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Did Latvia's Public Works Program Mitigate the Impact of the 2008–2010 Crisis?

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Abstract

To mitigate the impact of the 2008-2010 global financial crisis on vulnerable households, the Government of Latvia established Workplaces with Stipends, an emergency public works program that targeted registered unemployed people who were not receiving unemployment benefits. This paper evaluates the targeting performance and welfare impacts of the program. It exploits the over-subscription of Workplaces with Stipends to define a control group. The paper finds that the program was successful at targeting poor and vulnerable people, and that leakage to non-poor households was small. Using propensity score matching, the paper finds that the program's stipend mitigated the impact of job loss and, in the short term, raised participating household incomes by 37 percent relative to similar households not benefiting from the program. The paper also finds that the foregone income for this program was less than foregone incomes estimated in other countries. This suggests a dearth of income-generating opportunities in Latvia; thus the program provided temporary employment opportunities and helped the unemployed mitigate the impact of the crisis. However, relative to the depth of the crisis in Latvia, the Workplaces with Stipends program scale was small, which meant long waiting periods for program applicants.

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Did Latvia's Public Works Program Mitigate the Impact of the 2008-2010 Crisis?

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1 Introduction

Latvia was one of the hardest hit countries in the world during the 2008-2010 global financial crisis; during 2008-2010, Latvia's gross domestic product (GDP) contracted by 21 percent. During 2009 alone, Latvia's GDP contracted by 18 percent (Figure 1). Household budget surveys indicate that poverty rates increased by eight percentage points in one year - from 10.1 percent in 2008 to 18.1 percent in 2009 (Ajwad, Haimovich, and Azam, 2012). Net job creation was negative as layoffs rose sharply. Between 2008 and 2010, 126,000 jobs were lost, equivalent to 11.2 percent of the pre-crisis workforce. In 2008 Q3, unemployment rates began to rise and reached a peak of about 21 percent in 2010 Q1, compared to about seven percent in 2007-08. Government administrative data show that 40 percent of workers laid-off were not eligible for unemployment benefits, primarily because they failed to meet the requirement of having paid nine months of contributions into the unemployment insurance fund in a 12-month period.[1]

In September 2009, in response to rapidly rising unemployment rates, the Government of Latvia launched a public works program known as the Workplaces with Stipends (WWS) program, darba praktizēšana, simtlatnieku programma, or the 100-Lats-programma.[2] The program aimed to strengthen the social safety net in response to the unprecedented drop in economic activity.[3] Specifically, the program created temporary labor-intensive employment for people who had lost their jobs but were ineligible for unemployment benefits; also, the program benefitted communities through maintenance activities.

Public works programs are an important safety net intervention used widely around the world. Countries have introduced public works programs with diverse objectives such as

[1] Unemployed workers ineligible for unemployment benefits comprised a variable percentage, but as the crisis dragged on, the percentage rose to more than 50 percent as many more workers lacked sufficient work history required to secure benefits.

[2] Lats is the currency of Latvia, and 1 Lats was $2 on March 31, 2011.

[3] Prior to the crisis and relative to its neighbors, Latvia allocated a very small share of social welfare spending to programs designed to target the poorest households. The low spending on poverty-targeted benefits resulted in low coverage indicators—about six percent of households in the poorest quintile were covered by poverty-targeted safety nets (World Bank, 2010).

protection from large covariate shocks (natural disaster, macroeconomic crisis, or seasonal labor demand shortfalls), to protect households from temporary job losses, fight poverty, or help poor people gain temporary employment (del Nino, Subbarao, and Milazzo, 2009). The key characteristic of a public works program is that national governments, local governments, or non-governmental organizations (NGOs) finance or implement a program that creates temporary employment for people who are willing and able to participate; workers increase their incomes and communities benefit from the resulting public goods new or improved infrastructure, or services delivery.

Existing literature on public works focuses on project design and institutional or administrative arrangements; however, the literature on program effectiveness is relatively thin and primarily focused on low-income countries.[4] Existing, though limited, evidence suggests that public works programs help poor and vulnerable people cope with hard times, especially in the short term; however, evidence of longer term impacts is mixed (Baez et al., 2010).[5] Also, past work has stressed that it is important not to accept net transfers from the program (wage or stipend) as the net household income gain because public works beneficiaries must forgo some income to participate in the program, which requires a time commitment from participants. Because the value of foregone income depends on national context, specific context (prevailing market conditions), and household labor-supply decisions, the effectiveness of public works programs also varies across countries and situations.[6] For example, using a quasi-experimental approach, Jalan and Ravallion (2003) show that in Argentina's Trabajar program, the average net gain to public works participants is about half of the gross wage. Galasso and Ravallion (2004) found that in Argentina's Trabajar and Plan Jefes y Jefas programs, a large share of participants were women who would not otherwise have

[4]See del Nino et al. (2009), Grosh et al. (2008), and Subbarao et al. (2010) for design and administrative set up of various public works programs. Baez et al. (2010) provide a synthesis of existing impact evaluations on public works.

[5]Many of these examples are from Argentina.

[6]Datt and Ravallion (1994) found that other family members took up displaced productive activities when someone joined a workfare program in rural India. Such behavioral responses reduce foregone income.

been participating in the labor force. Therefore, under the Trabajar and Plan Jefes y Jefas programs, about half of the employment gain came from unemployed workers, and the other half arose from inactive workforce participants.

Latvia's public works program is an interesting case study. First, Latvia launched and implemented a public works program in response to a global financial crisis. Second, Latvia provides lessons on how public works programs can respond to a significant deterioration in labor market conditions. Third, relatively few public works program evaluations exist for upper-middle income countries (Latvia is an upper-middle income country) compared to low-income and lower-middle income countries.

This paper evaluates Latvia's WWS program as a crisis response safety net instrument. Subject to data constraints and other restrictions, this paper complements other studies by evaluating the targeting performance of the WWS program and the effectiveness of the WWS program as a crisis mitigation instrument. The paper relies primarily on a unique household survey administered to 3,000 households during December 2010 - March 2011.

The findings of the paper are as follows. First, WWS targeted the poor households very well. Second, WWS was successful at increasing short-term household incomes by 37 percent relative to similar households not benefiting from the program, and helped those households to cope with the crisis. Third, the foregone income was low, and some of the foregone income is accounted for by loss of other safety net payments suggesting that the WWS did not replace other labor opportunities. The WWS program increased household income by LVL 67 while the actual WWS payment was LVL 100 per month. Thus, the Latvian WWS program experience highlights the usefulness of public works programs as short-term safety net instruments during times of crisis, even in upper-middle income countries.

The remainder of this paper is organized as follows: Section 1.1 presents an overview of the WWS program; Section 2 describes the data used in this paper; Section 3 outlines the methodology used to evaluate the WWS program's targeting performance and impacts; Section 4 presents results of targeting performance and short-term program impacts on participant welfare; and Section 5 concludes.

1.1 WWS Program Description

During 2008-10, as the global financial crisis unfolded in Latvia, labor market conditions worsened and unemployment reached historic highs. A large and increasing proportion of registered unemployed people was not receiving unemployment benefits, or was receiving very low benefit amounts (Hazans, 2012). In September 2009, as a safety net response, the Government of Latvia implemented an emergency public works program (WWS) with financial support from the European Social Fund (ESF) and technical assistance from the World Bank. The goal of the WWS program was to reduce the severity of the social consequences of the global financial crisis on Latvians through temporary labor-intensive job creation. Central government expenditures on the WWS program amounted to about LVL 8.0 ($16) million in 2009; LVL 27 ($54) million in 2010; and LVL 20 ($40) million in 2011 (Government of Latvia, 2011).[7] During 2010 and 2011, Government expenditures amounted to about 0.25 percent of GDP, or 2.0 to 2.5 times expenditures on the main poverty-targeted social assistance program. i.e. the Guaranteed Minimum Income (GMI) program (World Bank, 2010). Government administrative data show that the WWS program created more than 110,000 temporary jobs over 2009-11.[8]

All registered unemployed people who were not receiving unemployment benefits were eligible to participate in the WWS program and opportunities were provided on a first-come, first-served basis.[9] The WWS program participants were eligible to participate up to six months with a two week minimum requirement. There was no limit to the number of

[7] On March 31, 2011 the exchange rate was: €1.00: US$1.42: LVL 0.704

[8] There is significant variation in the duration of participation in the WWS program, with some participants completing a few days and others completing the allocated six months before returning to the program to perform another period of participation.

[9] Registered unemployed could also chose WWS instead of unemployment benefits if unemployment benefits were less than the WWS stipend.

times a worker could benefit from WWS.[10] WWS opportunities included public infrastructure maintenance, environmental clean-up, social services (through civil society organizations), and municipal and state services (excluding municipal and state enterprises). In some cases, the WWS program also included a small training component that aimed to improve WWS skills to perform public works tasks. The WWS program was rationed through a self-targeting mechanism with two main components. First, a relatively low stipend was offered to WWS participants so that non-poor people would not crowd out poorer people. The WWS participants earned a stipend of about 80 percent of the binding net minimum monthly wage, or LVL 100 per month (about €142 or US$200).[11] Stakeholders in Latvia initially opposed the stipend on the grounds that it was exceedingly low. However, despite this opposition, the WWS program was always oversubscribed, and the program waiting list was almost double the number of available WWS positions.[12] The stipend of LVL 100 was not subject to taxes or social contributions and all program participants were automatically insured against work-related accidents. Second, WWS tasks were labor-intensive, which helped ration the program by dissuading non-poor people from participating. The program required a labor-intensity rate of about 80 percent; therefore, some maintenance activities were viable, but few if any asset-creation activities qualified. The program's high labor intensity was motivated by the prospect of allowing larger numbers of people to benefit from the program for a given budget.

The WWS program was implemented throughout Latvia and administered by 28 State Employment Affiliates (SEAs). The availability of WWS positions depended on municipal ability to create work sites that met two requirements. First, municipalities had to create "new" positions rather than transfer previously funded functions to the WWS program. This requirement proved challenging in Latvia because municipalities outsource several activities, and the WWS central financing structure was an incentive for municipalities to access it for

[10] After completing six months in a year, a beneficiary can re-register for the WWS program.

[11] In July 2011, the stipend was reduced to LVL 80 per person per month.

[12] No doubt a lower stipend would have resulted in a shorter waiting list, but Government set the rate at LVL 100 to maintain political support.

regular municipal functions.[13] However, there were rules that prohibited replacement. To ensure that WWS positions were newly created, Government provided municipalities with technical assistance to illustrate tasks that were eligible for program support, and followed up with inspections.

2 Data

This paper uses data from a unique household survey commissioned by the State Employment Agency and administered during December 2010-March 2011. The field work for the survey was carried out GfK Custom Research Baltic. The WWS Household Survey represents the population of registered unemployed persons. The sampling strategy required data analysis of the registered unemployed population from State Employment Agency data. All five regions of Latvia - Kurzemes, Latgale, Riga, Vidzemes, and Zemgales - were sampled. Data on the registered unemployed population were divided into four strata within each region:

Strata 1: people enrolled in WWS for less than six months prior to the survey (Treatment 1 or T1)

Strata 2: WWS applicants during August-November 2010 that were wait-listed (Control 1 or C1)

Strata 3: people laid off during August-October 2009, who became WWS beneficiaries and completed a stint of WWS at least six months prior (Treatment 2 or T2)

Strata 4: people laid off during August-October 2009 but did not register for WWS program (were not interested in the WWS program) (Control 2 or C2)

A random sample of 1,000 people was drawn from each Strata 1 (T1) and Strata 2 (C1); and, a random sample of 500 people was drawn from Strata 3 (T2) and Strata 4 (C2). In this paper, we call the individuals who were originally selected in random sampling as *assigned* individuals irrespective of the group they belong to. The questionnaire was administered to

[13]Municipalities outsourcing is particularly common among the larger and wealthier municipalities.

the entire household of the assigned individual, and collected information on all members of the assigned individual's household. The household survey questionnaire resembles other labor force surveys in Latvia and includes questions on education, employment status of household members, detailed questions for WWS participants, household expenditures, and asset ownership; data were collected through face-to-face interviews. The sample comprises 1,166 households in T1; 1,016 households in C1; 463 households in T2; 396 households in C2. Due to the time lapse between sampling and the actual interview, about 396 assigned persons in group T1 finished WWS participation, and about 222 assigned persons from group C1 began participating in WWS. We dropped those assigned persons who have different status than actually assigned. This is done to avoid contamination bias. In addition, about 64 assigned persons in group T1 have an additional household member enrolled in WWS besides the assigned individual. We also dropped these assigned individuals to avoid over estimation of the impact of WWS on household welfare. Similarly, we dropped 22 assigned workers from group C1 as some other household member was enrolled in the WWS program.[14] The final sample size used in the analysis is: T1 - 721; C1 - 769; T2 - 463; and C2 - 396.[15] Omitting some of the assigned individuals (from T1 and C1) to avoid contamination bias raises concerns about selectivity bias. However, as shown in appendix Table A1, we do not find significant differences in the characteristics of individuals who were dropped from T1 from the remaining individuals in T1. We find significant differences only in 3 out of 28 characteristics of those individuals from C1 who were dropped and those who remained in C1. Hence, it is safe to assume that omitting observations to avoid contamination bias does not introduce a selection bias. In this paper, we focus on targeting and short-term income impacts of the WWS program, and for this we compare assigned workers in T1 and C1.[16]

The WWS Household Survey collected aggregate household income information and in-

[14] We are grateful to Mihails Hazans for his advice on sampling.

[15] We keep only those assigned workers in T1 group who have participated in WWS for at least one month. This led to further dropping of 12 assigned individuals from group T1.

[16] Assigned individuals in group T2 and C2 were surveyed to study the medium/longer-term employability impacts of the WWS program.

dividual income components. At the aggregate level, the survey asked households: "What is the total monthly income of your household at the moment?" In addition to the single question on monthly household income, the survey also collected individual income components, including the WWS stipend, through different modules. We define Income-1 as the monthly income as reported by the household and Income-2 as monthly income derived through summation of different income components including WWS stipend. Although the survey collected information on the WWS stipend, it did not collect information on the duration for the WWS stipend payments.[17] Nevertheless, WWS participants received LVL 100 per month and there is no evidence suggesting under or over payments, we added the LVL 100 (LVL 100 for T1; 0 for household in C1) to household monthly income to get total monthly household income including WWS stipend.[18] There are marginal differences in average incomes based on the two income definitions (reported in Table 1). As program impacts differ based on income choice, we report impacts using both income measures. However, our preferred income measure is income-2, because it is obtained by aggregating different income components, and households are more likely to omit the WWS stipend in reporting the total aggregate monthly income.

3 Methodology

3.1 Targeting Performance

To assess WWS program targeting performance, we rank beneficiary households in the welfare distribution of the entire population. However, by design, the WWS Household Survey used in this paper represents *only* the registered unemployed population; the ranking of

[17]It is also not feasible to find out the duration of payment from the time spent in WWS program, as the duration of payment will not exactly match the time spent in the program.

[18]Almost 99 percent of assigned individuals in group T1 reported that payments were correct. As discussed in the data section, only one person (the assigned person) in each household is participating in WWS in the T1 group, and no one in the C1 group is in the WWS program.

sampled households in the overall population is unknown. We therefore combine the WWS Household Survey data with quintile cut-offs from the 2009 Household Budget Survey (HBS). Because the HBS is representative of the entire Latvian population, the quintile cut-offs generated from that welfare distribution can be used to determine the quintile to which the WWS beneficiary household belongs.

3.2 Program Impact: The Impact of the WWS Program on Welfare

This section describes the methodology used to measure the short-term impact of the WWS program on household welfare. This requires that we construct a counterfactual income for participating households in the absence of the program. Since a counterfactual income in the absence of the program is not available, assumptions are made to construct the counterfactual. Assumptions made in program evaluations are often dictated by data availability (Jalan and Ravallion, 2003).

This study exploits the fact that excess demand for the WWS program existed throughout its implementation. The long waiting list for WWS demonstrated that there were people interested in the program, who were similar to WWS participants in their preference for the WWS program, but who were not benefiting from the program. Applicants were wait-listed for the WWS program in the order in which they signed up. These applicants had already indicated a preference for program participation and to some extent had already revealed unobserved factors influencing their choice to participate (Galasso and Ravallion, 2004). However, latent heterogeneity between WWS participants and those on the WWS waiting list may bias impact estimates. As WWS enrolment is on a first-come, first-served basis, the possibility remains that those individuals who were more likely to be impacted by *any* crisis are first to register for the program and the first to participate in the program. To control for observable heterogeneity, propensity score matching is used to construct a counterfactual outcome from the sample of individuals on the waiting list. Following Rosenbaum and Rubin (1983) we use propensity score matching to estimate outcomes without the program (i.e., the average outcome for individuals who did not participate in the WWS program) and compare that outcome to the outcome for observationally similar participants in terms of propensity to participate in WWS program. Propensity is estimated using $Prob(T_i = 1|X_i)$, i.e., probability of participating ($T_i = 1$) in the WWS program conditional on observed (pre-determined) covariates, X_i.

Although propensity score matching (PSM) controls for observable differences, it does not rule out the possibility of selection bias due to unobserved differences between participants and even a well-matched comparison group. We use Rosenbaum bounds (Rosenbaum, 2002) to assess the sensitivity of our results to the selection on unobservables. While other methods exist to assess the sensitivity of PSM estimates to the selection on unobservables, Rosenbaum bounds are computationally attractive and offer an intuitively appealing measure of the way in which unobservables enter the model. In the interest of brevity, and because Rosenbaum bounds have become more widely used in econometric analyses of program evaluation, we have omitted the formal details. Instead, we note that the objective of the method is to obtain bounds on the significance level of a one-sided test for no treatment effect under different assumptions concerning the role of unobservables in the treatment selection process. Specifically, we report upper bounds on the p-value of the null of zero average treatment effect on different values of Γ, where Γ reflects the relative odds ratio of two observationally identical persons participation in the WWS program. Thus, Γ is one in a randomized experiment or in non-experimental data free of bias from selection on unobservables; higher values of Γ imply an increasingly important role of unobservables. For example, $\Gamma = 2$ implies that observationally identical persons can differ by a factor of two in their relative odds of participating in WWS program.

4 Results

4.1 Targeting Performance

The WWS program targets poor households very well: about 80 percent of beneficiaries are poor based on 90 LVL per capita per month needy line (Panel A of Table 2).[19,20] Almost 83 percent of WWS beneficiaries are in the bottom 20 percent of the income distribution and 96 percent of WWS beneficiaries are in the bottom 40 percent of the income distribution. The targeting performance of WWS is very good by international standards. Jalan and Ravallion (2003) find that 75-85 percent of participants in Trabajar program (considered a well-targeted program) in Argentina are poor.

Leakage of benefits to non-poor households is low—about one-fifth of enrolled beneficiaries are classified as non-poor based on the needy line, and less than five percent of all WWS beneficiaries are in the top 40 percent of the income distribution. The income distribution of people on the wait list and of people who have completed the WWS program also reveals good targeting.

All five regions of Latvia perform well in terms of targeting, although targeting performance varies across regions (Panel B of Table 2). Importantly, targeting performance is good in poor regions, such as Latgale. The WWS program targeting performance remains robust to alternative indicators of welfare. For example, most WWS beneficiaries are less educated (highest educational attainment is basic or secondary level), and only a few university graduates report participating in WWS. This educational pattern is not surprising because WWS positions are usually physically demanding. Three-quarters of WWS participants reported that their WWS jobs are physically demanding, hence likely unattractive to

[19] A household is defined as poor if its per capita income (before WWS stipend) is less than 90 LVL per month. Latvia has no official poverty line but the LVL 90 per capita per month is known as the "needy" line.

[20] For targeting performance, we have used calculated measure of income, Income-2, as welfare indicator. The per capita income is derived without the WWS stipend, i.e., (Income-2 - 100)/household size. Targeting performance does not vary much using Income-1, i.e. (Income-1 - 100)/household size.

educated workers.

4.2 Program Impact: The Impact of the WWS Program on Welfare

As discussed earlier, we believe that workers in control group C1 are similar to those in treatment group T1. To confirm this, in Table 3 we check whether the characteristics are similar among individuals in these two groups. Based on 32 ex ante variables, we find statistically significant differences in only 11 variables: age, gender, relationship to head of household, share of household members in 0-5 age range, home ownership, or living in a house held by private entity, and residing in different regions.[21] These findings suggest that although the assigned individuals in the treatment and control groups have characteristics that are statistically similar, they also differ significantly in some observed characteristics. Therefore, we control for observed differences before comparing outcomes of WWS participants and non-participants.

To control for observable heterogeneity, we adopt a propensity score matching technique to construct a counterfactual outcome from the sample of individuals on the waiting list. First, we estimated a probit model for calibrating the propensity score on the pooled sample of assigned individuals in the treatment and control groups. The complete model is reported in Table 4. The explanatory power of the model is low, suggesting that the two groups of individuals are similar with respect to many observed characteristics. Most explanatory variables have insignificant coefficients; geography, gender, relationship to household head, share of household members in 0-5 age range, and higher education have significant impact on WWS participation.[22] Propensity score results confirm our expectations from the simple averages reported in Table 3, as there are no differences in simple averages of many

[21]Technically, these variables were recorded at the time of interview; however, we do not expect these variables to have changed because of participation in short term WWS program.

[22]If the treatment was assigned randomly, none of the covariates is expected to significantly affect participation.

characteristics, and they are insignificant in the propensity score model.

As expected, we find considerable overlap in support between the treatment and control groups across the entire region (Figure 2). Table 5 explores whether the model has balanced all ex ante variables, i.e., we calculate differences between treatment and control groups for each characteristic in the matched sample. Conditioning variables are balanced, as indicated by the t-tests in Table 5, Panel A. Matching balances differences observed in the raw data; in the matched sample, no significant difference remains between treatment group and control group. Matching also significantly reduced standardized bias (SB). In most empirical studies, a SB below three percent, or five percent after matching, is seen as sufficient (Caliendo and Kopeinig, 2008). In our case, the SB is below three percent for almost all covariates. We reported another test in Panel B of Table 5. Following Sianesi (2004), we re-estimated the propensity score on the matched sample, i.e., only on participants and matched non-participants, and compare the $pseudo - R^2$s before and after matching. The $pseudo - R^2$ indicates how well the regressors explain the probability of participation. After matching, no systematic differences should exist in the covariate distribution between the two groups, therefore the $pseudo - R^2$ should be low. In our case, the $pseudo - R^2$ indeed approaches zero after matching.

Thus the diagnostic analysis reveals that matching controls for differences in unmatched data. In Table 6, we present the average impact of the WWS program on short-term household incomes.[23] We use Kernel matching (KM), which is a nonparametric matching estimator that uses weighted averages of (nearly) all - depending on the choice of the kernel function - individuals in the control group to construct the counterfactual outcome.[24] Thus, one major advantage of these approaches is the lower variance, which is achieved because more information is used.

Households in the treatment group earn about 37 percent more than the households in

[23]We use psmatch2 (Leuven and Sianesi, 2003) in STATA to get the PSM estimate.

[24]To implement the matching estimator, we used kernel weighting with the epanechnikov kernel and a fixed bandwidth of 0.10. Confidence intervals are obtained using 100 bootstrap repetitions.

the control group households (based on income-2 measure). Thus, the WWS program acted as an effective short-run safety net (i.e., during program enrollment). The average gain for participant households was about LVL 67 (income-2 definition), which was about two-thirds of the WWS stipend. Thus foregone income is about LVL 33. Since some income is foregone, the targeting performance reported in the earlier section (based on zero foregone income) overestimates the WWS program pro-poor finding. In Table 7, we present the average gains in WWS program based on some other estimators, and we find similar gains based on other matching estimators. Thus, net gain from the WWS program is not sensitive to the choice of the estimator. Jalan and Ravallion (2003) found different average gains in the Trabajar program in Argentina based on different matching estimators. They found a gain of AR$157 (three-quarters of Trabajar wage) using nearest estimator, while about AR$100 gains using other estimators.

Although the PSM estimation controls for selection of observables, any selection of unobservables can bias results. We assessed the sensitivity of our results to the selection of unobservables using Rosenbaum bounds. Table 8 reports the upper bound on the p-value of the null of zero average treatment effect for different values of Γ. If the upper bound on the p-value is less than, say, 0.10 for reasonably large values of Γ, then the treatment effect is said to be robust to hidden bias. We find the income gains to be sensitive to hidden bias only if Γ is larger than 2. While the Rosenbaum bounds do not yield point estimates of the treatment effects once hidden bias is taken into account, they increase confidence in WWS program impacts because the positive impact of the WWS program on income is robust to a large selection bias of unobservables.

The foregone income in the WWS program is lower than foregone income estimates for public works programs in other countries. For example, Chacaltana (2003) found that the net gain derived from the Trabajar Urbano program in Peru was equal to 24 percent of the nominal transfer. Beneficiaries received a monthly salary of 300 soles, while their control group was able to generate 227 soles on their own, in absence of the program. In 2002, in the Jefes program in Argentina, estimated net income benefit (after accounting for foregone participant income) was two-thirds of the AR$150 benefit. By May 2003, net income benefit had fallen to one-third of the transfer (Galasso and Ravallion, 2004, and Galasso, 2004). In the Empleo in Action program in Colombia, participant monthly employment income increased on average close to 39 percent over what would be earned without a program; but income was much higher for women (90 percent) and for youth between 18 and 25 year old (54 percent) (Departamento Nacional de Planeacion, Colombia, 2004).

Relative to other countries, foregone income in the WWS program in Latvia is low. This suggests that the control group was unable to generate income, likely because of a lack of labor market opportunities, which was also reflected in historically high unemployment rates; and because of the low coverage and benefits of poverty-targeted social assistance programs.[25] However, some foregone income can be explained by the loss of other safety net income for participants, e.g., guaranteed minimum income (GMI). For example, the treatment households earn 6 LVL less (4 LVL) compared to control group households from GMI (unemployment benefits) (Table-9). Moreover, the households that qualify for other safety nets might prefer the WWS program because it offers higher benefits. When households choose WWS, they lose top-up benefits such as those available under GMI. However, municipalities in Latvia might encourage WWS participation because the WWS costs are borne entirely by the central government, whereas municipal governments co-finance GMI benefits. Also, the work requirement makes the safety net politically acceptable.

In addition to looking at WWS program impact on income gains, we also looked at the impact on subjective measures such as coping strategies adopted by the households during the crisis (Table 10).[26] Particularly clear is the WWS program impact on nutrition and

[25]About 83 percent of workers who have participated or are enrolled in the WWS program report they had to wait; almost 46 percent of workers report waiting six or more months before participating. Among people who are now waiting, more than three-quarters report waits of three or more months, and about 34 percent have been waiting for six or more months. This suggests that for many of workers, WWS was the only opportunity available.

[26]Households were asked whether they adopted any coping strategy to mitigate the crisis impacts, and a number of possible strategies were numerated with the option of yes or no.

health outcomes: a lower proportion of households participating in WWS reported reducing their food intake (quantity and frequency), or reducing doctor visits (preventive and during illness) than households in the control group. This further confirms that the WWS program acts as an important safety net for WWS beneficiary households.[27]

5 Conclusion

Latvia was one one of the hardest hit countries during the 2008-2010 global financial crisis. GDP contracted by 18 percent in 2009; poverty rates increased by about 8.0 percentage points in 2009; and in 2010 Q1, the unemployment rate was almost triple the pre-crisis unemployment rate. To mitigate the impact of the crisis on vulnerable households, the Government of Latvia established an emergency public works program targeting registered unemployed people who were not receiving unemployment benefits. Between 2009 and end-2011, the WWS program created over 110,000 temporary jobs. This paper assesses the targeting performance and welfare impacts of the WWS program using a unique household survey collected between December 2010 and March 2011.

We find that the WWS program was successful at targeting poor and vulnerable people, and leakage to non-poor households was small. The relatively low stipend and labor-intensive works helped ration the program and deterred non-poor people from participating. To measure the WWS program impact, this paper exploits excess demand for WWS to construct a counterfactual group. Using a propensity score matching (PSM) method, we find that the WWS program raised short-term household incomes by 37 percent relative to similar households not benefiting from the program. The WWS program increased household income by LVL 67 while the actual WWS payment was LVL 100 per month. Thus, participants forego some income (about LVL 33) to participate in WWS; some of this foregone income is

[27]Households reported that the WWS program is useful as a safety net and as a program to uplift the local community. Most participants view the WWS program as an important safety net, and 96 percent believe that WWS projects are beneficial to the community.

due to loss of other safety net payments such as guaranteed minimum income benefits.

Foregone income due to the WWS program participation is lower than foregone incomes estimated elsewhere. This suggests that non-participants, who were on the waiting list, were unable to find alternate income-generating options. Long waiting lists for the WWS program also corroborate the finding that workers had very limited options. Thus the WWS program provided employment opportunities when prospects in the labor market were limited, and transferred additional income to beneficiary households. The WWS program experience highlights the usefulness of public works programs as short-term safety instruments during times of labor market crisis, even in upper-middle income countries. The WWS program experienced long waiting lists and large numbers of workers re-registered, which demonstrates that people valued the WWS program and that the program was too small given the devastating impact of the crisis.

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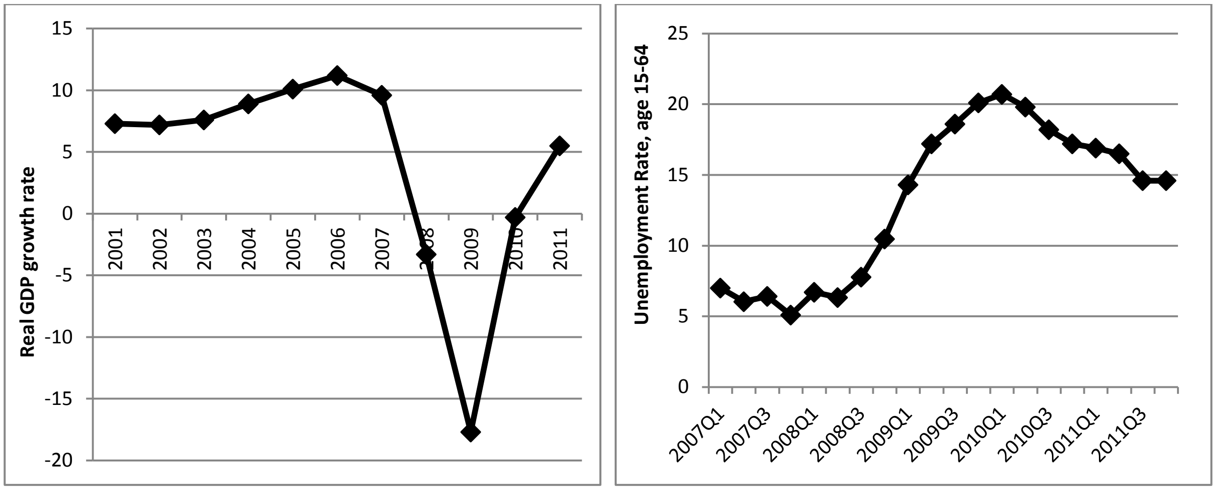
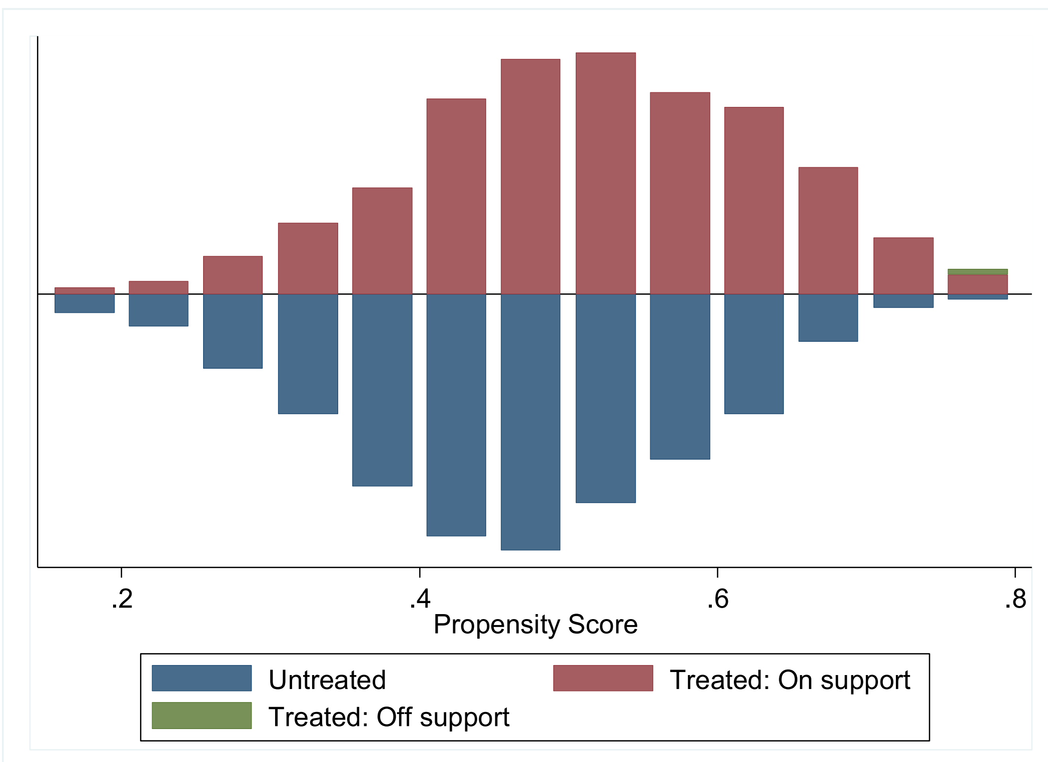
Figure 1: Economic growth and unemployment rates in Latvia



Source: Eurostat.

Figure 2: Overlapping support in the distribution of the propensity score



Note: Histogram of propensity score distribution for WWS participants and WWS waiting list; 3 (0.2%) of the participants are off the common support.

Table 1: Household monthly income, definitions

	Description	T1	C1	T1 and C1
		Currently in WWS	On waiting list	Total
Income-1	Monthly aggregate income reported by household (LVL)	237	202	220
Income-2	Monthly aggregate income calculated adding individual components (LVL)	249	183	219

Table 2: Targeting performance of WWS Program

	Quintile							
	Poorest	Quintile 2	Quintile 3	Quintile 4	Richest	Poor	Non-poor	Total
Panel A: Latvia								
T1-Currently in WWS	*83.0*	*13.0*	*2.1*	*1.5*	*0.4*	*80.2*	*19.8*	*100*
C1-On waiting list for WWS	*76.6*	*15.7*	*3.2*	*3.4*	*1.1*	*73.2*	*26.8*	*100*
T2-Completed WWS	*80.6*	*12.1*	*2.9*	*3.8*	*0.7*	*75.2*	*24.8*	*100*
Panel B: Regions								
Kurzemes								
T1-Currently in WWS	82.9	11.9	2.9	2.4	0.0	80.8	19.3	100
C1-On waiting list for WWS	72.3	19.6	3.8	3.8	0.5	68.8	31.2	100
T2-Completed WWS	70.9	19.7	2.6	5.1	1.7	64.5	35.5	100
Latgale								
T1-Currently in WWS	87.7	8.6	2.5	1.2	0.0	87.7	12.4	100
C1-On waiting list for WWS	79.0	14.0	2.6	4.4	0.0	76.5	23.5	100
T2-Completed WWS	79.3	12.1	5.2	3.5	0.0	76.3	23.7	100
Riga								
T1-Currently in WWS	80.5	14.3	3.0	1.5	0.8	75.7	24.3	100
C1-On waiting list for WWS	72.3	16.2	4.7	3.7	3.1	66.2	33.9	100
T2-Completed WWS	81.7	12.2	2.4	3.7	0.0	74.1	25.9	100
Vidzemes								
T1-Currently in WWS	80.5	16.7	1.2	0.6	1.2	77.8	22.2	100
C1-On waiting list for WWS	81.0	13.9	2.2	2.2	0.7	79.6	20.4	100
T2-Completed WWS	83.3	9.4	2.1	4.2	1.0	76.3	23.7	100
Zemgales								
T1-Currently in WWS	87.6	10.7	0.8	0.8	0.0	83.7	16.3	100
C1-On waiting list for WWS	83.7	12.2	1.6	2.4	0.0	81.6	18.4	100
T2-Completed WWS	89.5	5.3	3.2	2.1	0.0	87.5	12.5	100

Note: Per capita income is calculated without WWS stipend. A household is considered poor if its per capita income (without WWS stipend) is less than 90 LVL per month. The quintiles cut-offs are derived using the income distribution of Household Budget Survey-2009 (the inflation between 2009 and 2010 has been about zero, hence no adjustment is made in HBS-2009 cut offs).

Table 3: Difference in ex-ante variables, before matching

	Mean			T1=C1	
Variable	T1	C1	%bias	t	p>t
Age 18-24	0.094	0.149	-17.0	-3.27	0.00
Age 25-29	0.066	0.084	-6.9	-1.34	0.18
Age 30-39	0.178	0.139	10.7	2.07	0.04
Male	0.383	0.468	-17.3	-3.34	0.00
Household head	0.680	0.678	0.5	0.10	0.92
Spouse of household head	0.204	0.153	13.3	2.56	0.01
Single	0.193	0.252	-14.2	-2.74	0.01
Married	0.331	0.313	3.8	0.74	0.46
Share of members in age 0-5	5.040	6.531	-11.7	-2.25	0.02
Share of members in age 6-17	10.799	10.802	0.0	0.00	1.00
Share of members in age 1864	78.453	77.219	5.0	0.96	0.34
Household size	2.884	2.881	0.2	0.04	0.97
Unemployed 12 months ago	0.439	0.466	-5.3	-1.02	0.31
Secondary education	0.307	0.278	6.3	1.22	0.22
Secondary profession	0.390	0.389	0.1	0.02	0.98
Higher education	0.051	0.075	-10.0	-1.92	0.06
Own a flat	0.343	0.364	-4.4	-0.85	0.40
Own a house	0.152	0.096	16.8	3.24	0.00
House is owned by state	0.240	0.222	4.3	0.82	0.41
House is owned by private entity	0.132	0.185	-14.4	-2.77	0.01
Have other Dwelling in other parts of Latvia	0.084	0.075	3.3	0.63	0.53
House have 1 room	0.229	0.232	-0.8	-0.16	0.87
2 room	0.416	0.416	0.1	0.02	0.99
3 room	0.227	0.251	-5.5	-1.06	0.29
Detached/semi-detached House	0.417	0.437	-3.9	-0.75	0.45
Flat in apartment	0.025	0.025	-0.2	-0.03	0.97
Wooden wall	0.198	0.198	0.1	0.02	0.98
concrete wall	0.227	0.252	-5.8	-1.11	0.27
region==Latgale	0.112	0.149	-11.2	-2.14	0.03
region==Riga	0.186	0.256	-16.9	-3.25	0.00
region==Vidzemes	0.242	0.181	15.1	2.92	0.00
region==Zemgales	0.167	0.165	0.5	0.09	0.93

Note: Standardized bias (SB) for each variable is defined as the difference of sample means in the treated and control subsamples as a percentage of the square root of the average of sample variances in both groups.

Table 4: Probit for calibrating propensity score

	Coefficient	Standard error
Age 18-24	-0.172	(0.135)
Age 25-29	-0.020	(0.146)
Age 30-39	0.171	(0.105)
Male	-0.198***	(0.071)
Household head	0.268**	(0.120)
Spouse of household head	0.413***	(0.144)
Single	-0.057	(0.101)
Married	-0.077	(0.085)
Share of members in age 0-5	-0.009**	(0.004)
Share of members in age 6-17	-0.004	(0.003)
Share of members in age 1864	0.000	(0.002)
Household size	0.063*	(0.034)
Unemployed 12 months ago	-0.067	(0.068)
Secondary education	0.034	(0.094)
Secondary profession	-0.009	(0.093)
Higher education	-0.291*	(0.155)
Own a flat	-0.024	(0.177)
Own a house	0.287**	(0.143)
House is owned by state	-0.006	(0.133)
House is owned by private entity	-0.193	(0.143)
Have other Dwelling in other parts of Latvia	0.107	(0.124)
House have 1 room	0.056	(0.144)
2 room	0.016	(0.129)
3 room	-0.111	(0.127)
Detached/semi-detached House	-0.003	(0.133)
Flat in apartment	-0.102	(0.237)
Wooden wall	-0.113	(0.093)
concrete wall	-0.031	(0.085)
region==Latgale	-0.269**	(0.115)
region==Riga	-0.266***	(0.100)
region==Vidzemes	0.116	(0.099)
region==Zemgales	-0.121	(0.105)
Constant	-0.119	(0.309)
Number of observations	1,484	
Pseudo R-Square	0.0417	

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Balancing tests: difference in ex-ante variables after matching

Panel A

Variable	Mean Treated	Mean Control	%bias	%reduction |bias|	T1=C1 t	T1=C1 p>t
Age 18-24	0.095	0.104	-3.0	82.2	-0.62	0.53
Age 25-29	0.065	0.071	-2.3	67.5	-0.45	0.66
Age 30-39	0.175	0.169	1.7	83.9	0.32	0.75
Male	0.385	0.392	-1.4	92.2	-0.26	0.80
Household head	0.684	0.675	2.0	-303.8	0.38	0.70
Spouse of household head	0.199	0.196	0.8	94.1	0.14	0.89
Single	0.195	0.201	-1.5	89.6	-0.29	0.77
Married	0.328	0.333	-1.0	74.9	-0.18	0.86
Share of members in age 0-5	5.066	5.322	-2.0	82.8	-0.41	0.68
Share of members in age 6-17	10.637	10.691	-0.3	-1543.1	-0.06	0.96
Share of members in age 1864	78.533	78.316	0.9	82.4	0.17	0.87
Household size	2.873	2.909	-2.4	-1077.0	-0.44	0.66
Unemployed 12 months ago	0.440	0.445	-1.1	78.5	-0.22	0.83
Secondary education	0.309	0.306	0.7	89.4	0.12	0.90
Secondary profession	0.389	0.388	0.4	-184.8	0.07	0.95
Higher education	0.051	0.054	-0.9	91.3	-0.18	0.86
Own a flat	0.344	0.358	-3.0	32.6	-0.57	0.57
Own a house	0.150	0.139	3.4	80.0	0.59	0.55
House is owned by state	0.241	0.233	1.8	58.2	0.34	0.74
House is owned by private entity	0.134	0.136	-0.6	96.1	-0.11	0.91
Have other Dwelling in other parts of Latvia	0.082	0.085	-1.0	69.6	-0.18	0.85
House have 1 room	0.229	0.222	1.8	-113.4	0.34	0.73
3 room	0.416	0.420	-0.9	-952.1	-0.17	0.87
4 room	0.227	0.239	-2.8	49.4	-0.53	0.60
Detached/semi-detached House	0.419	0.431	-2.6	34.3	-0.49	0.63
Flat in apartment	0.025	0.025	-0.3	-57.6	-0.05	0.96
Wooden wall	0.200	0.203	-0.6	-417.3	-0.11	0.91
concrete wall	0.229	0.239	-2.2	62.8	-0.41	0.68
region==Latgale	0.111	0.118	-2.0	82.5	-0.39	0.70
region==Riga	0.188	0.188	-0.1	99.7	-0.01	0.99
region==Vidzemes	0.238	0.232	1.3	91.1	0.25	0.81
region==Zemgales	0.168	0.174	-1.5	-226.8	-0.29	0.77

Panel B

Sample	Pseudo R2	LR chi2	p>chi2
Unmatched	0.042	85.8	0.0
Matched	0.001	2.97	1.0

Note: Standardized bias (SB) for each variable is defined as the difference of sample means in the treated and control subsamples as a percentage of the square root of the average of sample variances in both groups. Reduction in bias refers to the percentage reduction in bias after matching.

Table 6: Average impact of WWS on incomes

		E(Y1|T=1)	E(Y1|T=0)	Matched Difference	Bootstrap Standard error	P-value	Normal-based [95% Confidence Interval]	
Household income								
Income-1	Monthly aggregate income reported by household	236.74	200.31	36.42	8.17	0.00	20.40	52.45
Income-2	Monthly aggregate income calculated adding individual income components	248.53	181.57	66.96	9.01	0.00	49.31	84.62
Per capita income								
Income-1	Monthly aggregate income reported by household	92.23	73.11	19.13	2.71	0.00	13.82	24.43
Income-2	Monthly aggregate income calculated adding individual income components	97.97	62.86	35.10	3.29	0.00	28.78	41.43

Note: The standard errors are derived via bootstrapping with 100 replications.

Table 7: Impact of WWS program, alternative matching methods

Income-2	
With replacement:	
Nearest neighbor	70.77
	(13.39)
5-nearest neighbor	63.42
	(10.19)
Caliper (б=0.001)	67.55
	(11.00)
Caliper (б=0.01)	64.72
	(9.57)

Table 8: Sensitivity of PSM estimates with respect to unobservables, Rosenbaum Bounds

Income-3							
Γ	1.0	1.2	1.4	1.6	1.8	2.0	2.2
P-value	0.000	0.000	0.000	0.000	0.007	0.092	0.375

Note: Rosenbaum critical p-values for test of the null of zero average treatment effect on treated (ATT) . For controls included in the propensity score, see Table 2.

Table 9: Difference in household income components

	Treatment group (T1)	Control group (C1)	Matched Difference (ATT)	Standard Error	T-stat
Labor income	68.0	94.6	-26.6	7.5	-3.6
Income from WWS	100.0	0.0	100.0	-	-
Income from informal sources	3.5	4.6	-1.1	0.9	-1.2
Income from other sources	7.5	8.3	-0.8	2.3	-0.4
Pension	41.5	35.4	6.0	4.5	1.3
Social transfers					
Guaranteed Minimum Income (GMI)	4.2	9.8	-5.6	1.5	-3.8
Housing Allowance	1.2	2.4	-1.2	0.7	-1.7
Heating Allowance	3.9	4.1	-0.2	1.1	-0.2
School Meals	1.8	3.0	-1.3	0.7	-1.7
Other municipal assistance	1.6	1.4	0.1	1.0	0.2
Children's Allowance	1.3	1.1	0.1	0.6	0.2
Parental/maternal Benefit	0.4	0.4	0.0	0.4	-0.1
State Family Benefit	5.4	5.2	0.2	0.5	0.5
Other family state benefit	1.2	2.3	-1.1	0.7	-1.5
Unemployment Benefit	0.4	4.0	-3.6	0.8	-4.5
Sickness Benefit	0.5	1.0	-0.5	0.5	-0.9
Disability Benefit	3.6	2.5	1.1	1.1	0.9
Other state benefit	2.1	1.2	0.9	0.7	1.3

Table 10: Coping strategy adopted by Treatment (T1) and Control (C1) households

	Difference in adoption rate (T1-C1)	T-stat (T1=C1)
Reduced consumption of food staple	-7.31	2.70
Skipped meals	-8.12	
Reduced lighting/heating/water consumption	-5.73	2.13
Reduced entertainment consumption	-2.04	0.78
Bought less clothes	-3.62	1.40
Withdrew preschool kid	-1.08	1.39
Withdrew from university	-0.21	0.24
Withdrew from training classes	-0.97	0.86
Reduced educational expenditures	-0.64	0.39
Reduced doctor's appointments (preventive)	-6.68	2.61
Reduced doctor's appointments (when ill)	-3.54	1.36
Stopped buying medicine	-5.14	1.99
Cancelled phone service	-3.21	2.26
Postponed investments in business	-1.13	1.05
Reduced help to friends	-3.36	1.95
Cut TV service	-4.23	3.25
Change transportation mode	-1.49	1.26
Cut internet service	0.28	0.21

Note: The difference is the difference in percentage of the households in Treatment and Control group reporting using the particular strategy as a response to crisis.

Appendix

Table A1: Difference between assigned individuals in final sample and dropped assigned individuals

	Remaining T1-Dropped T1	T-test: Remaining T1=Dropped T1	Remaining C1-Dropped C1	T-test: Remaining C1=Dropped C1
	Difference	T-Stat	Difference	T-Stat
Age 18-24	-0.004	(-0.25)	-0.046	(-1.83)
Age 25-29	0.004	(0.23)	-0.004	(-0.18)
Age 30-39	0.024	(1.00)	0.050	(1.90)
Male	0.047	(1.56)	-0.035	(-0.96)
Household head	0.006	(0.23)	0.022	(0.63)
Spouse of household head	-0.032	(-1.34)	-0.008	(-0.29)
Single	0.025	(1.02)	-0.057	(-1.82)
Married	0.042	(1.45)	-0.035	(-1.05)
Share of members in age 0-5	-0.459	(-0.65)	-0.931	(-0.95)
Share of members in age 6-17	0.525	(0.48)	1.089	(0.77)
Share of members in age 1864	-0.368	(-0.25)	0.966	(0.53)
Household size	0.043	(0.44)	0.013	(0.12)
Unemployed 12 months ago	0.045	(1.48)	-0.066	(-1.82)
Secondary education	-0.004	(-0.15)	-0.011	(-0.34)
Secondary profession	-0.026	(-0.90)	-0.000	(-0.00)
Higher education	0.026	(1.77)	-0.050**	(-2.74)
Own a flat	0.007	(0.25)	-0.023	(-0.64)
Own a house	-0.033	(-1.57)	0.014	(0.66)
House is owned by state	0.011	(0.43)	0.070*	(2.26)
House is owned by private entity	-0.005	(-0.27)	-0.046	(-1.66)
Have other Dwelling in other parts of Latvia	0.002	(0.11)	0.011	(0.56)
House have 1 room	0.008	(0.30)	-0.013	(-0.42)
2 room	-0.018	(-0.61)	0.060	(1.67)
3 room	0.038	(1.46)	-0.030	(-0.95)
Detached/semi-detached House	0.012	(0.39)	-0.005	(-0.14)
Flat in apartment	-0.007	(-0.70)	-0.018	(-1.68)
Wooden wall	0.015	(0.62)	0.105***	(3.46)
Concrete wall	-0.027	(-1.10)	-0.059	(-1.90)
Number of observations	1163		1016	

Note: t statistics in parentheses. * p<0.05, **p<0.01, *** p<0.001