conceptualize EPE and review the policy feedback literature. We then combine these insights to unpack the causal mechanism by which policy affects EPE, followed by a description of our experimental methodology and a discussion of the results.

2. LITERATURE

Disparities in External Political Efficacy

Recognizing the problem of inequalities in political participation, governments possess various tools to enhance political inclusiveness, most directly through the redistribution of material and human capital. By investing in education, income support, and skill development, the state attempts to ameliorate the structural inequalities that explain political participation disparities via the resource side, which posits that political engagement is, among others, a function of having the time, money, and civic skills to engage in political activities (Brady et al., 1995). While addressing these inequalities is essential, structural barriers remain difficult to shift, and the resource model alone cannot fully explain the wide variation in political engagement.

Disparities in psychological factors, such as political efficacy, are equally important for explaining variation in participation. The earliest description of political efficacy can be found in the work of Campbell et al. (1954, p. 187), where it is defined as “the feeling that individual political action does have, or can have, an impact on the political process, i.e., that it is worthwhile to perform one’s civic duties. It is the feeling that political and social change is possible, and that the individual citizen can play a part in bringing about this change.” Subsequent research has clarified that this concept must be approached through two distinct attitudinal dimensions (Hayes & Bean, 1993). Internal Political Efficacy (IPE) refers to a citizen’s subjective perception of their own competence to understand and effectively participate in politics. External Political Efficacy (EPE) concerns beliefs and perceptions about the responsiveness of governments and institutions to the demands of citizens like them. Taken together, these dimensions represent an individual’s perception of political (dis)empowerment and serve as key indicators of the legitimacy and health of democratic systems through their influence on political behavior and attitudes like participation (De Moor, 2016; Oser et al., 2023; Pollock, 1983), political trust and interest (Bienstman et al., 2024; Craig et al., 1990; Wolak, 2018) and overall satisfaction with democracy (DeHoog et al., 1990). Moreover, focusing on political efficacy can shed clearer light on, for example, how socioeconomic status shapes political involvement and on how it captures variation within groups (Alwin & Krosnick, 1991; Brady et al., 1995; Marx & Nguyen, 2016; Neundorff & Smets, 2017). Consequently, to counter political alienation, governments must address not only the objective resources of citizens but also their subjective perceptions¹ of the state.

¹ While EPE is conceptualized and operationalized as a subjective evaluation of institutional responsiveness, it can be argued that these perceptions may also reflect an objective reality for socioeconomically vulnerable people.

Lower socioeconomic status is consistently correlated with lower efficacy in both dimensions. Similar to patterns of political participation, these disparities are often explained by resources such as income and education (Etzet, 2023; Norris, 2015), age (Hayes & Bean, 1993), structural inequalities like gender, specifically for IPE (Matthieu et al., 2024), or unequal representation in these socio-demographic characteristics (Oser et al., 2023). These two dimensions also have distinct dynamics. As Pollock's (1983) work suggests, it is theoretically possible for individuals who doubt their own political competence (low IPE) to exhibit high levels of democratic participation if they believe that governments are responsive to their needs (high EPE). Conversely, welfare beneficiaries may feel highly competent in navigating complex institutional rules and political environments (high personal efficacy²) yet still perceive the government as unresponsive to their needs (low EPE). In this way, isolating EPE as the dependent variable can tell us more about the state's output structures (De Moor, 2016). For socioeconomically vulnerable citizens, the abstract 'government' is often inferred through concrete encounters with the welfare state policies (Bruch et al., 2010; Soss, 1999; Watson, 2015). By focusing on EPE as a dynamic evaluation of institutional responsiveness, we can investigate how specific features emanating from policy design and implementation serve as sources of meaning and political learning for citizens who are otherwise structurally sidelined (Mettler & Sorelle, 2018). To this end, we must first examine how social policy influences EPE.

Social Policy Feedback Effects

While public social spending in Western European countries profoundly influences citizens' daily lives (e.g., in Belgium, 29% of GDP in 2022)³, the political consequences of these policies are often overlooked. On the one hand, policy studies primarily focus on socioeconomic outcomes; on the other hand, the study of mass political behavior often views policy merely as a distant output within a systems theory framework, typically referring to Easton's (1957) model⁴. However, as Mettler & Soss (2004, p. 56) argue, political behavior cannot be adequately explained "without attention to how it is influenced by public policy; and public policy cannot be adequately analyzed apart from its effects on mass opinion and behavior." To understand why some citizens actively participate in

² In his paper, Soss (1999) examines, among other things, the personal efficacy of welfare beneficiaries. Given the proximity of personal efficacy to internal political efficacy (Bandura, 1982; Matthieu et al., 2024), it can be assumed here that this also partially translates into internal political efficacy."

³ Source: OECD, <https://www.compareyourcountry.org/social-expenditure>

⁴ Because Easton's (1957, p. 384) diagram also features a "Feedback" arrow running from the output side to the input side, it could be argued that the foundation of policy feedback lies here; however, in the remainder of the article, this specific part of the model is no longer mentioned or explained.

society and politics while others do not, the effects of policy must also be integrated into the analysis.

Large volumes have already been written on various domains within policy feedback, focusing on, for example, welfare state-related attitudes or electoral behavior (Kumlin & Stadelmann-Steffen, 2020); how policy differences affect interactions between interest groups and policymakers (e.g. Wilson, 1980)⁵; the extent to which public policies influence the attitudes, preferences, and behavior of mass publics (e.g. Piven & Cloward, 2012⁶); and how public policies impact inclusivity in citizenship by creating different classes of citizenship where some possess certain social rights and others do not (e.g. Quadagno, 1996)⁷. For this study, we limit ourselves to the effects of social policy on political involvement and participation. Verba et al. (1995) already observed that beneficiaries of means-tested programs (e.g. Aid to Families with Dependent Children) exhibit less participation than those of universal programs (e.g. Social Security). While they initially attributed this gap to the socioeconomic characteristics of the participants, subsequent empirical research has refuted this explanation (Mettler & Stonecash, 2008; Soss, 1999). The finding that, after controlling for socioeconomic and demographic characteristics, the group receiving a means-tested benefit exhibits lower participation than the group receiving a universal benefit suggests that the social programs themselves must be scrutinized.

When looking more specifically at a key precursor to political participation, namely political efficacy, we see that the effects of social policy are broadly categorized into resource and interpretive effects, analogous to the classification proposed by Pierson (1993). By redistributing material resources (primarily financial), social policy can enhance individuals' capacities to participate. Furthermore, these resources can create incentives for policy beneficiaries to participate due to their personal stake and the importance of these resources in their lives (Campbell, 2003; Mettler & Sorelle, 2018); consequently, closely monitoring and engaging with the political environment in which these policies develop can influence, in particular, their IPE (Shore, 2020). However, even when groups with similar characteristics both receive specific resources in the form of financial benefits (which are also comparable in size), variation in political participation is nonetheless observed;

⁵ Wilson, (1980) demonstrates that the distribution or concentration of policy costs and benefits can influence the political activation of interest groups. In cases where a policy concentrates benefits within a relatively small group (e.g., industry) while costs are widely dispersed among all taxpayers, there is a greater incentive for the industry lobby to remain organized and lobby for the policy than for the taxpayer who, because the cost is spread so widely, feels little impact.

⁶ Piven & Cloward, (2012), argue that, historically, by expanding public social provisions during periods of social instability and shrinking them during stability, the primary objective is to maintain social and economic order.

⁷ Quadagno (1996) argues that the historical design of social policy and welfare programs in the United States created racially stratified citizenship.

including lower voter turnout, political interest, political trust, and campaign involvement among groups receiving a means-tested benefit compared to those receiving a universal benefit. This indicates that these respective programs also generate distinct signals that are interpreted differently (Hirvonen et al., 2025; Mettler & Stonecash, 2008; Soss, 1999).

In policy design⁸, research often focuses on how well these formal criteria convey messages of deservingness, where beneficiaries perceive these social programs or assistance as an earned entitlement or right, something that can be applied for without stigma (as in the case of universal programs). Conversely, through these formal criteria, a social stigma may be attached to the use of a social benefit or program; this stigma can lead beneficiaries to associate program participation with a degree of personal failure (Mettler & Stonecash, 2008; Schneider & Ingram, 1993; Shore, 2020; Watson, 2015). In the case of means-tested programs, the stigma attached is often traced to the negative social construction of the target group for which the benefits are intended. As the typology⁹ of Ingram et al., (2007, p.102) demonstrates, policy design is rarely neutral; it reflects the degree of political power and the social image of the beneficiaries. For positively constructed groups with significant political capital, labeled as "Advantaged" (e.g., 'Homeowners' or 'Small business'), policy design tends to be universalistic. Because these groups are viewed as "deserving" and "productive," the state facilitates their access to resources through trust-based mechanisms that minimize administrative burden and avoid stigma. Conversely, for groups that are negatively depicted and possess far less political power, labeled as "Dependents" (e.g., 'The Poor', 'Homeless' or 'Welfare Mothers'), policy design is significantly more likely to be means-tested.

Thus, social policy can send signals that convey information to beneficiaries about their own status in society and, equally important, about the responsiveness of governments. External Political Efficacy can therefore be considered a central interpretive effect. These signals can be interpreted as measures of a government's responsiveness to people like them and of their own political standing in society.

⁸ Broadly speaking, a technical distinction is made in social policy between means-tested (as mentioned earlier) and non-means-tested. Within non-means-tested benefits—those that do not involve income or asset eligibility criteria—a further distinction is made between universal programs (e.g., child benefits, where every child is entitled to the same amount; or in the case of various European welfare states, parts of health insurance) and contributory programs (e.g., unemployment benefits or pensions based on prior rights). In contrast, means-tested benefits (e.g., a living wage/social assistance [leefloon] tested against individual or familial resources) are dependent on such criteria.

⁹ The framework identifies four types, each representing an ideal-typical extreme within a quadrant defined by two variables: political power (high vs. low) and social construction (positive vs. negative). These four categories (Advantaged, Dependents, Deviants, and Contenders) serve as analytical benchmarks. However, because these types represent extremes, many groups do not fit neatly into a single category. For instance, the group 'the disabled' often possesses moderate political power and is subject to social constructions that are neither purely positive nor negative. Furthermore Ingram et al., (2007), argue that a degree of path dependency exists in policy: groups that have been negatively constructed by historical policies often face subsequent policy designs that reinforce and perpetuate these negative constructions.

Policy design signals: The Causal Path from Rules & Procedures to EPE

To understand the causal path from policy design to EPE, it is necessary to identify the specific features of the design that generate interpretive signals. In line with the policy feedback framework, this study argues that the design of a policy provides signals through which citizens learn about their standing in the eyes of the state and assess the state's responsiveness. Two dimensions of policy design are particularly salient in this process, the terms of eligibility and the degree of automaticity (Campbell, 2012; Mettler & Sorelle, 2018).

In a universalistic design, eligibility is typically predicated on broad categories, such as residency or membership in a broad socioeconomic group, rather than individual-level scrutiny. This conveys a signal of social rights and inclusion where the state acknowledges the citizen's needs as a shared social responsibility. In contrast, means-tested designs, where eligibility is contingent upon strict income thresholds and a rigorous 'proof of need', function as a signal of suspicion and conditionality. Such designs cast the beneficiary as a 'suspect claimant' rather than a rights-bearing citizen. This perception is further reinforced by the degree of automaticity. Universal designs are often characterized by high automaticity, where benefits are automatically and often proactively conveyed, signaling institutional trust. Means-tested designs, by definition, have low automaticity; they impose a high administrative burden, signaling state surveillance and control rather than a responsive institution. Furthermore, the signals delivered by policy design are not interpreted uniformly across the population. Unlike the general public, socioeconomically vulnerable people, who are often the target population of social policy, possess a unique experiential knowledge. They have the capacity to decipher what formal rules, such as rigorous eligibility, mean in practice. They can understand that a lack of automaticity results in stigmatizing administrative hurdles, possible behavioral requirements, and sanctions or punitive measures. This leads to the following hypothesis:

H1a: Participants exposed to a means-tested design, with high eligibility and low automaticity, will report lower levels of EPE than the control group.

H1b: Participants exposed to a universal design, with low eligibility and high automaticity, will report higher levels of EPE than the control group.

Finally, the interpretive signals of policy design are often filtered through a citizen's past experiences with administrative interactions related to social policy (Soss, 1999). As the participants in this research all possess prior experience with welfare agency counselors,

either through Public Centers for Social Welfare¹⁰ or Public Employment Services¹¹, these interactions can serve as a moderating lens. Following Kumlin (2004) and others (e.g. Bruch et al., 2010; Shore & Tosun, 2019), direct welfare state experiences can be viewed most valuable through the procedural fairness perspective, where the quality of the interaction process between citizens and institutions is more influential than a material outcome. Empirically the procedural fairness perspective is robustly underpinned with empirical evidence which shows when an allocation process of benefits is perceived as fair, this can have an impact on general attitudes towards the political system and governments (Kumlin, 2004; Tyler, 1988, 1996). Perceptions of procedural fairness therefore can have a stronger effect on EPE than perceptions of self-interest or distributive justice. Furthermore, while the definition of a fair procedure can be complex and heterogeneous, studies have shown that the more citizens in contact with welfare state agencies feel they have opportunities to communicate their views and personal situation to public employees, the more positively the interaction is viewed¹² (Bruch et al., 2010; Kumlin, 2004; Shore & Tosun, 2019; Soss, 1999). This positive perception often spills over into assessments of government responsiveness. In this sense, identifying 'perceived voice opportunities' as a metric for evaluating interactions with welfare state bureaucracies is the most theoretically sound approach. The stigmatizing signals of a means-tested design are expected to be most damaging for those who have already experienced unhelpful interactions and few opportunities to voice their concerns. Conversely, a history of helpful treatment with more opportunities to speak may act as a buffer, mitigating negative signals or making the participant more receptive to the rights-based message of universal design. This leads to the final hypothesis:

H2: The effect of policy design on EPE is moderated by perceived voice opportunities.

¹⁰ OCMW

¹¹ Actiris

¹² Furthermore, this perception of a fair procedure when there are voice opportunities is not predicated on impacting the decision making when expressing these voices (Kumlin, 2004; Tyler, 1996)

3. DATA & METHODOLOGY

Context & subject pool information

The empirical core is a randomized survey experiment testing the causal impact of policy architecture on External Political Efficacy (EPE). The researcher is employed at a Brussels-based workfare organization specializing in labor market (re)integration¹³, which provided direct access to the subject pool, a group of socioeconomically vulnerable citizens (N=126), consisting of long-term unemployed individuals (>2 years) currently enrolled in the workfare program. Data were collected through one-on-one survey administration at the workplace, conducted in Dutch, English, or French depending on the respondent's preference. Prior to the experimental survey design, consultations were held with the counselors of the workfare program, who maintain daily contact with the participants and possess detailed knowledge of their material circumstances and personal situations. Their input informed both the choice of stimulus topic and the formulation of survey items, ensuring that the instrument was grounded in the lived reality of the target group. Of the 191 individuals enrolled in the program at the time of data collection, 52 were absent on structural, survey-unrelated grounds, including sick leave and participation in language courses or vocational training (activities participants are permitted to attend during working hours). Of 139 present, 126 completed the survey, yielding a response rate of 91% among those available¹⁴.

This target group was strategically selected because of their proximity to welfare state institutions, which gave them real-life knowledge to interpret the political signals embedded in policy design. Given the specific subject pool, assessing the external validity requires comparing the subject pool's demographics with those of the broader target population (Morton & Williams, 2010). Since all subjects hold the status of "non-working jobseekers" (a prerequisite for program enrolment), their demographic profile is compared with the general population of non-working jobseekers in Brussels¹⁵. This comparison, detailed in Table 1.2 in the appendix, indicates that the subject pool is, to some degree, representative of the broader population. Pearson chi-square tests revealed no statistically significant differences between the subject pool and the broader population across all demographic variables, including gender, age categories, and educational level.¹⁶

¹³ Groot Eiland (<https://www.grooteiland.brussels/>)

¹⁴ The low refusal rate is partly attributable to the collaboration with on-the-floor instructors and counselors, who repeatedly emphasized the importance of participation to program members at several moments prior to data collection. Together with the structural nature of absences, this reduces the likelihood of systematic non-response bias.

¹⁵ Source: Viewstat Actiris; data of december 2025

¹⁶ gender ($\chi^2=2.357$; $p=0.125$), age categories <25 ($\chi^2=1.488$; $p=0.223$), 25-49 ($\chi^2=0.515$; $p=0.473$), ≥ 50 ($\chi^2=2.954$; $p=0.086$), low-educated ($\chi^2=2.214$; $p=0.137$), medium-educated ($\chi^2=0.067$; $p=0.796$), high-educated ($\chi^2=2.571$; $p=0.109$) and foreign-educated ($\chi^2=0.00$; $p=0.991$)

Experimental design and Treatments

The research uses a randomized between-subjects experimental design administered via the Qualtrics survey platform. This approach allows for the isolation of the interpretive effect of policy design by ensuring that any observed differences in efficacy are a result of the stimulus rather than pre-existing individual characteristics. While Mutz (2011) and Druckman & Green (2021) argue that "hard-hitting" treatments can be necessary to trigger responses and that mundane realism is not a strict requirement for experimental treatments, this research intentionally prioritizes a degree of ecological validity. Prior consultations with program counselors confirmed that housing insecurity constitutes one of the most pressing and frequently discussed concerns among program participants. Anchoring the experimental stimulus in an existing policy (the Brussels Rent Subsidy), therefore, ensures not only theoretical relevance but also genuine personal salience for the respondents. The decision to anchor the experiment in an existing policy is additionally driven by the nature of the dependent variable. To shift an affective attitude as EPE, a concrete, realistic example can be more effective than an abstract hypothetical. As Campbell (2012) observes, while political attitudes are often difficult to alter due to the inertia of early socialization, group identification, and ideology, attitudes like EPE possess a distinctively dynamic element. These can shift in real-time to external stimuli and tangible government actions. Consequently, to overcome the stability of pre-existing political perceptions, experimental treatment must possess high "real-world" salience.

Two treatment groups are exposed to a news vignette¹⁷ concerning a reform of the Rent Subsidy, while the third is the control group. To enhance internal validity, the text for the treatment groups remains identical, except for the specific rules regarding eligibility and automaticity.

¹⁷ The vignette is presented in Dutch (as an article of news site 'BRUZZ'); English (as an article of news site 'The Brussels Times') or French (as an article of news site BX1) – see appendix for lay out of vignette and complete survey.

Table 1. Overview of the vignettes

<i>Vignette 1 Means-tested design</i> Brussels Government Discusses Reform of Rent Subsidy <i>BRUSSELS - The rent subsidy is financial support from the government for people on the waiting list for social housing who are renting in the private market. The government is considering a new system for the rent subsidy. Under the proposal, the subsidy would:</i> • <i>Become a more stringent targeted benefit, available only to people below a certain income threshold.</i> • <i>Be paid out after application and additional verification, in order to improve oversight.</i>	<i>Vignette 2 Universal design</i> Brussels Government Discusses Reform of Rent Subsidy <i>BRUSSELS - The rent subsidy is financial support from the government for people on the waiting list for social housing who are renting in the private market. The government is considering a new system for the rent subsidy. Under the proposal, the subsidy would:</i> • <i>Become a right for everyone on the waiting list.</i> • <i>Be paid out automatically, without application.</i>
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Measurements

The survey followed a between-subjects design. Participants first completed the moderator measurement at the beginning of the survey, before any exposure to the experimental stimulus. They were then randomly assigned to one of the three experimental conditions, after which the dependent variable was measured immediately. This structure ensures that moderator responses remain uncontaminated by the treatment, while the post-exposure measurement of the dependent variable captures the direct interpretive effect of the policy vignette.

Moderator variable. Before the experimental stimulus, participants are asked to evaluate their prior administrative interactions to measure the moderating effect of their past interactions with state institutions (*H2*). We measure ‘Perceived voice opportunities’ in encounters with the public employment service in Brussels (PES)¹⁸ and/or public centers for social welfare (PCSW)¹⁹, as respondents were normally in contact with one of these services prior to or during enrolment in the workfare program. The voice opportunities are measured by a 5-point Likert scale (strongly disagree to strongly agree) with 4 items (Kumlin, 2004; Kumlin & Haugsgjerd, 2017b; Shore & Tosun, 2019), with parallel item batteries for each agency they interacted with. For both PES/PCSW the items are: ‘My counselor/social assistant listened to what I had to say’; ‘There was room to explain my concerns and personal situation to my counselor/social assistant’; ‘My counselor/social assistant took my personal situation into account’; and ‘Sometimes it seemed as if my opinion was not important to my counselor/social assistant (re-coded)’. However, it

¹⁸ Actiris

¹⁹ OCMW

became apparent during data collection that registration with the PES constituted a purely administrative step for these respondents, with no substantive interaction to evaluate. Consequently, only the PCSW scale is retained for analysis, which demonstrated good internal consistency ($\alpha = 0.845$; see appendix for full scale statistics). These items allow us to determine if a history of negatively perceived interactions amplifies the exclusionary signals of the means-tested design. Additionally, as the experimental survey was embedded within the organization's broader satisfaction survey, participants also evaluated their relationship with their current workfare caseworker, measured by a 5-point Likert scale (strongly disagree to strongly agree) with 5 items ($\alpha = 0.790$; see appendix for full scale statistics). Respondents were asked to evaluate to what extent their trajectory counsellor: 'takes my personal situation into account'; 'listens to my problems and questions'; 'follows up on me adequately'; 'provides clear and accurate explanation of my trajectory'; and 'does not understand me' (re-coded).

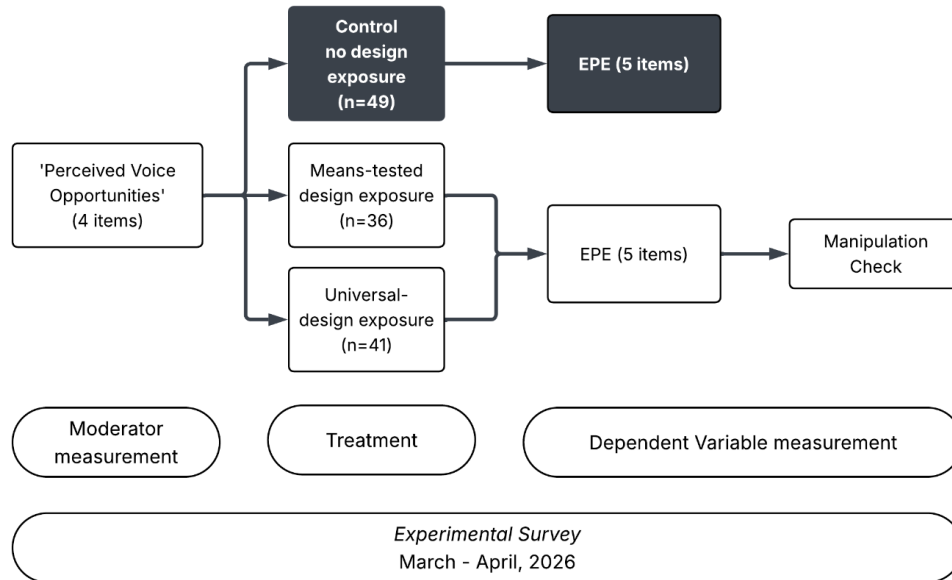
Dependent variable. Immediately following the randomized exposure to the policy vignette, the dependent variable (EPE) is measured. EPE is operationalized as a generalized, affectively charged belief about the responsiveness of political institutions to citizens like oneself (Esaïasson et al., 2015; Oser et al., 2023). Drawing on established scales from the American National Election Study (ANES), the Dutch National Election Study, and the Perceived Political Self-Efficacy scale (Caprara et al., 2009; Chamberlain, 2012; Groskurth et al., 2021; Vecchione et al., 2014), five items were selected and adapted to maximize comprehensibility for the target population. Respondents indicate their agreement on a five point Likert scale (strongly disagree to strongly agree) with the following statements: 'People like me have no say in what the Brussels government does' (recoded); 'People like me have no influence at all on Brussels government policy' (recoded); 'Political parties are only interested in my vote, not in my opinion' (recoded); 'The Brussels government is interested in what people like me think'; and 'The Brussels government pays attention to what people like me need'. The terms 'government' and 'policy' were clarified via tooltips embedded in the survey²⁰. The five items demonstrated good internal consistency ($\alpha = 0.815$; see appendix for full scale statistics). To summarize, Figure 1 shows an overview of the experimental survey design.

Control variables. Demographic variables were retrieved directly from the organization's administrative database and automatically linked to individual respondents via personalized survey links and Qualtrics embedded data, eliminating the need for self-reported demographics. The following variables were included: gender (male/female); age,

²⁰ The tooltip for 'Brussels government' read: "The group of people who govern a country or city and decide on rules, money and laws." The tooltip for 'policy' read: "Policy means, for example: making rules, giving money, or organising help for people."

recorded in three categories (<25, 25–49, ≥50 years); educational level, distinguished across four categories (low-educated, medium-educated, high-educated, and foreign-educated without equivalency recognition); and duration of program participation (in months).

Figure 1. Overview of the Experimental survey design



Randomization check

To verify that randomization produced comparable groups, Pearson chi-square tests were conducted for all available demographic variables across conditions (see Table 2). Randomization was successful for gender and all age categories, with no significant differences detected across any comparison. However, a significant imbalance was found for low-educated respondents. The universal condition contained a substantially higher proportion of low-educated participants compared to the control group ($\chi^2=7.393$, $p=0.007$), a pattern that also reached significance in the test across all three conditions ($\chi^2=9.467$, $p=0.009$). No significant differences were found for medium-educated, high-educated, or foreign-educated respondents.

Table 2. Demographic composition and randomization check across experimental conditions (N = 126)

	Control (n=49)	Means- tested (n=36)	Universal (n=41)	χ^2 all conditions	P	χ^2 Cntrl vs. Means- tested	p	χ^2 Cntrl vs. univer sal	p
Gender									
Men	51.02%	58.33%	65.85%	2.019	0.364	0.447	0.504	2.013	0.156
Women	48.98%	41.67%	34.15%	2.019	0.364	0.447	0.504	2.013	0.156
Age									
<25 years	18.37%	8.33%	17.07%	1.820	0.402	1.723	0.189	0.026	0.873
25 - 49 years	61.22%	75.00%	68.29%	1.813	0.404	1.783	0.182	0.487	0.485
>= 50 years	20.41%	16.67%	14.63%	0.538	0.764	0.190	0.663	0.509	0.476
Education									
Low-educated ²¹	12.24%	13.89%	36.59%	9.467	0.009**	0.050†	0.823	7.393	0.007**
Medium- educated ²²	28.57%	19.44%	14.63%	2.693	0.260	0.929	0.335	2.509	0.113
High-educated ²³	12.24%	13.89%	17.07%	0.431	0.806	0.050†	0.823	0.421	0.516
Foreign educated (no equivalency)	46.94%	52.78%	31.71%	3.812	0.149	0.283	0.595	2.158	0.142

Note: all tests are Pearson chi-square. Using dummy variables, all conditions (N=126) column compares all three conditions simultaneously (df=2); pairwise columns compare each treatment group against the control group (df=1). n=85 (Control vs. Means-tested); n=90 (Control vs. Universal).

**p<0.01

† Expected count <5 (4.66) in 25% of cells

Manipulation check

To ensure the internal validity of the experiment, the survey concludes with a manipulation check (Druckman & Green, 2021; Morton & Williams, 2010; Mutz, 2011). This consists of two factual, closed-ended questions. The first assessed the manipulation of eligibility with the question "Who is eligible for the rent allowance according to the news article you read?" (answer choices: everyone on the waiting list/only those whose income falls below a certain threshold/I don't know). The second checked the automaticity manipulation with the question, "How would the payment be made, according to the news article you read?" (answer choices: Automatic (without application)/Only upon request and verification/I don't know). This check is vital to confirm that respondents correctly "deciphered" the administrative logic of the treatment they received. Figure 2 shows the proportion of respondents who answered each question correctly or incorrectly. The Universal condition showed the highest proportion of incorrect or uncertain responses: 12.2% did not correctly identify the eligibility criterion and 14.6% did not correctly identify the payment mechanism. In the Means-tested condition, these figures were somewhat lower at 8.4%

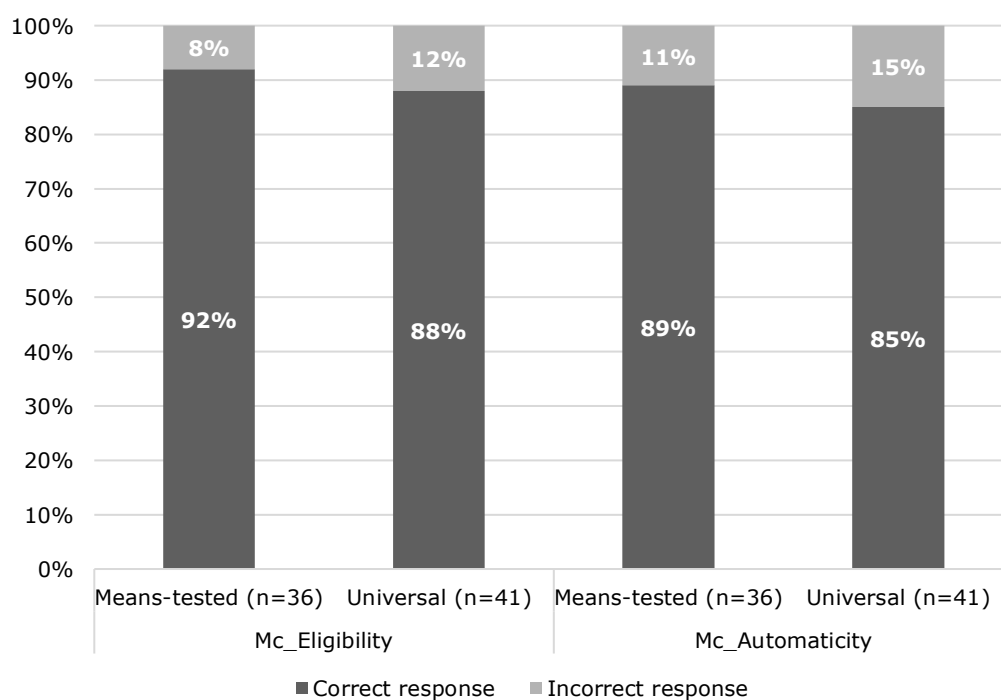
²¹ Lager onderwijs tot en met 2de graad secundair onderwijs

²² Derde graad secundair onderwijs

²³ Bachelor of Master

and 11.2% respectively. While the differences between conditions are not large, the Universal condition's higher failure rate coincides with an overrepresentation of less-educated respondents in that group. Respondents in the treatment conditions who answered either or both questions incorrectly were excluded from the analysis, reducing the analytical sample from N=126 to N=113 (control: n=49; means-tested: n=31; universal: n=33). This exclusion ensures that the results are a direct consequence of the policy design's interpretive signal rather than a miscomprehension of the stimulus. To assess the robustness of this decision, full-sample (Intent-to-Treat) analyses are reported in Table 8.1-8.3 in the appendix.

Figure 2. manipulation check



4. RESULTS

4.1 Analyses

The results are organized around the three hypotheses to test the causal impact of policy design on EPE among socioeconomically vulnerable citizens. First, the main effects of policy design on EPE (*H1a-H1b*) are examined by comparing mean EPE scores across experimental conditions using one-way ANOVA with planned contrasts (Field et al., 2012). To assess the treatment effects and to account for the observed imbalance in educational level (see Table 2), two OLS regression models predicting EPE are additionally estimated. The first model includes only experimental condition dummies, and the second model adding educational level as a covariate. Second, the moderation hypothesis (*H2*) is tested using OLS regression, with a first model estimating the main effects of treatment condition and ‘perceived voice opportunities’, followed by a second model that adds the interaction terms between treatment condition and ‘perceived Voice Opportunities’. Statistical significance is evaluated at $\alpha = .05$ (two-tailed).

4.2 Main Effects of Policy Design on EPE

Descriptives

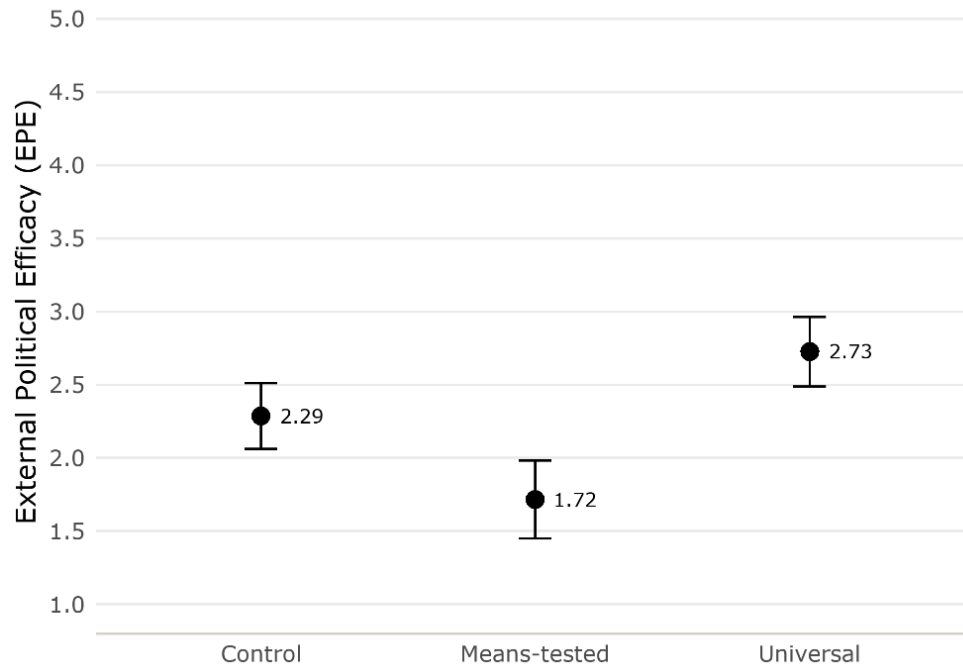
Analysis begins with mean EPE scores across the three conditions. Table 3 presents the descriptive statistics for EPE by experimental condition. EPE is measured on a 1-5 scale, where items are (re)coded so that higher scores indicate greater EPE. Comparison of the means reveals a pattern consistent with the hypothesized direction. Respondents exposed to the Means-tested policy vignette (more stringent and targeted benefit, eligibility restricted to those below an income threshold, payment conditional on application and additional verification) reported the lowest mean EPE ($M=1.72$, $SD=0.76$), followed by the control group ($M=2.29$, $SD=0.81$), while respondents exposed to the Universal policy vignette (benefit is a right for everyone on the waiting list, paid out automatically without application) reported the highest mean EPE ($M=2.73$, $SD=0.70$). As visualized in figure 3, the 95% confidence intervals for mean of the Means-tested condition is non-overlapping with the control group, while there is a marginal overlap between control and Universal condition. This suggests meaningful differences between groups.

Table 3. Descriptive Statistics for EPE by Experimental Condition

Condition	n	Mean	SD	SE	95% CI for mean ($M \pm 1.96 \times SE$)	
<i>Control</i>	49	2.29	0.81	0.12	2.05	2.52
<i>Means-tested</i>	31	1.72	0.76	0.14	1.44	1.99
<i>Universal</i>	33	2.73	0.70	0.12	2.49	2.97

Note. EPE is measured on a 1–5 scale (higher scores indicate greater external political efficacy).

Figure 3. Mean External Political Efficacy Scores across Experimental Conditions with 95% Confidence Intervals (N=113)



Hypothesis testing

As a first step to evaluate whether exposure to a Means-tested design decreases EPE relative to a baseline (*H1a*) and whether a Universal design increases EPE relative to a baseline (*H1b*) in a significant way, one-way ANOVA with planned contrasts was conducted. Given that directional hypotheses were formulated prior to data collection, planned contrasts are preferred over post-hoc tests (Field et al., 2012; Neels, 2017). Two planned contrasts were specified, contrast 1 comparing the Means-tested group to the control group, and contrast 2 comparing the Universal to the control group. As the control group appears in both contrasts, these comparisons are non-orthogonal, which slightly increases the chance of Type I error (Field et al., 2012). Applying a Bonferroni correction ($\alpha = 0.025$ per contrast) does not alter the conclusions, as both effects remain statistically significant. Prior to testing, Levene's test confirmed homogeneity of variances across all three conditions ($F(2,110) = 0.767, p = 0.467$)²⁴. The one-way ANOVA revealed a significant difference in EPE across experimental conditions ($F(2,110) = 14.128, p < 0.001$).

Consistent with *H1a*, contrast 1 (means-tested vs. control) confirmed that respondents exposed to the means-tested vignette reported significantly lower EPE than the control

²⁴ It is worth noting that significance tests of homogeneity of variances are sensitive to sample size. In small samples, Levene's test may lack the power to detect true differences in variances even when they exist (Field et al., 2012). Given that the smallest group in this study contains $n=31$ respondents, the non-significant result should be interpreted with appropriate caution.

group ($t(110) = -3.263, p=0.001, \text{Cohens' } d=-0.794^{25}$). Consistent with *H1b*, contrast 2 (Universal vs. control) confirmed that respondents exposed to the Universal vignette reported significantly higher EPE than the control group ($t(110) = 2.567, p=0.012, \text{Cohen's } d = 0.578$). Full results are presented in Tables 4.1 and 4.2 in the appendix.

Regression Models Predicting EPE

Although the planned contrasts provide a direct test of hypotheses *H1a* & *H1b*, ANOVA can be equivalently expressed as an OLS regression equation in which treatment condition dummies serve as predictors of EPE (Field et al., 2012). This equivalence allows for the extension of the baseline model with covariates, without altering the underlying logic of the analysis. Furthermore, OLS regression coefficients offer a more intuitive interpretation of the treatment effects, expressing the expected change in EPE associated with each experimental condition. Given the observed randomization imbalance in educational level (see Table 2), two OLS regression models are estimated. Model 1 includes only experimental condition dummies, with the control group as the reference category, and should reproduce the treatment effects found in the planned contrasts. Model 2 adds educational level as a covariate, with low-educated (who are overrepresented in the Universal condition) to test whether the treatment effects remain stable once education is accounted for.

Results of the two models are shown in Table 4. Model 1 confirms the pattern found in the planned contrasts. Exposure to the Universal policy vignette is associated with significantly higher EPE compared to the control group ($B=0.441, SE=0.172, p=0.012$), while exposure to the Means-tested policy vignette is associated with significantly lower EPE ($B=-0.572, SE=0.175, p=0.001$). The model accounts for 20,4% for the variance in EPE ($R^2=0.204, F(2,110) = 14.128, p<0.001$). Model 2 adds educational level as a covariate. None of the educational dummies reached statistical significance (all $p>0.05$). Crucially, the treatment effects remain substantively unchanged. The Universal condition significantly increases EPE compared to the control group ($B=0.363, SE=0.181, p=0.048$) and the Means-tested condition remains significant and virtually identical in magnitude ($B=-0.578, SE=0.176, p=0.001$), confirming that educational level does not confound the treatment effects. The addition of educational level yields a non-significant increase in explained variance ($R^2 \text{ change} = 0.018, F(3,107) = 0.805, p=0.494$). Full and detailed results are presented in Table 5.1 in the appendix.

²⁵ Estimated effect sizes use pooled SD for all the groups as the standardizer.

Table 4. OLS Regression of External Political Efficacy on Experimental Condition

Dependent variable: EPE	Model 1		Model 2	
	B	SE	B	SE
TREATMENT CONDITION				
Intercept	2.287***	(0.109)	2.440***	(0.195)
Universal design exposure	0.441*	(0.172)	0.363*	(0.181)
Means-tested design exposure	-0.571**	(0.175)	-0.578**	(0.176)
EDUCATION (REF: LOW -EDUCATED)				
Medium-educated	-		-0.256	(0.230)
High-educated	-		0.067	(0.253)
Foreign-educated (no equivalency)	-		-0.189	(0.198)
R ²	0.204		0.222	
Adjusted R ²	0.190		0.186	
R ² change	-		0.018	
F	14.128***	(df=2,110)	6.104***	(df=5,107)
N	113		113	

*Note. Reference categories: control group (treatment condition); low-educated (education). Standard errors in parentheses. * $p < .05$. ** $p < .01$. *** $p < .001$.*

Taken together, these findings confirm that the signals embedded in policy design, independently of resource effects of policy or individual characteristics, exert a meaningful interpretive effect, measured by EPE, among socioeconomically vulnerable citizens. Although the experimental design is not fit to make claims about underlying mechanisms (Druckman & Green, 2021; Mutz, 2011), the results provide evidence that policy design, through its eligibility and automaticity dimensions, generates distinct political lessons. Exposure to a Means-tested policy design, signaling conditionality and suspicion, significantly reduces EPE relative to the control group, while exposure to a universal design, signaling social rights and institutional trust, significantly increases EPE (Campbell, 2012; Mettler & Sorelle, 2018; Soss, 1999). Two caveats nonetheless apply. First, covariate adjustment is not equivalent to successful randomization (Druckman & Green, 2021; Field et al., 2012). Imbalance in educational level means the Universal condition started from a compositionally disadvantaged baseline, so the positive effect of that condition may be a conservative estimate. Second, as low-educated respondents were disproportionately concentrated in the Universal condition, the post-exclusion sample is not randomly reduced, which further complicates the interpretation of *H1b*. This is corroborated by the Intent-to-Treat analysis on the full sample ($N=126$), reported in the appendix, in which the Universal condition remains significant without covariate adjustment ($B=0.362$, $p=.029$) but falls below conventional significance once educational level is controlled for ($B=0.297$, $p=.084$). By contrast, the negative effect of the Means-tested condition remains stable across all ITT models ($p<.01$), supporting the robustness of *H1a*.

These main effects, however, do not yet speak to the conditional nature of policy feedback. As argued in the theoretical framework, the interpretive signals of policy design are not processed uniformly, they can be filtered through citizens' experiences with welfare state institutions.

4.3 Moderation effect: Perceived Voice Opportunities

Within the policy feedback literature, the political consequences of welfare state encounters are theorized to operate through two analytically distinct channels. The first concerns the interpretive signals embedded in policy design, with formal rules such as eligibility and automaticity, which is the focus of *H1*. The second concerns the administration of policy in which the quality of interactions between citizen and bureaucracy through which these formal rules are experienced is central. Even where policy design is held constant, substantial variation in political learning can emerge from the degree to which citizens feel heard, respected, and taken seriously in their encounters with welfare state officials (Bruch et al., 2010; Kumlin, 2004; Shore & Tosun, 2019). This research operationalizes this administrative dimension as 'Perceived Voice Opportunities' in interactions with the PCSW, as detailed in the methodology section. As not every respondent had prior contact with the PCSW, the moderation analysis is restricted to those who did ($n=55$).

Perceived Voice at the Public Centers for Social Welfare

To test *H2*, that the effect of policy design on EPE is moderated by Perceived Voice Opportunities, two OLS regression models were estimated. In each model, the perceived voice scale was mean centered prior to analysis to facilitate interpretation of the lower-order coefficients (Field, 2024). Model 1 estimates the main effects of treatment conditions and perceived voice simultaneously. Model 2 extends this by adding two interaction terms between treatment conditions and the centered perceived voice scale. A significant interaction term indicates that the effect of treatment on EPE varies as a function of perceived voice.

Results are presented in Table 5. Model 1 shows a positive but non-significant main effect of PCSW perceived voice ($B=0.081$, $SE=0.081$, $p=0.320$), alongside a significant effect of Means-tested exposure ($B=-0.997$, $SE=0.204$) and a non-significant effect of Universal exposure ($B=0.138$, $SE=0.233$). Model 2 adds the two interaction terms, neither of which reaches statistical significance (Means-tested $\times$ PCSWvoice: $B = -0.015$, $SE = 0.179$, $p = 0.932$; Universal $\times$ PCSWvoice: $B = -0.148$, $SE = 0.243$, $p = 0.545$), with virtually no increase in explained variance (R^2 change = .005, $F(2,49) = 0.197$). The distribution of prior PCSW contact across experimental conditions revealed cell sizes of $n = 21$ (control), $n = 21$ (means-tested), and $n = 13$ (universal). The output of these models is therefore considered statistically uninformative, and *H2* remains untested in any meaningful sense

with the available data. Replication in a larger sample with broader institutional coverage is necessary before any conclusion regarding the moderating role of perceived voice can be drawn. Given the inconclusive nature of the PCSW analysis, no further theoretical elaboration is warranted. Full results are presented in Table 6.1 in the appendix.

For completeness, a supplementary exploratory analysis was conducted using the caseworker relational quality scale described in the methodology section ($\alpha=0.790$). The two methodological caveats noted there apply: the scale captures relational quality rather than perceived voice strictly defined, and socially desirable responding cannot be excluded given the survey context. The scale was mean-centered prior to analysis ($M = 4.37$), with eleven respondents excluded due to missing data ($n = 102$). Model 1 replicates the main treatment effects on EPE, Means-tested exposure significantly reduces EPE ($B = -0.622$, $SE = 0.182$, $p < 0.001$) and Universal exposure significantly increases EPE ($B = 0.393$, $SE = 0.180$, $p = 0.031$). The main effect of relational quality is not significant ($B = -0.027$, $SE = 0.118$, $p = 0.820$). Model 2 adds two interaction terms. The interaction between Means-tested exposure and relational quality reaches statistical significance ($B = 0.588$, $SE = 0.274$, $p = 0.034$), while the interaction for universal exposure does not ($B = 0.369$, $SE = 0.313$, $p = 0.242$). The block addition of interaction terms did not reach conventional significance (R^2 change = 0.036, $F(2,96) = 2.304$, $p = 0.105$). Given the exploratory nature of this analysis, the measurement limitations of the scale, and the non-significant block F-test, no conclusions are drawn. The pattern may nonetheless suggest a direction worth pursuing in future research with more appropriate measurement instruments and larger samples. Full results are presented in Table 7.1 in the appendix.

Table 5. Moderation Analysis: Effect of Policy Design on EPE, Moderated by PCSW Perceived Voice Opportunities

Dependent variable: EPE	Model 1		Model 2	
	B	SE	B	SE
TREATMENT CONDITION				
Intercept	2.392***	(0.144)	2.392***	(0.147)
Universal design exposure	0.138	(0.233)	0.128	(0.238)
Means-tested design exposure	-0.997***	(0.204)	-0.999***	(0.207)
PCSWvoice _centered	0.081	(0.081)	0.111	(0.125)
INTERACTION				
PCSWvoice X Universal	-		-0.148	(0.243)
PCSWvoice X Means-tested	-		-0.015	(0.179)
R ²	0.397		0.402	
Adjusted R ²	0.361		0.342	
R ² change	-		0.005	
F	11.182***	(df=3,51)	0.197	(df=2,49)
N	55		55	

Note. Dependent variable: EPE (1–5 scale). Reference category: control group. perceived voice scale mean-centered ($M = 3.14$). Standard errors in parentheses. * $p < .05$. ** $p < .01$. *** $p < .001$.

5. CONCLUSION AND DISCUSSION

Policy feedback theory has long argued that the design of public policy shapes more than administrative outcomes, it also shapes the political attitudes of those who encounter it (Mettler & Sorelle, 2018; Mettler & Soss, 2004). This research sought to examine that claim empirically for a group of socioeconomically vulnerable citizens who are both, partially, dependent on welfare state institutions and relatively more absent from the political arena (Bovens & Wille, 2010; Traber et al., 2022). Through a randomized between-subject survey experiment, participants were exposed to either a Means-tested policy vignette, a Universal policy vignette, or a control condition, with external political efficacy (EPE) measured immediately thereafter. To what extent does social policy design, through universal or means-tested frameworks, shape the EPE of socioeconomically vulnerable citizens? The results support both main-effect hypotheses. Consistent with *H1a*, respondents exposed to the Means-tested vignette reported significantly lower EPE than the control group, and consistent with *H1b*, respondents in the Universal condition reported significantly higher EPE. The moderation hypothesis (*H2*), predicting that perceived voice opportunities would moderate these effects, could not be meaningfully tested due to insufficient statistical power in the available subsample ($n = 55$).

The central and most significant finding is that policy design generates a shift in EPE independently of any material resource transfer. Participants were exposed only to a brief description of a policy proposal; no actual benefit was provided. Yet EPE moved in opposite directions depending on the design logic of the vignette they encountered. Thus, EPE, as argued in the theoretical framework, constitutes the interpretive effect itself as attitudinal crystallization of what formal policy rules communicate to citizens about their standing in the eyes of the state (Campbell, 2012; Ingram et al., 2007; Mettler & Sorelle, 2018). Eligibility criteria signal whether a citizen is treated as a suspect claimant or a rights-bearing subject; automaticity signals whether the state extends institutional trust or imposes administrative surveillance. For this group, these signals are not abstract, they possess the capacity to decode what formal rules mean in practice as for them, this is experientially grounded. Welfare state institutions thus function as sites of political learning, where the formal logic of policy design is translated into citizen's assessment of how the state relates to people like them (Mettler & Sorelle, 2018; Soss, 1999). Notably the EPE levels across all conditions are low, indicating that respondents in no condition perceived the government as genuinely responsive to people like them. This underscores the depth of political disengagement within this group and suggests that while policy design can shift EPE meaningfully, it operates against a backdrop of entrenched political alienation that a single policy signal is unlikely to overcome.

A theoretically noteworthy pattern emerges in the relative magnitude of the two effects. The negative impact of the Means-tested condition (Cohen's $d = -0.794$, medium to high effect) is somewhat more pronounced than the positive impact of the Universal condition (Cohen's $d = 0.578$, medium effect) (Cohen, 1992). This directional asymmetry is consistent with a broader principle of negativity bias (Kahneman & Tversky, 1979), and more specifically within the policy feedback context where previous research has shown that welfare loss produces stronger attitudinal and behavioral responses than equivalent welfare gain (Zhang et al., 2023). For socioeconomically vulnerable citizens, the punitive signals of a Means-tested design (e.g. conditionality, suspicion, and administrative surveillance) may resonate more deeply than the affirming signals of a Universal design.

Several methodological considerations bound these conclusions. First, the experimental vignette simultaneously manipulated eligibility criteria and automaticity, making it impossible to isolate the contribution of each dimension separately. A factorial design crossing these elements independently would offer a more precise account of the causal mechanism (Druckman & Green, 2021; Mutz, 2011). Such a design would also create space to examine additional dimensions of interpretive signaling that this study could not address (Campbell, 2012). Second, the ecological validity of the stimulus is necessarily limited. In complex European welfare states, policy designs are frequently hybrid and can contain contradictory signals²⁶, while vulnerable citizens typically interact with multiple programs simultaneously (Rosenthal, 2021). The findings therefore demonstrate that interpretive signals are detectable and consequential, rather than provide a precise estimate of their real-world magnitude. The high personal salience of housing insecurity strengthens internal validity but limits generalizability to less relevant domains. Furthermore, because EPE was measured immediately after exposure, the durability of these shifts requires longitudinal follow-up or a within-subjects experimental design.

Concerning *H2*, the available data did not permit a meaningful test of whether perceived voice opportunities moderate the effect of policy design on EPE. Prior research has shown that direct welfare state encounters can affect EPE independently (Shore & Tosun, 2019), suggesting that the administrative dimension of policy is politically consequential in its own right. Whether and how that effect interacts with the interpretive signals of policy design remains to be established. Future research might also consider extending the outcome variable beyond EPE. Interactions with welfare agencies may also shape internal political efficacy, citizens' sense of their own political competence, through distinct pathways. For socioeconomically vulnerable citizens already absent from the political arena, EPE alone

²⁶ For example, although an unemployment benefit is formally contributory (rights-based), extensive activation and behavioral conditioning can result in citizens receiving signals of surveillance and distrust typically associated with means-tested assistance. Universal programs, such as child benefits, are also often hybrid: while universally accessible, they frequently include income-related social supplements.

may be insufficient to reverse political disengagement. Even when citizens perceive the government as responsive to people like them, acting on that perception requires a sense of personal political competence, the belief that one can understand and navigate political processes. Examining both dimensions simultaneously would offer a more complete picture of how welfare state contact structures political self-understanding.

Despite these limitations, the findings carry relevance for both scholarship and policy practice. They provide causal evidence that the interpretive dimension of policy design has measurable consequences for EPE among a population that is frequently absent from political life and underrepresented in research. As argued before, the design of social policy is not merely a matter of administrative efficiency, it can be a determinant of democratic inclusion. When formal policy rules communicate suspicion and control to citizens who are already structurally marginalized, they do not simply deliver a service more or less effective; they actively shape whether those citizens experience the political system as responsive to people like them. This consideration is far from trivial in contexts where institutional trust is already fragile and political disengagement runs deep.

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APPENDIX

1. FAIR-principes voor GENAI gebruik

FAIR-DATA PRINCIPE	FAIRE verantwoording Verfijnen onderzoeksvraag
Findable - Vindbaar	Microsoft Copilot (M365-licentie) gebruikt in oktober 2025 voor het structureren van de probleemstelling en verfijnen van de onderzoeksvraag
Interoperable – Begrijpelijk	Ik heb de tool gebruikt om vanuit een maatschappelijke probleemstelling mogelijke onderzoeksvragen te verkennen rond politieke efficacy bij groepen met een grote afstand tot de arbeidsmarkt. De uiteindelijke formulering van de onderzoeksvraag gebeurde op basis van eigen analyse en raadpleging van wetenschappelijke literatuur.
Reusable – Herbruikbaar	<i>Prompts: "als je deze probleemstelling bekijkt, wat zou een logische onderzoeksvraag kunnen zijn? Vanuit wel maatschappelijk/wetenschappelijk probleem vertrekken - vertrekkende vanuit mijn eigen werkveld waar ik moeilijk naast kon kijken - groepen in de samenleving die op de rand van de maatschappij leven (langdurig werklozen, schoolverlaters, nieuwkomers, mensen met psychische en fysieke problemen, mensen die leefloon ontvangen van OCMW, jongeren die deeltijds leren en werken omdat ze voltijds school niet kunnen (watervalsysteem)) --> overkoepelende term 'mensen met een grote afstand tot de arbeidsmarkt', geven vaak aan dat ze (zeker in het kader van de verkiezingen vorig jaar): - niet geïnteresseerd zijn in politiek - Niets verwachten van politiek, in de zin dat ze politiek linken aan overheden waarmee ze vaak in negatieve zin in contact komen (belastingen, bevolkingsdiensten, tewerkstellingsdiensten, OCMW's,...) - Negatieve gevoelens tegenover politiek - Vaak aangeven niet te gaan stemmen, waarom is het een probleem - input legitimacy (Representativeness)? - output legitimacy (Responsiveness)? - link IPA en Stemgedrag --> ook legitimacy probleem Wat is de Puzzel/het verwonderlijke dat je wil beantwoorden? - vooral focus op politieke socialisatie bij jongeren en jong volwassenen. Maar wat voor impact heeft leven op de rand van de maatschappij hierop? - vooral focus op interventies op school (burgerschapslessen)? maar wat hebben sociale projecten van impact (vaak zeer politiek bewuste mensen die deze projecten opzetten en die mensen begeleiden) - Vooral focus op schoolgaande jeugd. Mensen die ergens tijdens hun volwassen leven uit de boot vallen, wat voor impact heeft dat? Hogere educatie maakt verschil, man/vrouw maakt verschil. Wat voor effect heeft het op hoogopgeleide mannen (buitenland diploma) die werkloos zijn. ? "</i> <i>Prompt: "is there existing literature that empiracally researches the effect of unemployment on internal political efficacy. or the mediating effect of unemployment on the link between high political efficacy and higher education levels?"</i>
Accessible - Toegankelijk	Een link naar de volledige chatsessie kon niet gegenereerd worden via copilot maar kan wel volledig geraadpleegd worden in bijgevoegde Bijlage A

FAIR-DATA PRINCIPE	FAIRE verantwoording Brainstorm over methode
Findable - Vindbaar	Microsoft Copilot (M365-licentie) gebruikt in november-december 2025.
Interoperable – Begrijpelijk	Ik heb de tool gebruikt voor inspiratie over experimentele onderzoeksdesigns binnen politieke wetenschappen en meer specifiek binnen policy feedback literatuur.
Reusable – Herbruikbaar	Prompt: researching the interpretive effects of social policy on political efficacy, can you find some papers that examine how interpretive effects can be simulated and consequentially studied in an experimental design? Prompt: i'm searching for handbooks about survey experiments in political/social science. Can you help?
Accessible - Toegankelijk	Een link naar de volledige chatsessie kon niet gegenereerd worden via copilot maar kan wel volledig geraadpleegd worden in bijgevoegde Bijlage A

FAIR-DATA PRINCIPE	FAIRE verantwoording Technische assistentie Qualtrics
Findable - Vindbaar	Microsoft Copilot (M365-licentie) gebruikt in Januari 2026
Interoperable – Begrijpelijk	Copilot werd gebruikt om een technisch probleem met Qualtrics uit te zoeken, specifiek over de manier om met directories en bestaande data van respondenten embedded data toe te voegen aan de survey responses
Reusable – Herbruikbaar	Prompt: in Qualtrics, i have a list of 200 people i'm going to survey with an experimental vignette (2 arm and control group). I have already demographical data of this group. Can i input the list of all the people somewhere?
Accessible - Toegankelijk	Een link naar de volledige chatsessie kon niet gegenereerd worden via copilot maar kan wel volledig geraadpleegd worden in bijgevoegde Bijlage A

FAIR-DATA PRINCIPE	FAIRE verantwoording Eindredactie
Findable - Vindbaar	Claude Model Opus 4.7 (Anthropic, betalende versie) gebruikt in mei 2026

Interoperable – Begrijpelijk	Claude werd gebruikt voor eindredactie. Meer bepaald voor het proberen slinken van de wordcount en juiste nummering van tabellen en figuren
Reusable – Herbruikbaar	Prompt: Voor mijn masterproef moet ik ongeveer 800 woorden schrappen. hoe ga ik best te werk ? misschien eerst zaken die teveel herhaalt worden of te uitgebreid omschreven worden inkorten of wat stel je voor? Prompt: kan je in deze masterproef volgende controleren en aangeven waar ik aanpassingen moet doen: 1. tabelnummeringen (verschil in tekst en naar appendix, nummering in tekst moet overeen komen met tabel)
Accessible - Toegankelijk	Een link naar de volledige chatsessies: https://claude.ai/share/5ae62fe1-cb48-47ef-8717-d77547a854ae & https://claude.ai/share/6d0aa986-63a9-4b4c-9e0e-e0d85740b4b3. Ook beschikbaar in Bijlage A

2. Additional tables and figures

1. Chi square Subject Pool (N=126) vs Population

Table 1.1: Demographics of the Brussels Non-Working Jobseekers (Population) vs. Subject Pool

Variable	Population (N=96.650)	Subject pool (N=126)
Men	51,1%	57,94%
Women	48,9%	42,06%
<25 years	11,6%	15,08%
25 - 49 years	64,4%	67,46%
>= 50 years	24%	17,46%
Low-educated ²⁷	15,8%	20,63%
Medium-educated ²⁸	20,5%	21,43%
High-educated ²⁹	20,0%	14,29%
Foreign educated (no equivalency)	43,7%	43,65%

Table 1.2: Pearson chi² demographics population vs Subject pool

Dummy variables have been created for each category. The expected values entered are the proportions of the NWWZ population (dataset from Actiris's viewstat: figures for December 2025).

Variable (df=1)	χ^2	p-value
Geslacht	2,357	0,125
Laaggeschoold	2,214	0,137
middengespoold	0,067	0,796
Hooggeschoold	2,571	0,109
Buitenlandgeschoold	0,000	0,991
<25 jaar	1,488	0,223
25 – 49 jaar	0,515	0,473
≥ 50 jaar	2,954	0,086

2. Reliability statistics for External Political Efficacy scale

²⁷ Lager onderwijs tot en met 2de graad secundair onderwijs

²⁸ Derde graad secundair onderwijs

²⁹ Bachelor of Master

Table 2.1: Cronbach's alpha EPE scale

Cronbach's alpha	N of items
0,815	5

Note: Valid cases N=123, excludes cases N=3

Table 2.2: reliability statistics EPE items

<i>Item</i>	<i>Mean</i>	<i>Std-deviation</i>	<i>Cronbach's Alpha if Item Deleted</i>
q_epe_1_recode	2,20	1,152	0,765
q_epe_2_recode	2,34	1,200	0,767
q_epe_3_recode	1,90	0,918	0,822
q_epe_4	2,33	0,955	0,771
q_epe_5	2,45	1,249	0,761
Scale Statistics	11,23	4,182	-

3. Crosstabs Manipulation checks by experimental condition

Table 3.1 crosstab Mc_Eligibility

		Means-tested	Universal	Total
Correct for Universal (1)	Count	1	36	37
	%within treatment	2,8%	87,8%	48,1%
Correct for Means-tested (2)	Count	33	2	35
	%within treatment	91.7%	4.9%	45.5%
I don't know (3)	Count	2	3	5
	%within treatment	5.6%	7.3%	6.5%
Total	Count	36	41	77
	%within treatment	100%	100%	100%

Table 3.2 Mc_Automaticity

		Means-tested	Universal	Total
Correct for Universal (1)	Count	2	35	37
	%within treatment	5.6%	85.4%	48.1%
Correct for Means-tested (2)	Count	32	1	33
	%within treatment	88.9%	2.4%	42.9%
I don't know (3)	Count	2	5	7
	%within treatment	5.6%	12.2%	9.1%

Total	Count	36	41	77
	%within treatment	100%	100%	100%

4. One-way ANOVA with planned contrasts results: EPE by Experimental condition

Table 4.1: Levene's Test of Homogeneity of Variances

	Levene statistic	df1	df2	p
Omnibus	0.767	2	110	0.467

Note: Levene's test confirms homogeneity of variances ($p > .05$). Analytical sample: $N=113$ (control: $n=49$; means-tested: $n=31$; universal: $n=33$).

Tables 4.2: One-way ANOVA with planned contrasts

ANOVA

	Sum of Squares (Between Groups)	df	Mean square	F	P	ANOVA Effect sizes	
						η^2	95% CI
Omnibus	16.410	2	8.205	14.128	<0.001	0.204	[0.078, 0.321]

Contrast Coefficients

	Treatment_num		
Contrast	Controle	Meanstested	Universal
1: Means-tested vs control	-1	1	0
2 Universal vs control	-1	0	1

Contrast Tests

		Contrast	Value of Contrast	Std. Error	t	df	Sig. (2-tailed)	95% CI Lower	95% CI upper
EPE_Scale	Assumes equal variances	1	-,5706	,17489	-3,263	110	,001	-,9172	-,2240
		2	,4405	,17161	2,567	110	,012	,1004	,7806
		1	-,5706	,17830	-3,200	66,795	,002	-,9265	-,2147

	Does not assume equal variances	2	,4405	,16707	2,637	75,032	,010	,1077	,7734
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Contrast effect sizes

	Contrast	Standardizer ^a	Point Estimate	95% CI Lower	95% CI upper
Cohen's d	1	,76206	-,749	-1,208	-,287
	2	,76206	,578	,129	1,025
Hedges' correction	1	,76731	-,744	-1,199	-,285
	2	,76731	,574	,128	1,018

a. The denominator used in estimating the effect sizes.

Cohen's d uses the pooled standard deviation for all the groups.

Hedges' uses pooled standard deviation for all the groups, plus a correction factor.

5. Linear regression model (H1a-H1b)

Table 5.1: dummy coding

	universal_dummy	meanstested_dummy
Controle	0	0
Means-tested	0	1
Universal	1	0

Table 5.2 OLS Regression of External Political Efficacy on Experimental Condition and Educational Level

Model	R	R ²	Adjusted R ²	SE of the Estimate	R ² change	F change	df1	df2	Sig. F Change
1	0.452 ^a	0.204	0.190	0.76206	0.204	14.128	2	110	<0.001
2	0.471 ^b	0.222	0.186	0.76410	0.018	0.805	3	107	0.494

a. Predictors: (Constant), Meanstested_dummy, universal_dummy

b. Predictors: (Constant), Meanstested_dummy, universal_dummy, Hooggeschoold_dummy, Middengeschoold_dummy, Buitenlandgeschoold_dummy

ANOVA

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	16,410	2	8,205	14,128	<,001 ^b
	Residual	63,881	110	0,581		
	Total	80,291	112			
2	Regression	17,819	5	3,564	6,104	<,001 ^c
	Residual	62,472	107	0,584		
	Total	80,291	112			

a. Dependent Variable: EPE_Scale

b. Predictors: (Constant), Meanstested_dummy, universal_dummy

c. Predictors: (Constant), Meanstested_dummy, universal_dummy, Hooggeschoold_dummy, Middengeschoold_dummy, Buitenlandgeschoold_dummy

Coefficients

		Unstandardized Coefficients		Standardized Coefficients			95,0% Confidence Interval for B	
Model		B	Std. Error	Beta	t	Sig.	Lower Bound	Upper Bound
1	(Constant)	2,287	,109		21,005	<,001	2,071	2,502
	universal_dummy	,441	,172	,238	2,567	,012	,100	,781
	Meanstested_dummy	-,571	,175	-,302	-3,263	,001	-,917	-,224
2	(Constant)	2,440	,195		12,501	<,001	2,053	2,827
	universal_dummy	,363	,181	,196	2,002	,048	,004	,722
	Meanstested_dummy	-,578	,176	-,306	-3,276	,001	-,927	-,228
	Middengeschoold_dummy	-,256	,230	-,124	-1,115	,268	-,713	,200
	Hooggeschoold_dummy	,067	,253	,027	,263	,793	-,435	,568
	Buitenlandgeschoold_dummy	-,189	,198	-,111	-,952	,343	-,581	,204

Note. Dependent variable: EPE (1–5 scale). Reference categories: control group (treatment); laaggeschoold (education). N=113.

$$EPE_i = \beta_0 + \beta_1 \cdot \text{Meanstested}_i + \beta_2 \cdot \text{Universal}_i + \beta_3$$

Waarbij:

- β_0 = gemiddelde EPE van de controlegroep (= intercept)
- β_1 = verschil in EPE tussen means-tested en controle → test H1a
- β_2 = verschil in EPE tussen universal en controle → test H1b
- ε_i = foutterm

6. Perceived voice opportunities OCMW

Table 6.1 Reliability Statistics – Perceived voice scale

Item	Mean	Std. Deviation	Corrected Item-Total Correlation	Cronbach's Alpha
q_expvoiceOCMW_1	3.11	1.370	0.767	0.764
q_expvoiceOCMW_2	3.20	1.380	0.714	0.788
q_expvoiceOCMW_3	3.09	1.391	0.722	0.784
q_expvoice_4_recode	3.15	1.253	0.527	0.863
Scale	3.14	1.49		$\alpha = 0.845$

Note. N = 55. Scale range: 1–5.

Table 6.2 Moderation analysis: Effect of policy design on EPE, moderated by Perceived Voice_OCMW

Model 1 (hoofdeffecten):

$$EPE_i = \beta_0 + \beta_1 \cdot \text{Meanstested}_i + \beta_2 \cdot \text{Universal}_i + \beta_3 \cdot \text{OCMW_voice_centered}_i + \varepsilon_i$$

Model 2 (+ interactietermen):

$$EPE_i = \beta_0 + \beta_1 \cdot \text{Meanstested}_i + \beta_2 \cdot \text{Universal}_i + \beta_3 \cdot \text{OCMW_voice_centered}_i + \beta_4 \cdot (\text{Meanstested}_i \times \text{OCMW_voice_centered}_i) + \beta_5 \cdot (\text{Universal}_i \times \text{OCMW_voice_centered}_i) + \varepsilon_i$$

Model	R	R ²	Adjusted R ²	SE of the Estimate	R ² change	F change	df1	df2	Sig. F Change
1	,630 ^a	,397	,361	0.66096	0.397	11.182	3	51	<0.001
2	,634 ^b	,402	,341	0.67163	0.005	0.197	2	49	0.822

a. Predictors: (Constant), Exp_voice_scale_centered, universal_dummy, Meanstested_dummy

b. Predictors: (Constant), Exp_voice_scale_centered, universal_dummy, Meanstested_dummy, interact_Exp_Voice_universal, interact_Exp_Voice_meanstested

ANOVA

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	14,655	3	4,885	11,182	<,001 ^b
	Residual	22,280	51	,437		
	Total	36,935	54			
2	Regression	14,832	5	2,966	6,576	<,001 ^c
	Residual	22,103	49	,451		
	Total	36,935	54			

a. Dependent Variable: EPE_Scale

b. Predictors: (Constant), Exp_voice_scale_centered, universal_dummy, Meanstested_dummy

c. Predictors: (Constant), Exp_voice_scale_centered, universal_dummy, Meanstested_dummy, interact_Exp_Voice_universal, interact_Exp_Voice_meanstested

Coefficients		Unstandardized Coefficients		Standardized Coefficients			95,0% Confidence Interval for B	
Model		B	Std. Error	Beta	t	Sig.	Lower Bound	Upper Bound
1	(Constant)	2,392***	,144		16,583	<,001	2,102	2,681
	universal_dummy	,138	,233	,071	,590	,558	-,331	,606
	Meanstested_dummy	-,997***	,204	-,591	-4,886	<,001	-1,407	-,588
	Exp_voice_scale_centered	,081	,081	,109	1,005	,320	-,081	,243
2	(Constant)	2,392***	,147		16,322	<,001	2,098	2,687
	universal_dummy	,128	,238	,066	,538	,593	-,350	,605
	Meanstested_dummy	-,999***	,207	-,592	-4,814	<,001	-1,416	-,582
	Exp_voice_scale_centered	,111	,125	,149	,882	,382	-,141	,363
	interact_Exp_Voice_meanstested	-,015	,179	-,013	-,086	,932	-,374	,344
	interact_Exp_Voice_universal	-,148	,243	-,079	-,610	,545	-,637	,340

a. Dependent Variable: EPE_Scale. Reference category: control group. OCMW_voice_centered = perceived voice scale mean-centered (M = 3.14). * $p < .05$. ** $p < .01$. *** $p < .001$.

7 Perceived Voice opportunities_ Groot Eiland

Table 7.1 Reliability Statistics – Perceived voice scale

Item	Mean	Std. Deviation	Corrected Item-Total Correlation	Cronbach's Alpha
q_tb_1	4,35	,719	,773	,692
q_tb_2	4,58	,657	,591	,750
q_tb_3	4,38	,879	,609	,737
q_tb_4	4,50	,596	,572	,759
q_tb_5_recode	4,03	1,135	,468	,820
Scale	4.37	3.025		α = .790

Note. N = 98. Scale range: 1–5.

Table 7.2 Moderation analysis: Effect of policy design on EPE, moderated by Perceived Voice at Groot Eiland (GE Trajectbegeleider)

Model 1 (hoofdeffecten):

$$EPE_i = \beta_0 + \beta_1 \cdot \text{Meanstested}_i + \beta_2 \cdot \text{Universal}_i + \beta_3 \cdot \text{GE_voice_centered}_i + \varepsilon_i$$

Model 2 (+ interactietermen):

$$EPE_i = \beta_0 + \beta_1 \cdot \text{Meanstested}_i + \beta_2 \cdot \text{Universal}_i + \beta_3 \cdot \text{GE_voice_centered}_i + \beta_4 \cdot (\text{Meanstested}_i \times \text{GE_voice_centered}_i) + \beta_5 \cdot (\text{Universal}_i \times \text{GE_voice_centered}_i) + \varepsilon_i$$

Model	R	R ²	Adjusted R ²	SE of the Estimate	R ² change	F change	df1	df2	Sig. F Change
1	,466 ^a	,217	,193	,75655	0.217	9.073	3	98	<0.001
2	,503 ^b	,253	,214	,74668	0.036	2.304	2	96	0.105

a. Predictors: (Constant), GE_Voice_Scale_centered, universal_dummy, Meanstested_dummy

b. Predictors: (Constant), GE_Voice_Scale_centered, universal_dummy, Meanstested_dummy, interact_GE_Voice_universal, interact_GE_Voice_meanstested

ANOVA

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	15,579	3	5,193	9,073	<,001 ^b
	Residual	56,091	98	,572		
	Total	71,671	101			
2	Regression	18,148	5	3,630	6,510	<,001 ^c
	Residual	53,523	96	,558		
	Total	71,671	101			

a. Dependent Variable: EPE_Scale

b. Predictors: (Constant), GE_Voice_Scale_centered, universal_dummy, Meanstested_dummy

c. Predictors: (Constant), GE_Voice_Scale_centered, universal_dummy, Meanstested_dummy, interact_GE_Voice_universal, interact_GE_Voice_meanstested

Coefficients		Unstandardized Coefficients		Standardized Coefficients			95,0% Confidence Interval for B	
Model		B	Std. Error	Beta	t	Sig.	Lower Bound	Upper Bound
1	(Constant)	2,307	,116		19,967	<,001	2,078	2,536
	universal_dummy	,393	,180	,214	2,184	,031	,036	,751
	Meanstested_dummy	-,622	,182	-,335	-3,411	<,001	-,984	-,260
	GE_Voice_Scale_centered	-,027	,118	-,020	-,228	,820	-,261	,207
2	(Constant)	2,289	,114		20,012	<,001	2,062	2,516
	universal_dummy	,411	,178	,224	2,311	,023	,058	,764
	Meanstested_dummy	-,621	,180	-,334	-3,447	<,001	-,979	-,264
	GE_Voice_Scale_centered	-,381	,211	-,290	-1,805	,074	-,801	,038
	interact_GE_Voice_universal	,369	,313	,141	1,178	,242	-,253	,992
	interact_GE_Voice_meanstested	,588	,274	,298	2,145	,034	,044	1,131

8 Robustness check: Intent to Treat Analysis

Table 8.1 Levene's Test of Homogeneity of Variances

	Levene statistic	df1	df2	p
Omnibus	0.384	2	123	0.682

Note: Levene's test confirms homogeneity of variances ($p > .05$). Analytical sample: $N=126$

Tables 8.2 One-way ANOVA with planned contrasts

ANOVA

	Sum of Squares (Between Groups)	df	Mean square	F	P	ANOVA Effect sizes	
						η^2	95% CI
Omnibus	14.797	2	7.398	12.353	<0.001	0.167	[0.057, 0.276]

Contrast Coefficients

	Treatment_num		
Contrast	Controle	Meanstested	Universal
1: Means-tested vs control	-1	1	0
2 Universal vs control	-1	0	1

Contrast Tests

		Contrast	Value of Contrast	Std. Error	t	df	Sig. (2-tailed)	95% CI Lower	95% CI upper
EPE_Scale	Assumes equal variances	1	-,5145	,16988	-3,029	123	,003	-,8508	-,1782
		2	,3620	,16380	2,210	123	,029	,0378	,6863
	Does not assume equal variances	1	-,5145	,17266	-2,980	77,265	,004	-,8583	-,1707
		2	,3620	,16260	2,227	87,285	,029	,0389	,6852

Contrast effect sizes

	Contrast	Standardizer ^a	Point Estimate	95% CI Lower	95% CI upper
Cohen's d	1	,77391	-,665	-1,102	-,225
	2	,77391	,468	,048	,886
Hedges' correction	1	,77867	-,661	-1,095	-,224
	2	,77867	,465	,048	,880

a. The denominator used in estimating the effect sizes.

Cohen's d uses the pooled standard deviation for all the groups.

Hedges' uses pooled standard deviation for all the groups, plus a correction factor.

Table 8.3 Linear regression model (H1a-H1b)

OLS Regression of External Political Efficacy on Experimental Condition and Educational Level

Model	R	R ²	Adjusted R ²	SE of the Estimate	R ² change	F change	df1	df2	Sig. F Change
1	,409 ^a	,167	,154	,77391	,167	12,353	2	123	<,001
2	,434 ^b	,189	,155	,77346	,021	1,048	3	120	,374

a. Predictors: (Constant), Meantested_dummy, universal_dummy

b. Predictors: (Constant), Meantested_dummy, universal_dummy, Hooggeschoold_dummy, Middengeschoold_dummy, Buitenlandgeschoold_dummy

ANOVA

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	14,797	2	7,398	12,353	<,001 ^b
	Residual	73,669	123	,599		
	Total	88,465	125			
2	Regression	16,677	5	3,335	5,575	<,001 ^c
	Residual	71,788	120	,598		
	Total	88,465	125			

a. Dependent Variable: EPE_Scale

b. Predictors: (Constant), Meantested_dummy, universal_dummy

c. Predictors: (Constant), Meanstested_dummy, universal_dummy, Hooggeschoold_dummy, Middengeschoold_dummy, Buitenlandgeschoold_dummy

Coefficients

		Unstandardized Coefficients		Standardized Coefficients			95,0% Confidence Interval for B	
Model		B	Std. Error	Beta	t	Sig.	Lower Bound	Upper Bound
1	(Constant)	2,287	,111		20,684	<,001	2,068	2,506
	universal_dummy	,362	,164	,202	2,210	,029	,038	,686
	Meanstested_dummy	-,515	,170	-,277	-3,029	,003	-,851	-,178
2	(Constant)	2,415	,191		12,630	<,001	2,036	2,794
	universal_dummy	,297	,171	,166	1,743	,084	-,041	,635
	Meanstested_dummy	-,530	,170	-,286	-3,109	,002	-,868	-,193
	Middengeschoold_dummy	-,247	,220	-,121	-1,125	,263	-,683	,188
	Hooggeschoold_dummy	,131	,239	,054	,546	,586	-,343	,604
	Buitenlandgeschoold_dummy	-,157	,191	-,093	-,819	,415	-,536	,222

Note. Dependent variable: EPE (1–5 scale). Reference categories: control group (treatment); laaggeschoold (education). N=113.

3. Survey

3.1 Treatment vignettes (in Dutch, French, English)

Lees aandachtig het onderstaande nieuwsartikel:



Brusselse Regering Bespreekt Hervorming Huurtoelage

BRUSSEL - De huurtoelage is financiële hulp van de overheid voor mensen die op de wachtlijst staan voor een sociale woning en ondertussen huren op de private markt.

De regering denkt na over een nieuw systeem voor de huurtoelage. In het voorstel zou de toelage:

- **Een recht zijn** voor iedereen op de wachtlijst.
- **Automatisch** worden **uitbetaald**, zonder aanvraag.

Lees aandachtig het onderstaande nieuwsartikel:



Brusselse Regering Bespreekt Hervorming Huurtoelage

BRUSSEL - De huurtoelage is financiële hulp van de overheid voor mensen die op de wachtlijst staan voor een sociale woning en ondertussen huren op de private markt.

De regering denkt na over een nieuw systeem voor de huurtoelage. In het voorstel zou de toelage:

- **Een strenger systeem worden**, enkel voor mensen die **onder een bepaalde inkomensgrens** vallen.

- **Uitbetaald** worden **na aanvraag en extra controle**, om beter **toezicht** te houden.

Lis attentivement l'article ci-dessous:

Gouvernement bruxellois discute de la réforme de l'allocation de loyer



BRUXELLES - L'allocation de loyer est une aide financière du gouvernement pour les personnes qui sont sr la liste d'attente pour un logement social qui louent en attendent un logement sur le marché privé.

Le gouvernement réfléchit à une nouveau système pour l'allocation de loyer. Dans la proposition, l'allocation serait:

- **Un droit** pour toutes les personnes sur la liste d'attente
- **Payée automatiquement**, sans demande

Lis attentivement l'article ci-dessous:

Gouvernement bruxellois discute de la réforme l'allocation de loyer



BRUXELLES - L'allocation de loyer est une aide financière du gouvernement pour les personnes qui sont sr la liste d'attente pour un logement social qui louent en attendent un logement sur le marché privé.

Le gouvernement réfléchit à une nouveau système pour l'allocation de loyer. Dans la proposition, l'allocation serait:

- **Devenir un système plus strict**, seulement pour les personnes qui **ont un revenu en dessous d'un certain montant**.
- être **payée après une demande** et avec **plus de contrôle, pour mieux vérifier**.

Please read the news article below carefully:

The Brussels Times

Brussels Government Discusses Reform of Rent Subsidy

BRUSSELS - *The rent subsidy is financial support from the government for people on the waiting list for social housing who are renting in the private market.*

The government is considering a new system for the rent subsidy. Under the proposal, the subsidy would:

- *Become **a more stringent targeted** benefit, available only to people **below a certain income threshold**.*
- ***Be paid out** after **application and additional verification**, in order to **improve oversight**.*

Please read the news article below carefully:

The Brussels Times

Brussels Government Discusses Reform of Rent Subsidy

BRUSSELS - *The rent subsidy is financial support from the government for people on the waiting list for social housing who are renting in the private market.*

The government is considering a new system for the rent subsidy. Under the proposal, the subsidy would:

- *Become **a right** for everyone on the waiting list.*
- ***Be paid out automatically**, without application.*

3.2. Informed consent form (accessible through hyperlink on first page of the survey)



PERSOONSgegevens - INFORMATIE- EN TOESTEMMINGSFORMULIER

We stellen u voor deel te nemen aan wetenschappelijk onderzoek. Dit onderzoek kadert in het afleggen van een Masterproef van **Kasper Billiet** in een studietraject aan de Vrije Universiteit Brussel (hierna: VUB).

VUB neemt de verantwoordelijkheid op voor de Masterproef en de verwerking van persoonsgegevens, samen met de student, en begeleidt de student bij het verzamelen, analyseren en veilig opslaan van de gegevens.

Indien u deelneemt, kunt u uw deelname op elk moment stopzetten, zonder daarvoor een reden op te geven.

Hieronder vindt u meer informatie over het onderzoek. Aarzel niet om ons bijkomende vragen te stellen.

1. Beschrijving van het onderzoek

- **Titel:** Political Lessons from the Frontline: The Causal Impact of Policy Design on External Political Efficacy among socioeconomically vulnerable citizens
- **Masterstudent:** Kasper Billiet (kasper.billiet@vub.be)
- **Promotor:** Joke Matthieu (joke.matthieu@vub.be)
- **Faculteit:** Economische en Sociale Wetenschappen & Solvay Business School
- **Doel van het onderzoek:** onderzoeken van het causale verband tussen sociaal beleidsdesign en 'external political efficacy'
- **Praktische uitvoering:** Afnemen van een experimentele survey (N=206; geschatte invultijd=10 minuten)

2. Verantwoordelijke voor de verwerking van de gegevens

De student Kasper Billiet (kasper.billiet@vub.be), student politieke wetenschappen, en

Vrije Universiteit Brussel, Pleinlaan 2, 1050 Brussel, ondernemingsnummer 449.012.406

zijn gezamenlijk verantwoordelijk voor deze studie en de bijhorende verwerking van persoonsgegevens.



PERSOONSgegevens - INFORMATIE- EN TOESTEMMINGSFORMULIER

U hebt het recht uw gegevens in te zien, te verbeteren, te wissen, informatie over de verwerking te vragen, een kopie van uw gegevens te vragen, de manier waarop we met uw gegevens omgaan te beperken en ons te vragen uw gegevens aan derden over te dragen.

U hebt steeds het recht de toestemming om uw gegevens te verwerken in te trekken, zonder dat u daarvoor een reden moet geven. In dit geval kunnen we de resultaten verkregen aan de hand van de verwerking van uw gegevens vóór de intrekking van uw toestemming niettemin blijven gebruiken.

De uitoefening van uw rechten kan beperkt worden als dit ons onderzoek ernstig zou bemoeilijken.

10. Contact

Als u vragen hebt, of als u uw rechten wilt uitoefenen, kunt u contact opnemen met Kasper Billiet (kasper.billiet@vub.be / 0477258235) of met de functionaris voor gegevensbescherming van de VUB (dpo@vub.be).

11. Klachten

Als u een klacht wilt indienen over de manier waarop wij met uw gegevens omgaan, kunt u contact opnemen met de onderzoekers, de promotor of de functionaris voor gegevensbescherming (zie contactgegevens hierboven).

Of u kunt een klacht indienen bij de Gegevensbeschermingsautoriteit:

Drukkerstraat 35, 1000 Brussel
tel. +32 2 274 48 00
e-mail: contact@apd-gba.be
website: www.gegevensbeschermingsautoriteit.be

12. Verklaring van de onderzoekers

De onderzoekers bevestigen dat de deelnemer een exemplaar van dit document heeft ontvangen en dat ze alle eventuele vragen beantwoord hebben.

naam: Kasper Billiet
datum: 15/02/2026

handtekening:

☒ *Handtekening*

X Kasper Billiet

Kasper Billiet

Onderskend door: 3d9882b-0ad-4d9f-96c2-34f729a659f



PERSOONSgegevens - INFORMATIE- EN TOESTEMMINGSFORMULIER

3. Voorwaarden voor deelname

niet van toepassing

4. Risico's / persoonlijke voordelen

Deelname houdt geen risico's in.

Aan de deelname zijn geen persoonlijke voordelen verbonden.

5. Vertrouwelijkheid

We behandelen de verzamelde gegevens vertrouwelijk en respecteren de privacywetgeving, waaronder de Algemene Verordening Gegevensbescherming (GDPR).

6. Gegevens - Doel - Bewaartermijn - Overdracht

Voor dit onderzoek verzamelen we de volgende persoonsgegevens:

Leeftijd, geslacht, studieniveau, type contract binnen Groot Eiland, duurtijd van traject binnen Groot Eiland, naam van uw persoonlijke trajectbegeleider, taalniveau Nederlands

We verwerken uw gegevens alleen voor wetenschappelijk onderzoek in het kader van het afleggen van een Bachelor- of Masterproef.

We bewaren uw gegevens gedurende 1 jaar na afloop van het onderzoek.

Uw gegevens kunnen worden hergebruikt voor wetenschappelijk onderzoek in het algemeen belang, in de EU en in de landen met een gelijkwaardige bescherming, erkend door de Europese Commissie, op voorwaarde dat er beschermingsmaatregelen worden genomen.

7. Citaten

Zonder uw uitdrukkelijke toestemming, zullen wij u niet citeren in publicaties.

8. Beveiliging

De opslag van uw gegevens biedt een hoge mate van bescherming, in overeenstemming met de richtlijnen van de VUB (VUB Charter van de onderzoeker, ICT-veiligheidsbeleid).

We zullen uw gegevens zo snel mogelijk pseudonimiseren. Dat betekent dat we gegevens waarmee u rechtstreeks geïdentificeerd kunt worden, zoals uw naam en e-mailadres, vervangen door codes. Er is een sleutel nodig om de codes terug te zetten naar de originele gegevens. Alleen de onderzoekers en de promotor hebben toegang tot deze sleutel.

9. Uw rechten



PERSOONSgegevens - INFORMATIE- EN TOESTEMMINGSFORMULIER

13. Toestemming van de deelnemer

- ☐ Ik heb alle nodige vragen kunnen stellen en ga akkoord met deelname aan het onderzoek.
- ☐ Ik begrijp dat mijn deelname aan het onderzoek vrijwillig is en dat ik mijn deelname op elk moment kan stopzetten, zonder dat ik daarvoor een reden moet geven.
- ☐ Ik ga akkoord met het verzamelen en verwerken van de volgende gegevens voor het onderzoek

Tevredenheidsenquête Medewerkers In Traject Groot Eiland

Start of Block: Welkom

i_informedconsent Bedankt dat je wil meedoen aan deze vragenlijst. We willen graag weten hoe jij de werking van Groot Eiland ervaart: wat loopt goed, wat kan beter, en hoe kunnen wij je nog beter ondersteunen? We horen graag jouw mening.

 Deze vragenlijst wordt uitgevoerd door Groot Eiland in samenwerking met de Vrije Universiteit Brussel voor een onderzoek. Enkele vragen gaan ook over politiek, zodat we beter begrijpen hoe je tegenover de overheid staat.

 ⌚ De vragenlijst duurt ongeveer 6 tot 8 minuten.

 📁 Vertrouwelijk & niet herleidbaar tot jou
 De antwoorden worden vertrouwelijk verwerkt en niet gekoppeld aan jouw identiteit. We noteren enkel algemene gegevens (zoals leeftijdscategorie). Deze info kan niet gebruikt worden om jou persoonlijk te herkennen.

 Een gedetailleerde studiebeschrijving en meer informatie over de persoonsgegevensverwerking in dit onderzoek kan je hier downloaden: informed consent.pdf

q_informedconsent Ik heb de informatie hierboven gelezen en ga akkoord om vertrouwelijk deel te nemen aan deze vragenlijst.

- ☐ Ja, ik doe mee
- ☐ Nee, ik wil niet deelnemen

End of Block: Welkom

Start of Block: Trajectbegeleiding

Q24
 We beginnen met enkele vragen over de samenwerking met je trajectbegeleider. We vragen dit omdat een goede begeleiding erg belangrijk is voor een succesvol traject.

 Geef gerust aan wat je echt vindt, ook als je ergens minder tevreden over bent. Je antwoorden zijn vertrouwelijk en worden niet gedeeld met je begeleider.

q_tb In hoeverre ben je het eens of oneens met de volgende stellingen.
Mijn trajectbegeleider:

	Helemaal oneens	Oneens	Niet eens - niet oneens	Eens	Helemaal eens
houdt rekening met mijn persoonlijke situatie.	☐	☐	☐	☐	☐
luistert naar mijn problemen en vragen.	☐	☐	☐	☐	☐
volgt mij goed op.	☐	☐	☐	☐	☐
geeft duidelijke en juiste uitleg over mijn traject.	☐	☐	☐	☐	☐
begrijpt mij niet	☐	☐	☐	☐	☐

End of Block: Trajectbegeleiding

Start of Block: Vaardigheden en competenties

q_vc
Naast de begeleiding zijn we benieuwd naar wat je tot nu toe hebt geleerd. Deze vragen helpen ons om te zien of het traject bij Groot Eiland je

ook echt sterker maakt in je werk en je dagelijks leven.

In hoeverre ben je het eens of oneens met de volgende stellingen

	Helemaal oneens	Oneens	Niet eens - niet oneens	Eens	Helemaal eens
Ik leer in dit traject wat ik verwacht had.	☐	☐	☐	☐	☐
Door dit traject voel ik dat ik stappen vooruit maak.	☐	☐	☐	☐	☐
Ik kan moeilijke taken uitvoeren als ik er de nodige moeite voor doe.	☐	☐	☐	☐	☐
Ik kan meestal rustig blijven wanneer ik een moeilijke taak krijg.	☐	☐	☐	☐	☐
Ik begrijp wat er op de werkvloer van mij wordt verwacht.	☐	☐	☐	☐	☐
Ik twijfel of ik genoeg vooruitgang maak.	☐	☐	☐	☐	☐

End of Block: Vaardigheden en competenties

Start of Block: Voice opportunities OCMW/Actiris

q_expvoicefilter
Soms krijg je ook ondersteuning van andere organisaties, zoals het OCMW of Actiris. Om een compleet beeld te krijgen van de hulp die je ontvangt, willen we ook graag kort naar jouw ervaringen met hen vragen. Dit helpt ons te begrijpen hoe verschillende diensten in Brussel op elkaar aansluiten.

 Heb je

in de voorbije 2 jaar contact gehad met een sociaal assistent van het OCMW of een
consulent van Actiris?

 Je mag meerdere antwoorden aanduiden

- ☐ Ja, met Actiris
 - ☐ Ja, met OCMW
 - ☐ Nee, geen contact gehad
 - ☐ Ik weet het niet / Ik herinner het mij niet
-

q_expvoiceOCMW_1 Mijn sociaal assistent bij het OCMW luisterde naar wat ik te zeggen
had.

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceOCMW_2 Er was ruimte om mijn bezorgdheden en persoonlijke situatie goed uit te leggen aan mijn sociaal assistent van het OCMW

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceOCMW_3 Mijn sociaal assistent van het OCMW hield rekening met mijn persoonlijke situatie.

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceOCMW_4 Soms leek het alsof mijn mening niet belangrijk was voor mijn sociaal assistent bij het OCMW

- ☐ Helemaal oneens
- ☐ Oneens
- ☐ Niet eens - niet oneens
- ☐ Eens
- ☐ Helemaal eens

Page Break

q_expvoiceActiris_1 Mijn consulent bij Actiris luisterde naar wat ik te zeggen had.

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceActiris_2 Er was ruimte om mijn bezorgdheden en persoonlijke situatie goed uit te leggen aan mijn consulent van Actiris

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceActiris_3 Mijn consulent van Actiris hield rekening met mijn persoonlijke situatie.

- ☐ Helemaal oneens
 - ☐ Oneens
 - ☐ Niet eens - niet oneens
 - ☐ Eens
 - ☐ Helemaal eens
-

q_expvoiceActiris_4 Soms leek het alsof mijn mening niet belangrijk was voor mijn consulent van Actiris

- ☐ Helemaal oneens
- ☐ Oneens
- ☐ Niet eens - niet oneens
- ☐ Eens
- ☐ Helemaal eens

End of Block: Voice opportunities OCMW/Actiris

Start of Block: Intro treatment&PE

i_treatment&PE
De Brusselse overheid neemt elke dag beslissingen over zaken die invloed hebben op jouw leven, zoals hulp organiseren of regels maken voor werk en wonen.

We willen graag begrijpen hoe jij naar deze overheid kijkt. Het gaat hierbij puur om jouw persoonlijke gevoel.

End of Block: Intro treatment&PE

Start of Block: Means tested

tr_means tested_nl <u>Lees aandachtig het onderstaande
 nieuwsartikel:</u>

 <span
 style="font-family:Verdana,sans-serif"><span style="font-
 family:"AlianzaW03-Slab900",serif">Brusselse Regering
 Bespreekt Hervorming Huurtoelage

 <span style="font-
 size:11.0pt">BRUSSEL
 <span style="font-family:"FSIndustrie-
 Regular",serif">- <span style="font-
 family:"FSIndustrie-Regular",serif">De huurtoelage is financiële hulp van de overheid voor
 mensen die op de wachtlijst staan voor een sociale woning en ondertussen huren op de
 private markt.

 <span style="font-
 size:10pt"><span style="font-family:Verdana,sans-
 serif"><span style="font-family:"FSIndustrie-
 Regular",serif">De regering denkt na over een nieuw systeem voor de huurtoelage. In het
 voorstel zou de toelage:

 <font
 face="FSIndustrie-Regular, serif">een strenger
 systeem worden, enkel voor mensen die onder een bepaalde
 inkomensgrens vallen. <font face="FSIndustrie-Regular,
 serif">Uitbetaald worden na
 aanvraag en extra controle, om
 beter toezicht te houden.

End of Block: Means tested

Start of Block: Universal

tr_universal <u>Lees aandachtig het onderstaande nieuwsartikel:</u>

 Brusselse Regering Bespreekt Hervorming
 Huurtoelage

 BRUSSEL - De huurtoelage is financiële hulp van
 de overheid voor mensen die op de wachtlijst staan voor een sociale woning en ondertussen
 huren op de private markt.

 De regering denkt na over een nieuw systeem voor
 de huurtoelage. In het voorstel zou de toelage:
 Een recht
 zijn voor iedereen op de wachtlijst.
 Automatisch worden uitbetaald, zonder
 aanvraag.

End of Block: Universal

Start of Block: Controle

tr_controle <u>ga naar de volgende pagina:</u>

End of Block: Controle

Start of Block: Political efficacy

q_epe <div style="padding-top: 60px;">In hoeverre ben je het eens of oneens met de volgende stellingen over de Brusselse overheidDe groep mensen die een land of stad bestuurt en beslist over regels, geld en wetten. en het beleidBeleid is bijvoorbeeld: regels maken, geld geven, of hulp organiseren voor mensen.?</div>

	Helemaal oneens	Oneens	Niet eens - niet oneens	Eens	Helemaal eens
Mensen zoals ik hebben niets te zeggen over wat de Brusselse overheid doet.	☐	☐	☐	☐	☐
Mensen zoals ik hebben geen enkele invloed op het beleid van de Brusselse overheid.	☐	☐	☐	☐	☐
Politieke partijen zijn enkel geïnteresseerd in mijn stem en niet in mijn mening.	☐	☐	☐	☐	☐
De Brusselse overheid is geïnteresseerd in wat mensen zoals ik denken.	☐	☐	☐	☐	☐
De Brusselse overheid houdt zich bezig met wat mensen zoals ik nodig hebben.	☐	☐	☐	☐	☐

End of Block: Political efficacy

Start of Block: Manipulation check

mc_eligibility Nu volgen twee korte vragen over het nieuwsbericht dat je net las. Zo weten we of de belangrijkste punten uit de tekst duidelijk naar voren kwamen.

Wie komt in aanmerking voor de huurtoelage volgens het nieuwsbericht dat je las?

- ☐ Iedereen op de wachtlijst
 - ☐ Alleen wie onder een inkomensgrens valt.
 - ☐ Ik weet het niet
-

mc_automaticity Hoe zou de betaling gebeuren volgens het nieuwsbericht dat je las?

- ☐ Automatisch (zonder aanvraag)
- ☐ Alleen na aanvraag en controle
- ☐ Ik weet het niet

End of Block: Manipulation check

Start of Block: Algemene tevredenheid

q_algtevr_1 Je bent er bijna! We sluiten af met twee algemene vragen over je totale ervaring. Je antwoorden tot nu toe waren heel waardevol. Geef hier nog een laatste keer aan hoe je in het algemeen tegenover je traject staat.

Ik ben tevreden over mijn traject bij Groot Eiland.

- ☐ Helemaal oneens
- ☐ Oneens
- ☐ Eens
- ☐ Helemaal eens

q_algtevr_2 Ik zou Groot Eiland aanraden aan anderen.

☐ Helemaal oneens

☐ Oneens

☐ Eens

☐ Helemaal eens

End of Block: Algemene tevredenheid
