Chapter 11

Public Works Programs

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1 Introduction

Public works programs (PWP), also called workfare or cash-for-work programs, are government policies that provide employment on small infrastructure projects to support the income of the poor. They are one of the most popular forms of anti-poverty policy around the world: the Report on the State of Safety Nets by The World Bank (2015) found them active in 94 countries. There is also a long tradition of using PWP in economic downturns, from droughts in pre-colonial South Asia (Dreze and Sen, 1991), to the 1930s depression in the US (Fishback, 2017), and the 2001 financial crisis in Argentina (Galasso and Ravallion, 2004). Throughout the 19th century workhouses were the main if not the only option for the poor to receive public

assistance in England, Europe and North America (Ravallion, 2015).

The justifications for imposing work requirements rather than just giving cash to the poor were moral and political, but also economic: workhouses disincentivized the poor from relying on assistance for too long and used their workforce for productive purposes. Besley and Coate (1992) formalized these arguments to explain how "workfare" (PWP) could achieve better pro-poor targeting than "welfare" (cash transfers) when governments lack information on who is poor. The largest contributor to our understanding of PWP is Martin Ravallion. He proposed a general theory and brought the first empirical evidence in the 1990s (Ravallion, 1990, 1991; Datt and Ravallion, 1994). He kept pushing the research frontier through the 2000s (Jalan and Ravallion, 2003; Galasso and Ravallion, 2004; Ravallion et al., 2005) and the 2010s (Murgai et al., 2016; Dutta et al., 2014; Alik-Lagrange and Ravallion, 2018).

We review the theoretical and empirical literature on PWP. They have attracted a lot less attention than cash transfers, which is why many of the papers we review are recent, oftentimes still working papers. On the methodological side also the evaluation of PWP lags that of cash transfer, with few well-powered experimental designs, and many small-scale, often quasi-experimental studies. The paucity of studies is problematic because PWP can take very different forms, some are short-term while others are permanent, some are universal while others are targeted, some define eligibility at the

household level and others at the individual level etc. Table 1 lists the PWP included in this review, the context they are implemented in, the main features of their design, and the type of evaluation. The evidence base is skewed towards India's MG-NREGS, and Ethiopia's PSNP, both permanent programs, and most programs were studied only once.

Table 1: List of PWP included in this review

Name	Country	Context	Duration	Eligibility	Targeting	Evaluation	Studies
MG-NREGS	India	Rural	Permanent	Household	Universal	Quasi-Experimental*	38
PSNP	Ethiopia	Rural	Permanent	Household	Community	Quasi-Experimental	7
Maharashtra's EGS	India	Rural	Permanent	Household	Universal	Quasi-Experimental	4
Trabajar / Jefes y Jefas	Argentina	Both	Temporary	Individual	Universal	Quasi-Experimental	3
PEJDEC	Cote d'Ivoire	Urban	Temporary	Individual	Youth only	Experimental	1
CWLP	Tunisia	Rural	Temporary	Individual	NGO	Experimental	1
UPSNP	Ethiopia	Urban	Temporary	Household	Community	Experimental	1
PEJDC	Burkina Faso	Urban	Temporary	Individual	Youth only	Experimental	1
Empleo en Accion	Colombia	Both	Temporary	Individual	Lotteries	Quasi-Experimental	1
MASAF	Malawi	Rural	Temporary	Household	Community	Experimental	1
STEP LIPW	DRC	Both (Post-Conflict)	Temporary	Individual	Universal	Experimental	1
Londo	CAR	Both (Post-Conflict)	Temporary	Individual	Lotteries	Quasi-Experimental	1
RMG	Lao PDR	Rural	Temporary	Individual	Women only	Experimental	1
UYEP	Papua New Guinea	Urban	Temporary	Individual	Youth only	Quasi-Experimental	1

* There are a few experimental studies on the MG-NREGS (Banerjee et al., 2017, 2020; Muralidharan et al., 2016, 2023; Field et al., 2021), but most of the literature is quasi-experimental.

Our objective is to identify where a consensus has emerged, where the evidence is still inconclusive and where more research is needed. Whenever there is enough evidence, we describe how the context of implementation or the design features of PWP affect their impacts on beneficiaries and the broader community. We build on at least three recent reviews devoted to MG-NREGS (Sukhtankar, 2017), to PWP effects on productivity (Gehrke and Hartwig, 2018) and to short and long-term effects of PWP for participating households (Bagga et al., 2023).[1] Although our focus is on developing countries today, there are interesting parallels with the evaluation of New Deal programs in the 1930s US (Fishback, 2017).

2 Targeting, costs and benefits for participants

Self-targeting: In the absence of reliable information on households' wealth and income levels, social protection administrations in low and middle-income settings often rely on ordeal mechanisms. As early as 1834, the English Poor Laws conditioned access to poor relief on the accomplishment of a work requirement in workhouses (Ravallion, 2015). Beside moral and political motivations, work requirements were designed to repel non-poor claimants. Recent PWP in poor and middle-income countries have the same motivation (Subbarao, 2013; Ravallion, 1991): unskilled wage work offered in PWP screen out workers who have better outside options. This revealing mechanism comes at the cost of forgone earnings for the participants, reducing PWP cost-effectiveness. Besley and Coate (1992) derive the theoretical conditions under

[1] From the 11 studies reviewed in Bagga et al., 2023 four are working papers, three are reports and four are drafts that we were not able to access. For these four studies, we only refer to the review and not the papers themselves.

which work requirements can be cost effective compared to an unconditional cash transfer. For the self-targeting gains in poverty reduction to compensate participants' forgone earnings, the difference in wage rates earned by the poor and the rest of the population needs to be large enough.

In their argument, Besley and Coate (1992) make two implicit assumptions: that individuals are aware of the P W P wage-rate, and that if they are willing to work for that wage they will be employed on PWP. Dutta et al. (2014) document very limited knowledge about MG-NREGS in Bihar and show that an information campaign did not increase hours worked. They find strong evidence of rationing: many workers who would be ready to work at the PWP rate do not get employment. Other supply-side constraints appear to be at play, such as budget constraints and local political economy considerations (see section 5).

The empirical literature assessing the cost-efficiency of PWP using this theoretical framework is scant. Barrett and Clay (2003) find targeting errors in the PSNP because richer households have more workers available. Despite the rationing, Dutta et al. (2014) find evidence of good self-targeting performance in MGNREGS, with only few households participating in the upper deciles of the consumption distribution. Factoring-in self-reported forgone earnings among participants of MG-NREGS in Bihar, Murgai et al. (2016) conclude that a universal basic income with the same budget would achieve greater poverty alleviation. Using the same data, Ravallion (2022) argues that gains could be achieved by letting beneficiaries trade their work assignment under the scheme.

Direct net income gains: Work requirements are essential for PWP targeting performance. However they impose foregone earnings on beneficiaries who would be employed otherwise, so that the direct net income gains are not equal to the wages they earn on the program. The income benefits to PWP participants can be decomposed into two parts. First, participants usually work more hours than in the absence of the scheme. Second, wages rates offered under PWP can be higher than on the private labor market for similar jobs. It might seem surprising that PWP that impose work requirements as an ordeal mechanism would not also offer low wages. But minimum wage laws are often more binding for PWP than for private (informal) wage work. The empirical evidence points to different scenarios in rural and urban settings and depending on the population targeted. Imbert and Papp (2015) find MG-NREGS earnings are 12% higher than the average daily earnings for rural casual workers. Consistently, Alik-Lagrange and Ravallion (2018) document sizable increase in the number of days worked due to the program. In urban areas, on the one hand Franklin et al. (2024) estimate an average hourly wage premium of 1.67 but no impact on the total number of hours worked by participants of the UPSNP. On the other, Bertrand et al. (2021) find that earnings and total hours worked were respectively 63% and 12% higher for participants of the PEJDEC, a program which targeted unemployed youths. This suggests more positive direct impacts on employment and income of participants in settings characterized by surplus labor (Table 2).

Table 2: Direct effect of PWP on participants income in fully employed and underemployed populations

	Effect of PWP on participants		
	Wage rates	Hours worked	Income
Lean season/underemployment	+	+	++
Full employment	+	0	+

Welfare impact beyond income In Besley and Coate (1992) seminal model PWP participants would otherwise do private sector work, so there is no extra disutility associated with work requirements. In practice, PWP participants are often otherwise underemployed, so that participants work more hours than they would in the absence of the program (Bertrand et al., 2021). Consumption is not a satisfactory metric of welfare in this context, given that the work involved is physically demanding and unpleasant, and that many participants would be working less otherwise. Alik-Lagrange and Ravallion (2018) provides empirical evidence that considering this disutility of work implies a more "pro-poor" targeting performance, but not sufficiently to compensate for the direct welfare loss from the work requirement. A growing empirical literature

evaluates impacts on other dimensions of welfare directly related to the disutility of work, such as negative impacts on newborn infant survival among women eligible to MG-NREGS (Chari et al., 2019). Others empirical results show how participation can contribute to improvement in subjective well-being and psychological outcomes. This includes positive impacts on happiness documented in war-torn Central African Republic (Alik-Lagrange et al., 2023), and on pride and positive attitude toward the future in Côte d'Ivoire (Bertrand et al., 2021). PWP can also mitigate the personal and social costs of unemployment. In the setting of Rohynga camps in Bangladesh, (Hussam et al., 2022) show that providing employment to refugees increases their psycho-social well-being as compared to just giving them cash. We will discuss below PWP effects on women empowerment.

3 Effects on Labor Markets

Which equilibrium? The effect that PWP have on labor markets depends on their design, size, and crucially on the nature of the prevailing labor market equilibrium pre-intervention. To simplify, there are three possible views of the labor market equilibrium, which offer radically different predictions on the effect of PWP (Table 3). First, PWP are often motivated by the existence of surplus labor (Lewis, 1954; Sen, 1966; Harris and Todaro, 1970). In a surplus labor economy, a PWP that sets wages below the market wage attract the un(der)employed, and increase total employment, with no effect on the prevailing wage. Second, if employers have market power, then the

government hiring on PWP introduce "contestability" on the labor market (Dreze and Sen, 1991, Basu et al., 2009). Alternatively, if wages are set in reference to a socially accepted prevailing wage (as in Kaur, 2019), then PWP can change the social norm. Depending on the exact nature of the labor market imperfection, PWP could crowd in private employment (in the monopsonistic case) or crowd it out (in the socially accepted wage case). Finally, if labor markets are competitive and labor supply is fixed, then PWP will decrease the labor supply to the private sector and increase wages, with no effect on total employment (Ravallion, 1990).[2]

Table 3: Effect of PWP on labor markets depending on the labor market equilibrium

Labor Market Equilibrium	Effect of PWP on	
	Private Sector Wages	Total Employment
Surplus Labor	0	+
Monopsony	+	+
Other market imperfections	+	?
Perfectly competitive	+	0

What does the evidence say? In principle, policymakers and economists should be able to predict the labor market effects of a PWP program if they knew what the prevailing equilibrium was. Unfortunately, in most contexts we have very little empirical guidance to answer this question. In fact, it is often

[2] We ignore here longer-run effects on labor supply (e.g. via human capital) and potential effects on the labor demand (e.g. via investment) which we discuss in the next section.

the evaluation of PWP that informs the debate on the nature of the prevailing labor market equilibrium. The evaluation of MG-NREGS is a case in point. Imbert and Papp (2015) find positive effects on wages and negative effects on private employment, with no effect on total employment. Since the findings are not consistent with the labor surplus or the monopsony frameworks, they develop a model of labor markets with hiring frictions but competitive wage setting. By contrast, Muralidharan et al. (2023) find positive effects of MG-NREGS in Andhra Pradesh on wages and crowd in of private employment, especially in villages where agricultural employers may have more market power. They conclude that labor markets in this setting are monopsonistic. Among the many other papers devoted to the labor market effects of MG-NREGS (see Sukhtankar, 2017, for a review), Berg et al. (2018) find positive effects on agricultural wages but do not look at employment, and Zimmermann (2020) finds no effect on wages or employment, but focuses on a compositional change of private employment from casual work to self-employment. Overall, there seems to be some consensus that the MG-NREGS increased private sector wages, except in settings where the labor demand is slack, but its effect on employment seems ambiguous and depends on local employer market power. Unfortunately, the evidence base outside of rural India is very limited. Galasso and Ravallion (2004) find that the Argentinian program *Jefes y Jefas* increased employment among beneficiaries, most of them unemployed or out of the labor force. Beegle et al. (2017) find no evidence of labor market tightening following the random roll-out of a PWP program in rural Malawi, consistent

with the labor surplus hypothesis. By contrast, Franklin et al. (2024) show that the UPSNP had positive wage effects and no effect on total employment in Addis Ababa, which they interpret through the lens of a perfectly competitive framework.[3]

Spatial equilibrium effects If labor markets are not strictly local but integrated over space, e.g. through commuting or migration, then PWP can affect labor markets beyond the place they target. These spatial spillovers need to be considered to measure the full effect of PWP. Merfeld (2019) shows that during MG-NREGS roll-out, Indian villages located in MG-NREGS districts close to the border of non-MG-NREGS districts experienced smaller wage increases than villages in the interior. Similarly, Muralidharan et al. (2023) show that wages rose faster in villages located within a 20km radius from treated blocks, and estimate that spatial spillovers account for a third of the total effect. Using both the roll-out of the program and state-level variation in implementation quality, Imbert and Papp (2020b) argue that spatial spillovers of the MG-NREGS go beyond rural areas: they show that the program reduces seasonal migration and pushes up urban wages. The negative effects of MG-NREGS on seasonal migration are confirmed by Imbert and Papp (2020a), but

3 In his review of the literature on New Deal relief spending, which includes public works program from the Work Progress Administration, Fishback (2017) highlights zero or even negative effects on private employment, and positive effect on private wages, especially post 1935, when the US economy had recovered from the 1929 crisis.

Muralidharan et al. (2023) find no evidence of effects on migration overall.[4] In an urban context, Franklin et al. (2024) show that half of the poor working in Addis Ababa commute to another neighborhood, so that a local PWP can affect labor market effects across the city. They identify the effects of the UPSNP on wages by combining the random roll-out of the program and baseline information on the commuting network. They find that the positive effects of PWP on private wages are twice as large as what a simple comparison of wages of workers living in treated and untreated neighborhoods would suggest.

Better than cash? The labor market effects of PWP change the terms of the comparison between cash transfers and PWP. Work requirements impose a cost on beneficiaries which reduce the income gains from PWP as compared to a cash transfer with similar budget. But if work requirements trigger changes in the labor market equilibrium that result in a rise in private sector wages (and in the monopsonistic case a rise in private employment), they can also generate large income gains, not only for program participants but for all wage workers, many of whom are poor. The few studies that quantify the direct income gains from PWP (for beneficiaries) and the indirect gains (for workers who earn higher wages) for the poor find that indirect gains make for a substantial part of total gains: from a third for MG-NREGS (Imbert and Papp, 2015) to two

4 Whether negative or zero, these results contrast with the evidence that cash transfers, which impose no work requirements on participants, increase migration (Angelucci 2015; Gazeaud et al., 2023).

thirds for UPNSP (Franklin et al., 2021). Of course, rising wages impose a cost on employers: Muralidharan et al. (2023) show that only the very top of the landholding distribution experiences a net income loss from MG-NREGS expansion. Hence through their labor market effects, PWP provide an additional redistribution to poorer households: Franklin et al. (2024) show that once wage effects are taken into account the UPSNP generates higher income gains than a cash transfer of comparable budget. Conversely, Murgai et al. (2016) evaluate the income gains from MG-NREGS in Bihar where the labor market is slack and the program has no wage effects, and conclude that a cash (or a food) transfer would be a better anti-poverty policy.

4 Broader socio-economic effects

Skills One aim of PWP is to improve the long-term labor market prospects of participants by building up their skills or their work experience. In their meta-analysis based on 11 experimental studies, Bagga et al. (2023) show that PWP tend to have positive and overall significant effects on employment in the short-run, but smaller and overall insignificant effects in the long-run. For example, Bertrand et al. (2021) show that five months into the PEJDEC program, randomly chosen participants had 100% employment as compared to 85% in the control, and earned three times more. By contrast, there was no discernable effect 12 months after the end of the program. Alik-Lagrange et al. (2017) show that the positive effects of Columbia's PWP on hours worked and earnings declined in the medium-run but are still positive for rural areas. If we consider

the three rural programs reviewed in Bagga et al. (2023), two of them (in Tunisia by Leight and Mvukiyehe (2023) and in DRC Brandily et al. (2020)) report positive effects on earnings after 12 months, and one of them (Djibouti) finds no effect. Overall PWP have some long-run effect on employment or earnings but only for (low-skilled) rural participants.

Consumption and savings: What do the poor do with the additional income from PWP participation and rising private sector wages? First, if they are credit constrained, one would expect household consumption to increase. Perhaps surprisingly for anti-poverty programs, many PWP evaluations report small or null effects on food consumption: Bagga et al. (2023) in their meta-analysis report a small and insignificant positive effect overall from nine studies. Interestingly, the few studies that do report positive effects on food consumption focus on rural programs that target poorer and more credit constrained households. This is the case for all three rural evaluations reviewed in Bagga et al. (2023) in Tunisia (Leight and Mvukiyehe, 2023), rural DRC (Brandily et al.,2020) and Djibouti, as well as for three studies on India's rural employment guarantee (Ravi and Engler, 2009; Deininger and Liu, 2013; Klonner and Oldiges, 2022). Beegle et al. (2017) on Malawi is the only rural study that finds no effects on current consumption. By contrastthe effects are small and close to zero for all urban studies reviewed in Bagga et al. (2023), except for Côte d'Ivoire (Bertrand et al., 2021). Looking at durable consumption, the evidence is much clearer, with positive effects on

household assets in the medium-run for all six studies reviewed in Bagga et al. (2023), as well as in two MG-NREGS studies (Ravi and Engler, 2009; Deininger and Liu, 2013), and Berhane et al. (2014) on PSNP. The review by Bagga et al. (2023) reports larger positive effects on savings, although still not significant overall. PWP in urban settings tend to find larger positive impacts on savings, which makes sense if urban households have better access to finance (Bertrand et al., 2021; Franklin et al., 2021). By contrast few rural studies report increased savings (Ravi and Engler, 2009).

Insurance: PWP do not only bring static income gains, they can also provide social insurance against income shocks (Ravallion, 1990; Barrett et al., 2004). Historically, they have been implemented as a short-term response to economic downturns, from the pre-colonial famines in South Asia (Dreze and Sen, 1991), to the 2001 financial crisis in Argentina (Jalan and Ravallion, 2003). But some programs are also long-running, such as the PSNP or the MG-NREGS and can serve as safety net against common or idiosyncratic shocks. MGNREGS expands in bad agricultural years (Gehrke, 2019; Fetzer, 2020), and grew during the COVID epidemic, even though it fell short of meeting the demand for work (Narayanan et al., 2022). Similarly, Berhane et al. (2014) argue that the PSNP reduced food insecurity during a period of rising food prices and droughts in the 2000s, and Abay et al. (2022, 2023) that it protected rural households during the COVID pandemic. By providing social insurance, PWP can have two additional effects: reduce the need for other types of risk-coping

strategies and allow households to make risky (but productive) investments. Bell and Mukhopadhyay (2020) provide evidence of both effects: they show that MG-NREGS reduces borrowing in the lean season, when household income is low, but increases it in the agricultural season, when households can invest in their farm. On the crowd-out of alternative insurance mechanisms, Morten (2019) simulates the effects of a rural employment guarantee similar to MG-NREGS in a structural model of informal risk sharing and temporary migration. She finds that an employment guarantee crowds out both informal risk sharing and seasonal migration (which is consistent with the evidence from Imbert and Papp, 2020b,a). On the investment in more risky activities, Gehrke (2019) shows that MG-NREGS allows agricultural households to use more inputs and cultivate more risky crops, such as cotton, and Zimmermann (2020) finds that it increases self-employment. Gilligan et al. (2009) find that PSNP participants are more likely to borrow and invest in new agricultural technologies, as well as in non-agricultural businesses.

Value of works: PWP are not only income transfers they also build potentially valuable assets. However, since the main objective of the PWP is to provide employment locally, the projects are often labor intensive and small.[5] MG-NREGS works included rural roads, irrigation facilities, creche buildings, and fishponds, while UPSNP workers cleaned streets, maintained drains, planted

[5] In the US in the 1930s the Public Works Administration focused on infrastructures, while the Work Progress Administration focused on employment (Fishback 2017).

trees, and gardened flowerbeds. Unfortunately, we have very little empirical evidence on the value of these works (Gehrke and Hartwig, 2018). Neither Bahru and Zeller (2022) using household surveys nor Gazeaud and Stephane (2023) using satellite data find any effect of the rural PSNP agricultural productivity and both question the productive value of the PWP. In Andhra Pradesh Deininger and Liu (2013) find evidence that the MG-NREGS supported improvements on private agricultural land of beneficiary households, which they argue might have increased the direct gains from participation. Based on a survey in 100 villages of Maharashtra, Ranaware et al. (2015) find that the vast majority of MG-NREGS works are functional and contribute directly or indirectly to better agriculture (Drèze and Khera, 2017). Both Imbert and Papp (2015) and Muralidharan et al. (2023) argue that the rise in private sector wages cannot quantitatively be explained by the productivity effects of the MG-NREGS assets, but without direct evidence on this channel. Franklin et al. (2024) find that all residents of UPSNP neighborhoods, whether they participated to the program or not, reported improvements in terms of cleanliness, drainage, and smell. Using the correlation between hypothetical rents and neighborhood characteristics (controlling for housing characteristics), they estimate that improved neighborhood amenities increased resident welfare by 3%. Overall, the literature suggests that PWP may generate little productivity gains, and only modest amenity improvements, but there is not enough evidence to draw definite conclusions.

Female empowerment: By offering the same amount of paid work at the same wage for male and female beneficiaries, PWP reduce gender gaps in contexts where few women work and earn lower wages. Many PWP, including MG-NREGS (Dutta et al. (2014)) mandate female participation through gender quotas and facilitate it with childcare provision at the worksites. Ajayi et al. (2022) report that PWP in Burkina Faso triple childcare usage, improve child development as well as women's employment, and financial and psycho-social outcomes. Alik-Lagrange et al. (2023) finds evidence that working on PWP helped shift both male and female attitudes about who does what within the household in Central African Republic. Field et al. (2021) show that paying female participants to the MG-NREGS on their own bank account and training them to use it increased their labor force participation and changed their views on the social acceptability of women's work. In Lao People's Democratic Republic Perova et al. (2021) provide experimental evidence that PWP increase female income but do not change their experience of gender-based violence.

Effects on children Parents' participation to PWP can have ambiguous effects on children. On the one hand, Shah and Millett Steinberg (2015) use a difference-in-difference strategy and find evidence that MG-NREGS roll-out decreased school enrollment of 13-16 year olds, with boys joining the labor market early and girls doing domestic chores and sibling care. Li and Sekhri (2020) even find negative effects on primary school enrollment and argue that

child labor increased. Zaidi et al. (2017) also provide descriptive evidence of girls taken out of school while their mothers worked in the MG-NREGS. On the other hand, Afridi et al. (2016) and Mani et al. (2020) find positive effects of MG-NREGS roll-out on schooling. They argue that the negative effects from a higher opportunity cost of time for mothers are offset by the increase in female income, which changes household bargaining in favor of children's education.

5 Political Economy

Corruption: The implementation of PWP, especially those that are implemented for a long period of time, rely on a complex administrative structure which is vulnerable to corruption. Corrupt local officials inflate employment records, claim workers' wages, and share them with higher officials in exchange for their cooperation (Banerjee et al., 2020). As a transparency measure, details of all MG-NREGS expenditures and individual beneficiaries were made public. Independent surveys found many "ghost workers" who did not exist and "ghost days" which were never worked (Niehaus and Sukhtankar, 2013b,a). Imbert and Papp (2011) estimate that in the first years of implementation (2007-08) only 42 to 56% of reported MG-NREGS employment was confirmed by nationally representative survey data. Corruption plagued MG-NREGS implementation: Niehaus and Sukhtankar (2013b) show that a statutory wage increase was never passed on to workers, and Ravallion (2019) argue that corrupt officials reduce employment by fear of being exposed if they involve too many workers.

Fortunately, MG-NREGS was also a fertile ground for anti-corruption policies. Muralidharan et al. (2016) report on the introduction in Andra Pradesh (AP) of smart cards that link beneficiary payments to their biometrics and show that it decreased the number of "ghost days" and increased employment provision. The smart cards were subsequently implemented in the whole state. Banerjee et al. (2020) study a financial reform in the Bihar that cut off intermediary administrative levels from the flow of MG-NREGS funds, thus making it easier for local officials to claim reimbursement, but at the same time mandated real-time reporting of beneficiary details. The reform had mixed effects: it reduced "ghost workers", with negative effect on officials' personal wealth, but had no positive effect on employment and even increased payment delays. The Bihar reform was halted after nine months only: Banerjee et al. (2020) document explicit opposition of intermediary officials to the reform. The different fate of these two reforms may be due to the better quality of IT infrastructure in AP than in Bihar, but also to the fact that the new system was introduced only for new workers in AP, which left room for corrupt officials to extract rents on "ghost workers", while the Bihar system was imposed on all payments. Overall anti-corruption efforts seem to have paid off: Imbert and Papp (2014) estimate that in 2011-12 80% of reported MG-NREGS expenditures were borne out by survey data. Drèze and Khera (2017) notes that the implementation of MG-NREGS has contributed to reviving the institutions of local democracy (village councils and assemblies). The results referenced here are specific to MG-NREGS, a program

implemented over a long period of time. We are not aware of results related to corruption for short term PWP, which tend to rely on simpler administrative structures and lower state capacity. More research is needed in such contexts, where the monitoring of workers selection, participation in activities at the worksite and payments can be more challenging.

Local political economy: Because the local level is key in the implementation of PWP, local politics can play an important role in deciding which workers are enrolled, which projects are selected, and overall, how much money is spent for PWP. The simplest form of political influence in PWP is favoritism: Caeyers and Dercon (2012) show that households with personal connections to local politicians received 28% higher payments from the PSNP. A more complicated form of political economy is clientelism: Anderson et al. (2015) develop a theoretical model of vote-trading in which a landed elite would generally oppose the implementation of redistributive programs such as PWP which could push up wages, but would co-opt it if they can use it to buy votes. They find support for these subtle predictions in the context of Maharashtra's Employment Guarantee Scheme (EGS), precursor of MG-NREGS. The importance of PWP for local politics is also illustrated by Banerjee et al. (2017)'s findings that providing information on politicians' role in MG-NREGS during local elections increased accountability and electoral competition. Even though in many contexts parties are less present at the local level, there is abundant evidence that party politics also play a role in budget

allocation for PWP. In the context of West Bengal where local elections are partisan, Dey and Sen (2016) present evidence that the village councils ruled by the Trinamool Congress (TMC) party allocated more MG-NREGS funds to villages that voted for them, which strengthened its position in subsequent local elections. In the context of Rajasthan, where local elections are non-partisan, Gupta and Mukhopadhyay (2016) find that the Indian National Congress (INC) party who was in power centrally and launched the MG-NREGS may have strategically sanctioned more MG-NREGS projects in constituencies where it had a Member of Parliament (MP) to gain votes in legislative elections. Conversely, Dasgupta and Kapur (2020) document the flip side of political alignment: when political responsibilities are unclear ("divided agency"), the local administration in charge of MG-NREGS get fewer resources. Zimmermann (2021) tests the effectiveness of this strategy and shows that districts with longer exposure to MG-NREGS were less likely to vote for the INC and its allies in the legislative elections that followed its roll-out, especially in states where program implementation was poor. Zimmermann (2021)'s results cast doubt on the idea that PWP always win votes to the party in power and suggest that voters punish poor implementation (as Banerjee et al. (2017) show at the local level).

Conflict: PWP are often implemented in contexts of ongoing or recent conflict: examples include the Central African Republic's recent Londö PWP, and the priority given to districts with ongoing Maoist insurrection for the early phases

of the MG-NREGS. The hope is that higher incomes, and more stable livelihoods could help poor households cope with the consequences of violence, but also that they could dissuade some of their members to join the conflict. Dasgupta et al. (2017) use a difference-in-difference framework to show that the roll-out of the MG-NREGS reduced Maoist violence, but only in states where program implementation was good, which is consistent with the evidence on rising rural wages from Imbert and Papp (2015). Fetzer (2020) estimates the relationship between agricultural income shocks due to low rainfall and Maoist conflict before and after MG-NREGS implementation. He finds that the MG-NREGS cushions rural wages from negative climatic shocks and prevents the usual rise in conflict in bad agricultural years. Using the same identification strategy as Zimmermann (2020), who found no positive wageor employment effects of the MG-NREGS, Khanna and Zimmermann (2017) find that MG-NREGS increases the intensity of Maoist conflict. They highlight two potential explanations for this finding: that Maoists and government may fight to control the future stream of MG-NREGS resources or that civilians cooperate more with the government, which in the short-run can increase counter-insurgency efforts. Another potential benefit of PWP in conflict-ridden settings is that they could facilitate reconciliation and increase social cohesion through joint work. In the Central African Republic context, Alik-Lagrange et al. (2023) find that, as compared to non-participants in the same community, participants tothe Londö PWP were more satisfied with the security situation but did not report higher social cohesion. Since reported social cohesion was high for participants and non-

participants, this could suggest either that PWP had no effect on social cohesion or had large positive spillover effects on non-participants. Ivaschenko et al. (2017) document little robust evidence that Papua New Guinea's Urban Youth Employment Project reduced participants' engagement in or exposure to crime. Overall, the evidence suggests that PWP can be an effective tool to prevent conflict by increasing household income and helping them cope with negative income shocks. The evidence is however not conclusive on whether they are effective at addressing the broader social causes and consequences of violence.

6 Conclusion

Are work requirements among recipients of social assistance good or bad? PWP are often credited of many virtues and accused of many perverse effects. Over the past decade, the evidence base has grown significantly. We report key aspects on which a consensus seems to form, but most of the results we refer to are for a few large, long-running programs (such as MG-NREGS and PSNP). More empirical evidence needs to be produced across contexts, as the effects of PWP seem to vary significantly with design features and conditions of implementation.

Through work requirements, PWP generally achieve good pro-poor targeting, but they do impose substantial costs to beneficiaries in terms of forgone income and/or disutility of work. Their direct effects on income and wellbeing of participants are positive, but only dominate cash in situations where

employment opportunities are scarce. There is no systematic evidence on how targeting performance varies depending on the duration of the programs and the types of shocks faced by beneficiaries.

In some settings, PWP compete with private employment, which leads to higher wages, and even higher employment if employers have market power. These indirect effects can be very large for the poor whose livelihood depends on casual wage employment. PWP in that way can achieve more redistribution than an equivalent cash transfer.

Much could be learned from a controlled experiment comparing unconditional cash transfers against workfare, with varying wage rates. Not only would this deliver important results on the balancing welfare effects of work requirements, but it would also provide novel evidence on households' and individuals' labor supply decisions.

Interestingly, the income gains documented in the literature do not necessarily translate into higher consumption but increase savings in urban settings. By providing social insurance, PWP also seems to increase risky investments, e.g. the adoption of new crops or business creation, but more evidence is needed on this point.

There are also some aspects on which the evidence suggests that PWP deliver little benefits. This is the case of skill development and productive assets and amenities, which are explicit goals of many PWP, but are in practice subordinate to the aim of employment generation. More research is needed on

these outcomes which could play an important role in the cost-benefit analysis of PWP.

Finally, on some important dimensions the evidence is too scant to offer clear conclusions. One of these dimensions are intra-household dynamics around PWP participation, their impact on female empowerment and on children welfare. Another is the effectiveness of PWP in fostering social cohesion in post-conflict settings.

Author Acknowledgements: We thank Jean Drèze, Simon Franklin, Esther Gehrke, Paul Gertler, Estelle Koussoube, Paul Niehaus for their invaluable feedback. We are grateful to Lakshya Kaviya for excellent research assistance. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors and do not necessarily represent the views of the World Bank, its Executive Directors, or the governments of the countries they represent.

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