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Authors

Dionne, Kim Yi
Horowitz, Jeremy

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The Political Effects of Agricultural Subsidies in Africa: Evidence from Malawi

Kim Yi DIONNE and
Smith College

Jeremy HOROWITZ
Dartmouth College

Abstract

Across sub-Saharan Africa agricultural subsidy programs have again become a common strategy for combatting rural poverty, increasing agricultural production, and reducing food insecurity. Despite a large literature examining subsidies' effects on output and welfare, little is known about their political effects. This paper examines Malawi's Agricultural Input Subsidy Programme, one of the largest and most expensive programs implemented, which was launched by the government in 2005. We examine whether the incumbent party, the Democratic Progressive Party headed by president Bingu wa Mutharika, benefited from Malawi's subsidy program by examining a longitudinal dataset of 1,846 rural Malawians interviewed in 2008 and again in 2010. The individual-level data show no evidence that the subsidy program was targeted to Mutharika's co-ethnics or co-partisans. Our analysis further demonstrates that the subsidy program increased support for the incumbent party. These results suggest that even when parties are unable or unwilling to target distributional programs at the local level, they may nonetheless derive political benefits. As anti-poverty programs – including agricultural subsidies to small-scale farmers – become increasingly common across the continent, our results suggest that they may help to explain patterns of party affiliation and vote choice, particularly where traditional patterns of partisan affiliation related to ethnic or regional identities weaken.

1. Introduction

Do political leaders benefit from anti-poverty programs? There is a large and growing literature on the targeting of government expenditures, but less is known about the political effects of distributive programs, particularly large-scale poverty-reduction efforts that target substantial portions of the population. Across Africa, governments have increasingly adopted agriculture subsidy programs in recent years to combat rural poverty and food insecurity, embracing a strategy common in the 1960s and 1970s before structural adjustments programs reduced such market interventions in the 1990s (Minot and Benson

Complete contact information for the corresponding author: Dr. Kim Yi Dionne, Smith College, Government, 231 Wright Hall, Northampton, MA 01063, United States, Phone: +1-413-585-3563, Fax: +1-413-585-3389, kdionne@smith.edu.

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2009; Banful 2011; Kelly, Crawford, and Ricker-Gilbert 2011; Chirwa and Dorward 2013; Jayne and Rashid 2013). While the political appeal of agricultural subsidies in countries where the majority of the population is engaged in smallholder agriculture are obvious, there has been little quantitative research on their effects.¹ In part this lacuna stems from the difficulty of quantifying the political effects of subsidy programs. Because subsidy programs may be targeted, often for political reasons (Banful 2011; Pan and Christiaensen 2012; Jayne and Rashid 2013), researchers must confront the thorny challenge of teasing apart selection effects from potential treatment effects.

This paper contributes to studies of distributive politics by examining Malawi's Agricultural Input Subsidy Programme (AISP), one of the largest and most expensive programs implemented to date. To examine whether the incumbent party, the Democratic Progressive Party (DPP) headed by president Bingu wa Mutharika, benefited from Malawi's subsidy program, we draw on panel data from a survey of 1,846 respondents interviewed in 2008 and again in 2010. We proceed in two steps. We first investigate whether the program was targeted at the local level. We propose that because of informational constraints and the weakness of party institutions at the grassroots level, the subsidy is likely to be untargeted with respect to party support and the main determinant of party allegiances – ethnicity – at the village level. Consistent with these expectations, we find no evidence of partisan or ethnic targeting in our sample area. This finding is interesting in its own right, especially given dominant theories of distributive politics that argue whether politicians benefit by targeting material transfers to core supporters or swing voters (e.g., Cox and McCubbins 1986; Dixit and Londregan 1996).

The second step in the analysis is to test for potential effects on preferences. While we find no evidence of political targeting at the individual level, we do not claim that distribution was random. Accordingly, testing for political effects requires accounting for potential confounding factors. We employ two alternative methods for addressing possible omitted variables. The first uses a lagged dependent variable (LDV) approach and the second employs a difference-in-difference framework. Both approaches yield similar estimates, indicating that during the period under examination the subsidy increased support for the incumbent party by 6.2% to 7.5%. While this increase might seem relatively modest, we emphasize that this result is the estimated effect of receiving the subsidy in a single year of a multi-year program and that the individual effects of the subsidy are likely to be attenuated because non-recipients may have also benefitted indirectly, for example through reduced food prices.

The main contribution of this paper is to add to the growing empirical literature on the political benefits of poverty-reduction programs (e.g., de la O 2013; Zucco 2013). We draw from the new wave of agricultural subsidy programs in Africa to demonstrate that such programs can alter political preferences even in settings like Malawi, where entrenched ethno-regional partisan ties might be expected to limit the political effects of government-sponsored programs. In doing so, the paper also contributes to the literature specifically

¹Notable exceptions include Banful (2011), Pan and Christiaensen (2012), Mason, Ricker-Gilbert, and Jayne (2013), and Brazys, Heaney, and Walsh (2015).

related to the political economy of agricultural subsidies (e.g., Banful 2011, Mason, Jayne, and van de Walle 2013; Brazys, Heaney, and Walsh 2015; Chirwa and Dorward 2013). The findings have important implications for the larger literature on distributive politics and on our understanding of voter behavior in Africa. The conventional wisdom in scholarship on patronage and clientelism, particularly in Africa, is that ruling parties build and maintain support by channeling material favors to core supporters (e.g., Bratton and van de Walle 1997). One of the key debates in the empirical literature on voter behavior in Africa's emerging democracies is whether government performance affects voters' political preferences in contexts where social identities, particularly ethnicity, are salient. The analysis presented here shows that distributive programs need not be targeted to core supporters (or co-ethnics) to be politically beneficial to incumbents, suggesting that leaders in Africa's emerging democracies (and elsewhere) may be able to enhance their support by implementing anti-poverty programs that do not discriminate against non-partisans or out-groups at the local level.

2. The Political Effect of Anti-Poverty Programs

Should incumbent leaders expect to reap political rewards from implementing targeted anti-poverty programs? On the one hand, the answer may seem obvious. The theoretical literature on retrospective voting suggests that voters reward parties that implement desired policies (Ferejohn 1986). To the extent that distributive programs lead to real improvements in welfare, voters may well compensate the incumbent at the ballot box. Studies from emerging democracies in the developing world find evidence of such a link. De la O (2013) shows that in Mexico a large-scale anti-poverty cash transfer program that provided benefits to low-income families increased voter turnout and support for the incumbent party. In Brazil, Zucco (2013) reports similar effects when examining a cash transfer program aimed at low-income families with children. Manacorda et. al (2011) show that a short-term poverty relief program in Uruguay had similar effects, increasing support for the incumbent party that launched the program. Pop-Eleches and Pop-Eleches (2012) examine a program that distributed coupons to poor families for the purchase of reduced-priced computers and found that beneficiaries were significantly more likely to support parties of the governing coalition. Finally, Harding (2014) and Harding and Stasavage (2014) present evidence from Ghana and Kenya that voters reward incumbents for improving roads and expanding access to primary education.

There are at least two channels through which targeted subsidy programs like the agricultural subsidy we examine in this paper might affect voter preferences. First, retrospective theories of economic voting suggest that when such programs have a positive effect on individual economic welfare, voters will reward the party responsible for implementing the program. Agricultural programs are generally highly visible initiatives that have a direct effect on material well-being for large numbers of citizens. In Malawi, for example, studies have shown a strong positive relationship between expanded fertilizer use resulting from the subsidy program and crop yields (Shively and Ricker-Gilbert 2013). Others have linked the subsidy to dramatic increases in maize output that reduced food insecurity and brought down the price of maize in local markets (Denning et al. 2009; Dorward et al. 2010). Existing studies suggest that subsidy programs have contributed to

improved evaluations of incumbent job performance in Malawi and elsewhere (Ferree and Horowitz 2010; Cooksey 2012). This is important, because, as Harding and Stasavage (2014) argue, voters are more likely to reward incumbent leaders for programs that can be directly attributed to those political actors.

The clientelism literature suggests an alternative mechanism through which subsidy programs might affect voter behavior. In contexts where the distribution of valued benefits is controlled by party agents, citizens may trade their vote for material transfers (Lemarchand 1972; Bratton and Van de Walle 1997; Stokes 2005). By this logic, the receipt of subsidy coupons might be expected to strengthen patron-client bonds, solidifying support for the party that controls access to state largess. However, for reasons described below, we expect the clientelist mechanism to be less relevant in the Malawian context that we study because of weak local-level party infrastructure.

At the same time, there is reason to be skeptical about anti-poverty programs' power to influence political preferences, particularly in African contexts where voter preferences are often driven by ethnic and regional identities. Longstanding approaches to political behavior in Africa suggest that voters hold strong preferences for candidates and parties associated with their own ethnic communities and only trust co-ethnics to deliver benefits to their group (Bates 1983; Horowitz 1985; Posner 2005; van de Walle 2007). Where ethnicity underlies political preferences, voters may be unresponsive to material transfers and may be hesitant to give incumbents credit for distributive programs, even when such programs do not discriminate by ethnicity or partisanship.² Moreover, the clientelism literature suggests an additional reason why voters may be unmoved by anti-poverty programs: where local monitoring systems are weak, voters may simply accept government favors but continue to vote according to pre-existing preferences (Nichter 2010).

The existing empirical literature from African cases has so far offered mixed findings on the connection between government performance and voter preferences. Several recent studies provide evidence in favor of retrospective voting theories (Posner and Simon 2002; Bratton, Bhavani, and Chen 2012; Ferree 2006; Harding 2014; Harding and Stasavage 2014). Other studies, however, suggest that in some cases ethnicity can trump performance (e.g., Bratton and Kimenyi 2008). With the exception of Harding (2014) and Harding and Stasavage (2014), these works tend to focus on broad performance measures, rather than specific anti-poverty programs. As such, we still know relatively little about the potential effect of particular policy initiatives.

There is good reason to be skeptical about the political effects of anti-poverty programs in the Malawian context in particular. In four of five elections after the return to competitive politics in 1994, electoral results exhibited a clear ethno-regional character, with voters in the North, Center, and South lining up *en masse* behind the party (or multiple parties) associated with their regions and the ethnic communities in each area (Ferree and Horowitz 2010; Tsoka 2009; Dulani and Dionne 2014). Likewise, the incumbent party at the time of

²As Posner (2005) has shown in Zambia, voters tend to maintain the belief that presidents favor their own regions even when evidence of favoritism is absent.

our study – the DPP – was particularly weak. The incumbent president, Mutharika, came to power in the 2004 election as the hand-picked successor of the retiring president, Bakili Muluzi. Mutharika was elected as the head of the United Democratic Front (UDF) party, over which Muluzi continued to preside after the 2004 election. Subsequent power struggles led Mutharika to abandon the UDF and launch the DPP in 2005. As a new party, the DPP lacked even minimal infrastructure and was poorly suited to monitor clientelist exchanges at the local level. The principal contribution of this article is to show that anti-poverty programs, particularly in the form of a targeted agricultural subsidy, can affect political preferences even when ethno-regional identities are politically salient and parties lack the ability to monitor distributive exchanges.

3. Malawi's Agricultural Input Subsidy Programme

Intervening in agricultural markets has long been a central political strategy used by African leaders to build and maintain support. In the period following independence African leaders implemented a variety of agricultural policies – tariffs, price controls, subsidies, credit schemes, and so forth – to reward favored constituencies (Bates 1981). These interventions favored urban residents and rural notables, not ordinary farmers who posed little threat to incumbent leaders. By the 1990s, governments across the continent had removed or scaled back these programs in response to fiscal constraints and donor pressure to reduce the role of the state in the economy. In recent years, however, large-scale subsidy programs have re-emerged in several countries as initiatives to combat stagnant agricultural productivity and chronic food insecurity (Minot and Benson 2009). One estimate suggests that seven leading African countries presently spend over US\$ 2 billion per year on subsidy programs (Shively and Ricker-Gilbert 2013).

Malawi's Agricultural Input Subsidy Programme (AISP) was launched in response to a sharp decline in rainfall in the 2004/05 growing season³ that left an estimated 4.9 million Malawians (roughly 40% of the population) vulnerable to hunger and food insecurity (Malawi Vulnerability Assessment Committee, 2005). The AISP targeted poor subsistence farmers starting with the 2005/06 season. The program represented a massive expansion of efforts to alleviate food insecurity that had been initiated earlier in the decade (Harrigan 2008). The program is often heralded as a success: one report, for example, credits the AISP with taking Malawi from having a 43% food deficit in 2005 to achieving a 53% food surplus in 2007—becoming a net food exporter in just two years (Denning et al. 2009). These claims, however, have been challenged by those questioning the validity of such statistics (Jerven 2013) and by those who have studied household-level data to measure the program's enduring effects and its ability to reduce food insecurity and poverty (Ricker-Gilbert and Jayne 2011; Lunduka, Ricker-Gilbert, and Fisher 2013).

The AISP's core objective was to increase resource-poor smallholder farmers' access to improved agricultural inputs (fertilizer and seeds) to achieve food self-sufficiency and to increase smallholder farmers' incomes through increased food and cash crop production (Dorward et al. 2010). Beneficiaries received coupons to be redeemed at government-

³Malawi's growing season runs from November to April.

designated retailers for either seed or subsidized fertilizer. The AISP distributed three types of coupons: a fertilizer coupon, a seed coupon, and what was referred to as a flexible coupon, which could be exchanged for a variety of seed options. The program grew in subsequent years, reaching an estimated 65% of farm households at its peak in 2008/09 at a cost of approximately USD \$285 million in that year, equivalent to 16% of the government's annual budget or about \$22 per citizen in a country where GDP per capita is less than \$300 (Dorward et al. 2010).

Allocation decisions⁴ are made at the national level by officials from the Ministry of Agriculture. District allocations are based on the amount of land under cultivation and the number of farm families per district, information that is provided by village-level officials and cross-checked by agricultural extension agents. At the village level, allocation was jointly determined by the Ministry of Agriculture, District Development Committees, Area Development Committees, and Traditional Authorities. Individual beneficiaries were supposed to be identified through use of the farm household register and open meetings held by Ministry of Agriculture staff. Subsidy program committees (usually formed from already existing “village development committees”) and village chiefs would identify eligible beneficiaries. These committees submitted to Ministry of Agriculture staff a list of names of those in need, from which the Ministry of Agriculture selected beneficiaries. District officials then transferred coupons to representatives of village committees (often at a large public gathering for multiple villages), and these committees distributed coupons to beneficiaries.

Given the multiple actors involved in the identification of recipients and the delivery of coupons, there were a number of opportunities for the government to engage in targeting.⁵ The literature on distributive politics offers contrasting views on whether incumbents should be expected to target rewards to core supporters (Cox and McCubbins 1986), swing voters (Dixit and Londregan 1998) or some mix of the two. Empirical studies from other contexts find evidence consistent with both models (e.g., Miguel and Zaidi 2003; Khemani 2007; Dahlberg and Johansson 2002). With specific regard to targeting in agricultural input subsidy programs in African countries, relevant studies found evidence of political targeting toward opposition strongholds in Ghana (Banful 2011), core support areas in Zambia (Mason, Jayne, and van de Walle 2013), and an absence of targeting with respect to patterns of prior electoral support in Malawi (Brazys, Heaney, and Walsh 2015).⁶ More work is needed to identify the conditions under which leaders adopt alternative allocation strategies.

⁴Allocation at the village level and individual-level beneficiary identification changed over the years AISP was implemented. See Table 10.2 in Chirwa and Dorward (2013) for a summary overview of the change over time in AISP beneficiary identification and coupon allocation. Our foregoing description draws from Chirwa and Dorward (2013) and our observations of the 2008/09 season, the season aligned with the data we analyze.

⁵It is possible that the overriding objective of increasing food production might lead policy makers to allocate the subsidy according to efficiency criteria in ways that proscribe the ability to target according to political criteria. While efficiency no doubt plays an important role in targeting, we doubt that the absence of political targeting can be explained by the efficiency imperative both because national-level studies consistently show evidence of political and/or ethnic targeting (Banful 2011; Mason and Ricker-Gilbert 2013; Mason, Jayne, and van de Walle 2013; Brazys, Heaney, and Walsh 2015) and because anecdotal accounts of Malawi's experience are replete with stories of political influence.

⁶At the household level, Pan and Christiaensen (2012) found politically connected households in Tanzania were more likely to receive subsidies. Beyond political factors, prior studies in Malawi also provide evidence of targeting towards households with greater resources such as wealth or landholdings (Chirwa and Dorward 2013; Kilic, Whitney, and Winters 2015), greater educational

Regardless of whether the Malawian government may have sought to reward core supporters or to court potential swing voters through the allocation of subsidy coupons across districts, we expect that the ability to target *individuals* at the local level was constrained in Malawi by the relatively high degree of ethnic homogeneity at the village level.⁷ The prerequisite for individual targeting is that party agents must be able to identify core supporters, opposition supporters, and possible swing voters. In most election years, ethnicity serves as a useful predictor of electoral preferences (Ferree and Horowitz 2010), making it possible to identify core and swing *districts* based on ethnic demography. However, because rural villages have little ethnic diversity, ethnicity is much less useful for identifying core and swing *individuals* within villages. Two additional features of the Malawian context limit the potential for village-level targeting. First, during the period under study partisan orientations were abnormally fluid, making ethnicity even less useful as a marker of party allegiances. Following Mutharika's departure from the UDF in 2005, the president engaged in an aggressive campaign to attract support across ethnic and regional divisions, an effort that was highly successful as indicated by the broad-based electoral support he received in the 2009 election in which he defeated the second-place candidate, John Tembo, by a margin of 66% to 31%. The shifting political landscape, however, meant that traditional ethno-regional alignments were temporarily upended.⁸ Second, Malawian parties lack the local-level networks that allow machine-based parties in other parts of the world to identify core and swing voters at the village or neighborhood level. Political parties in Malawi function primarily at the national level with weak or nonexistent formal structures at the local level (Svåsand 2011). This was particularly true of Mutharika's DPP, which, having been formed in the aftermath of the president's 2005 departure from UDF, initially lacked even basic local-level infrastructure. While individual politicians, such as parliamentarians, no doubt maintained their own informal networks at the local level, the challenge of distinguishing partisans from non-partisans based on observable markers would have been formidable. Given these informational limitations we expect the subsidy program to be largely untargeted with respect to ethnicity and party support at the individual level within villages (though it might still have been targeted with regard to these factors at the district level).

4. Data

The analysis of subsidy targeting and its political effects employs data from two waves of a panel survey (2008 and 2010) of rural Malawians in 122 villages clustered in three districts: Rumphi, Mchinji, and Balaka. (Figure A1 in the on-line appendix provides a map showing

attainment by the household head (Kilic, Whitney, and Winters 2015), or having a male household head (Chibwana, Fisher, and Shively 2012).

⁷While we lack village-level ethnicity data, the 2008 census allows for an approximation of local-level ethnic demography. The lowest-level at which data is aggregated in the census is the Enumeration Area (EA), a geographic unit that often spans several villages in rural areas. The census data shows that for the EAs in the three districts in which our survey villages are located (Rumphi, Mchinji, and Balaka), the median size of the largest group at the EA level is 86% of the population. Our individual-level survey data (described more fully below) confirm the high degree of ethnic homogeneity within our project villages. In the villages in Rumphi 94.5% of respondents were Tumbuka; in the villages in Mchinji 89.6% of respondents were Chewa; and in the villages in Balaka 78% of respondents were Yao.

⁸Data from the Afrobarometer 2008 survey, conducted in October and November, show that the ethno-regional patterns of partisan support observed in prior elections did not hold at the time that targeting decisions for the 2009/10 growing season were being made. The data (in Tables A4 and A5 in the online appendix) show that the DPP was the leading party in all three regions and all major ethnic groups at the time of the survey. Thus, even in more diverse villages, ethnicity would have been of limited utility for targeting core and swing voters during the period under study.

the research sites.) Each administrative region (North, Center, and South) of Malawi is represented in the study, as are the three major ethnolinguistic groups (the Tumbuka, Chewa, and Yao). The surveys are part of the Malawi Longitudinal Study of Families and Health (MLSFH), which began in 1998 and aims to understand how villagers cope with health challenges like HIV/AIDS.

In each district, the MLSFH used a cluster sampling strategy across selected census enumeration areas. A random one-in-four sample of women of reproductive age and their husbands was drawn from villages to yield a target sample in 1998 of 1,500 women and their husbands (Kohler et al., 2015, 395). The resulting sample in 1998 included 1,532 ever-married women aged 15-49 and 1,065 of their spouses. In 2004, the MLSFH added a sample of 984 adolescents aged 15-24, and during the 2008 round a sample of 549 parents of respondents in earlier MLSFH rounds was added (Kohler et al. 2015). New spouses of MLSFH respondents were also added in each wave. Though the original sampling strategy in 1998 was not designed to be representative of the rural population in Malawi, the sample's characteristics are very similar to those of the rural population interviewed by the Malawi Demographic and Health Surveys that covered nationally representative samples (Kohler et al. 2015). Between the two rounds studied here, 1,016 respondents were lost to follow-up;⁹ thus, though the 2008 round included a sample of 3,909 respondents and the 2010 sample included 3,786 respondents, our analysis includes only those 2,851 respondents who were interviewed in both 2008 and 2010. The analytical sample in this paper drops to 1,846 when we remove study participants whose responses on standard questions are inconsistent between the 2008 and 2010 waves (i.e., those reporting a different gender or incompatible ages across waves) and when responses are dropped because of missing information on key variables.¹⁰ We augment the individual-level panel data with village-level data collected through a survey of village headmen in the 122 research villages conducted in 2008.

5. Was the Program Targeted?

The first step in the analysis is to test for evidence of targeting. While the results from this analysis are interesting in their own right, the primary goal is to identify factors that might confound the analysis in the next section of the subsidy's effects on voters' political preferences.

To examine targeting in our survey area, we draw on a question on the 2010 survey that asked respondents whether they had received a voucher for fertilizer or seeds in each of the previous two years. We focus on those who received the subsidy in the 2009/10 growing

⁹Of these, 90 died, 576 moved or were temporarily absent, and 350 were categorized as "other", which can include being hospitalized or refusing to participate (Kohler et al., 2015, supplementary appendix pp. 30-31).

¹⁰To eliminate potential mismatches, we dropped observations if: 1) the gender or ethnic group did not match across waves; 2) age in 2010 was five years more or less than it should have been based on the 2008 answer; 3) farm size in 2010 was four times more or less than in 2008; or the reported number of children in 2010 was four more or less than in 2008. Based on field observations of the survey, we attribute mismatches to two main factors. First, the information available to the enumerators was not always sufficient to uniquely identify respondents from the prior wave, leading to error in the selection of individuals to be re-interviewed. Second, respondents in some cases chose to "stand in" for individuals who were not in the study area at the time of the 2010 wave in order to obtain the small benefits distributed to participants (typically, a bar of soap or other similar items). The online appendix provides additional analysis of attrition and addresses concerns about possible bias introduced as a result of attrition. These alternative analyses are not substantially different from our main results.

season, which immediately preceded the second wave of the survey. The data show that 73.7% of respondents received the agriculture subsidy in 2009/10. Respondents in the three district clusters were about as likely to receive the subsidy, with 74.6% receiving it in Rumphu, 78% in Mchinji, and 68.2% in Balaka.

The independent variables for the targeting analysis come from the 2008 survey unless otherwise specified. Our key independent variable measures party support according to responses to a question that asked, “Do you feel close to any particular political party?” Those who answered affirmatively were then asked which party. In total, 53.2% of the sample registered support for a party, with the largest share (35.4%) expressing support for the incumbent party, DPP, and smaller shares indicating support for one of the opposition parties (13% for UDF, 3.5% for MCP, 1.3% for AFORD, and .1% for others). The distribution of party support in our survey area mirrors national-level trends found in the 2008 Afrobarometer survey.¹¹ We also include a measure of whether respondents are “minority partisans” in their villages, supporting a party other than the party thought to be supported by most people in the village.

We test for targeting along ethnic lines, given the centrality of ethnic divisions and ethno-regional favoritism in Malawi's political history (Chirwa 1998; Vail and White 1989). We include a dummy variable for Lomwe respondents (the president's co-ethnics) and also include dummies for Malawi's other major ethnic communities, the Tumbuka, Chewa, and Yao. In our survey area, these four groups make up 93.9% of the sample. We also include an indicator of whether respondents come from minority groups within their villages, using data from the headmen survey on the majority ethnic group at the village-level.

Following Pan and Christiaensen (2012), who found that local elites in Tanzania tended to capture the benefits of a similar subsidy program, the analysis includes several measures of social stature to test whether those in leadership positions within their communities may have been more likely to benefit. We include indicator variables measuring whether respondents were members of the Village Development Committee, the Chief's Council, or the District Development Committee in 2008.

To test whether the program benefitted the most needy, we include multiple measures of socio-economic status. First, we include a measure of wealth constructed as an index of household asset ownership.¹² We include three measures (taken from the 2010 survey) of whether respondents experienced negative economic shocks in the year prior to the 2009/10 growing season. These relate to: 1) loss of income, 2) poor crop yields, or 3) the death or serious illness of an adult member of the household. Though only the second measure explicitly references an agriculturally related loss, all three measures capture severe shocks that make households particularly vulnerable to food insecurity. We also include a measure

¹¹Based on a national survey of 1,200 respondents conducted in October–November 2008, the Afrobarometer found that 67.7% of Malawians “felt close” to a political party, a difference of 14.5% relative to respondents in our survey area. Among these respondents, the distribution of support across the major parties was similar to that found in our survey area, with the largest share (48.3%) supporting the incumbent DPP, and smaller shares supporting the UDF (9%), the MCP (6.3%), and AFORD (0.4%).

¹²Wealth is measured by an asset index constructed using principal components analysis based on a series of 17 questions that asked about household asset ownership (e.g., radio, television, bicycle, etc.). In poor countries, asset indices are generally seen as a better measure of wealth than income, which can vary considerably over time (Filmer and Pritchett 2001).

of farm size (in acres) and standard demographic measures: age, education, and whether the household was headed by a female.

We use logistic regression to examine individual-level subsidy targeting, and include village fixed effects to account for village-level differences that might affect access to coupons, including politically-motivated targeting across villages. We cluster standard errors by household because in some cases multiple respondents (husbands and wives) were interviewed in the same household. Table 1 shows the results and Figure 1 plots the marginal effects of each variable holding other covariates at their mean values. Figure 1 indicates that, conditional on village, the subsidy was not targeted with respect to party preferences or ethnicity in our survey area. Supporters of the incumbent party (the DPP) in 2008 were no more or less likely to benefit from the subsidy program in 2009/10 (relative to those who did not express support for any particular party in 2008). Likewise, opposition party supporters were no less likely to receive the benefit, nor were individuals who were minority partisans in their villages. The results also show that, relative to members of smaller ethnic groups (Ngoni, Sena, Tonga, Senga, and “other”), respondents from Malawi’s major ethnic communities were no more or less likely to receive the subsidy during the 2009/10 season. We also entered each ethnic dummy variable individually in additional tests (results not shown) and found no evidence of ethnic targeting in these specifications. Likewise, we find no evidence of discrimination against individuals from minority groups at the village level.

Consistent with findings by other scholars (e.g., Chirwa and Dorward 2013; Kilic, Whitney, and Winters 2015), we fail to find evidence that Malawi’s AISP effectively targeted those with greatest need, despite the program’s stated goal of reaching those most at risk for food insecurity. Our results show that neither poorer respondents, nor those with smaller land holdings were more likely to receive the subsidy. Moreover, respondents who experienced an economic shock related to loss of crops, livestock or income in the previous year were no more likely to benefit from the AISP than those unaffected by such income shocks. Further, households that suffered the death or serious illness of an adult in the previous year were actually less likely to receive the input subsidy. Together, these results suggest the program did not successfully target those with greatest need – poor, smallholder farmers threatened by food insecurity.

The results on demographic factors show that age and education were unrelated to receiving the subsidy. We find negative and significant gender effects. In our survey area, female-headed households were 10.4% less likely to receive the subsidy. Given that the models control for a wide range of factors that might affect levels of need, social stature, and economic shocks, the finding reported here suggests that female heads of households were less likely to benefit from the program as a result of gender discrimination rather than other factors that might be correlated with gender.¹³ Finally, the results indicate that members of

¹³We also considered the possibility that because the AISP program sought to target farmers who would be able to use the subsidized inputs efficiently to expand maize production, they may have avoided giving the subsidy to female-headed households that might have faced greater labor constraints. We doubt this interpretation, however, because the data show that in our sample female-headed households on average produced nearly as much maize as male-headed households (16.4 vs. 21.3 50-kilogram bags in the 2007/08 growing season), suggesting that labor constraints were not a significant inhibitor of production for most female-headed households.

district and village development committees or chief's councils were not statistically significantly more likely to benefit from the program.

6. Effects on Party Support?

Did the agricultural subsidy increase support for the incumbent party? To answer this question we examine trends in party support among recipients and non-recipients across the two survey rounds. Our measure of political preferences – taken from questions on the 2008 and 2010 surveys that asked respondents whether they “feel close” to any party – sets a high threshold for finding a positive effect. Similar studies typically use measures of vote choice (not available on the MLSFH surveys used here), which are likely to be more fluid and potentially subject to influence by anti-poverty programs. Our measure probes deeper connections between voters and parties, and is therefore less likely to be influenced by short-term changes in government policy.¹⁴ Moreover, Zucco's (2013) study of the political effects of a conditional cash transfer program in Brazil found evidence of an effect only on vote choice and not on partisanship. Thus, the measure of party support available in our survey data in all likelihood biases against finding a positive effect of the subsidy on political preferences. A second challenge relates to the nature of the treatment effect we seek to estimate. Sociotropic theories of economic voting have found that voters may punish and reward incumbents based on the overall performance of the economy (e.g., Markus 1988). If voters in Malawi base assessments of the incumbent on aggregate outcomes – rather than their own personal welfare – the subsidy program may represent a treatment that was received by all Malawians. Studies have shown that the AISP contributed to reduced food prices in Malawi, indicating that Malawians may have benefited indirectly even if they did not receive subsidy coupons (Dorward et al. 2010). These factors also bias the analysis against finding a connection between individual measures of subsidy reception and party support.

Despite these challenges, the data suggest that the subsidy did affect political preferences. Table 2 compares the increase in support for the DPP between 2008 and 2010 among those who did and did not receive the 2009/10 subsidy. Among non-recipients, the percent expressing support for the DPP rose by 3.4%, from 34.8% to 38.2%, while for those who did receive the subsidy, the increase was approximately three times larger: 9%, from 35.3% to 44.3%. Thus, receiving the subsidy in 2009/10 is associated with a 5.6% increase in DPP support.

While these results are suggestive of a treatment effect, there is, of course, the possibility that subsidy recipients differed from non-recipients in some important ways, and these underlying differences – not the subsidy – account for the greater increase in DPP support among recipients, relative to non-recipients. We are reassured by the results from the previous section, which showed that the 2009/10 subsidy was largely untargeted in our survey area, particularly with regard to prior party support and the primary demographic

¹⁴Data from the Afrobarometer Round 4 survey conducted in Malawi in late 2008 show that the “feel close” measure provides a good proxy for electoral preferences (details are provided in the online appendix). We follow Dalton and Weldon (2007) in treating this measure as tapping affinities toward parties that are likely to be separate from immediate vote choice even in contexts where there is a close relationship between such affinities and electoral decisions.

factor – ethnicity – that has traditionally been associated with party support in Malawi. Nonetheless, differences might remain that could confound the treatment estimation.

Our estimation strategy follows Angrist and Pischke (2009), who advocate the use of two alternative approaches for dealing with omitted variable bias in panel data. The first employs a lagged dependent variable (in our case party preferences) to account for the possibility that the subsidy was targeted based on pre-treatment trends in party support. The second uses a difference-in-difference model that eliminates possible confounding by time-invariant individual characteristics (observed and unobserved) and controls for a number of time-variant factors that might be correlated with the treatment and party preferences. In conjunction, these approaches are useful for bracketing the estimated effect when the potential sources of omitted variable biases are unknown (Angrist and Pischke 2009, p. 246). The two approaches produce estimates of a statistically-significant treatment effect ranging from 6.2% to 7.5%.

The first specification, which we refer to as the lagged-dependent variable (LDV) model, accounts for a large number of potential confounding variables, drawing both on existing studies of electoral preferences in Africa and on the analysis presented in the previous section. We include all variables from the analysis of targeting in the previous section, as well as several other covariates. We briefly explain the rationale for their inclusion. Unless otherwise specified, we measure these variables using data from the 2008 survey, prior to the distribution of the 2009/10 subsidy.

First and foremost, it is important to include the measures of party support from the first (2008) survey round. We speculate that those who did not feel close to any party in 2008 might be more responsive to the subsidy, given weaker pre-existing party ties. Likewise, we expect that opposition supporters might be particularly resistant, given their pre-existing attachments.

It is also important to account for ethnicity, given that members of some communities might be more likely to become DPP supporters for reasons other than the subsidy program. In particular, members of the president's own ethnic group (the Lomwe) might be more likely to follow Mutharika to the DPP than members of other communities. The Chewa, who have long been associated with the Malawi Congress Party (MCP), might be particularly disinclined to become DPP supporters. The Yao might also be particularly disinclined to become DPP supporters given the acrimonious split in 2005 between president Mutharika and former president Bakili Muluzi, a Yao. Finally, since the demise of the AFORD party following the 2004 elections (Tsoka 2009), the Tumbuka have been less tied to a particular political party and might therefore be more likely to move toward the DPP. In addition, we include a measure of whether individuals come from minority groups within their villages.

We also include a number of variables found to be associated with subsidy reception based on our analysis of targeting in Figure 1. We account for the possibility that individuals with local political ties might be more likely to become DPP partisans by controlling for membership on district development committees, village development committees, and the chief's council. We also account for individual economic shocks – loss of crops, loss of

income source, and the death or illness of a family member – that might reduce support for the incumbent party, based on research from the United States that shows that voters punish incumbents when their personal well-being is affected by natural disasters and other unforeseen events (e.g., Achen and Bartels 2012; Healy, Malhotra, and Mo 2010). We include measures of these shocks in both 2008 (from the 2008 survey) and 2009 (from the 2010 survey). We also include variables that track participation in the many other government anti-poverty programs in Malawi to account for possible correlation with subsidy reception. Specifically, we account for participation in the following programs (measured in the 2010 survey): free food/maize distribution, food-for-work, inputs-for work, scholarships for secondary education; scholarships for tertiary education, and direct cash transfers. We include a measure of subsidy reception in the previous year (2008/09) to account for the possibility that the 2009/10 subsidy may have targeted individuals whose views of Mutharika were in transition due to the prior year's subsidy.¹⁵ Finally, we account for demographic factors – age, education, farm size, income, and households headed by women – that could affect the strength of pre-existing partisan ties and therefore the likelihood of changing partisan allegiances.

In the previous section, our analysis showed minimal evidence of targeting with regard to variables measured in 2008. However, because multicollinearity between variables could reduce the significance of variables in the targeting model, we test for differences between control and treatment groups on each variable individually and add those that were not included in the analysis of targeting. Following Ho et al. (2007), we test for differences of means (using t-tests) and differences in distributions (using Kolmogorov-Smirnov tests). The balance statistics (shown in Table A1 in the online appendix) reveal statistically significant differences on several covariates, indicating the need to control for these factors.

We estimate a logit model of DPP support in 2010 that controls for all variables described above and includes village fixed effects to account for possible targeting across villages in our survey area. We cluster standard errors by household. The results, presented in column 1 of Table 3, show an estimated treatment effect (7.5%) that is significant at the $p < .05$ level. Full logit results are shown in Table A2 in the on-line appendix.

As an alternative way to address covariate imbalance, we employ matching before estimating the effect of the subsidy on party preferences using the LDV approach. For this we use the Coarsened Exact Matching (CEM) approach developed by Iacus, King, and Porro (2012). Matching works by creating matched pairs between those who received the 2009/10 subsidy and those who did not that are similar along observed covariates. Respondents that are not matched are excluded from the analysis, thereby improving overall balance on relevant factors between the treated and untreated groups. The advantage of matching is that one can account for possible confounds through pre-processing rather than controlling for confounds in a parametric model. The parametric approach relies on assumptions about the

¹⁵As a robustness test, we also include controls for receipt of the subsidy in prior years (2005/06, 2006/07, and 2007/08). Results are similar despite the drop in the sample size due to some missing data on subsidy reception in prior years. The results are shown in Table A9 in the online appendix.

functional form between confounds and the outcome variables, which if incorrect can bias the estimate. Matching, by contrast, makes no such assumptions (Ho et al., 2007).

We match on variables that we consider to be most relevant based on theoretical importance and the balance statistics shown in Table A1 in the on-line appendix: region, prior partisanship, membership in the village development committee, whether respondents experienced an illness or death in the family within the last two years, female-headed household, and age. We limit the matching to this set of variables because including additional variables greatly reduces sample size and because we are able to improve imbalance by matching on this set of variables (as shown in Table A1). We use matching with replacement, which has the advantage of producing better matches and dropping fewer observations than one-to-one matching (Blackwell, Iacus, King, and Porro 2009). Matching eliminates 105 observations (roughly 6%) by dropping 86 treated observations and 19 untreated respondents. We account for remaining imbalance by including all covariates in the estimation of the treatment effect, as recommended by Ho et al. (2007). Column 2 in Table 3 presents the results from a logit model employing the matched data. The model produces a similar estimate of the treatment effect (6.9%), comparable to the estimate in the pre-matching results, and again the estimated effect is statistically significant.

The second estimation strategy uses a difference-in-difference approach designed to account for all time-invariant individual-level factors that could be correlated with both subsidy reception and party preferences. For this, we estimate a pooled OLS model that includes a dummy variable for treatment condition, a dummy for the time period, and the interaction of the two. Specified in this way, the model is equivalent to a two-way fixed effects model that includes both individual fixed effects and a period dummy. With this specification, the only potential omitted variables of concern are time-varying factors that might be correlated with both treatment status and partisan preferences. As in previous models, we include measures of economic shocks that occurred between the two survey rounds and which might be correlated with both subsidy reception and party preferences. These include measures of whether the respondent's household experienced a loss of crops or livestock, the loss of an income source, or the death or serious illness of an adult family member. We also include measures of household participation in a host of other government-sponsored anti-poverty programs: food distribution, food-for-work, inputs-for-work, scholarships for secondary and tertiary education, and cash transfers. All time-invariant factors from previous models – such as gender, education, ethnicity, and village – are excluded by design as these factors (and other unobserved time-invariant factors) are accounted for by the specification. The results, shown in column 3 of Table 3, indicate an estimated treatment effect of 6.2% that is again significant at conventional levels (full results are shown in Table A3 in the on-line appendix).

7. Discussion

In this section we discuss the limitations of our estimation strategy and relate our findings to relevant literatures. With regard to the methods used to identify the effects of the subsidy on political orientations, the main limitation is that because the program was not randomly distributed, we cannot entirely rule out omitted variable bias with regard to unobserved

factors. To address this concern, we use the sensitivity test developed by Rosenbaum (2002) to estimate the extent to which our results are potentially driven by one or more omitted factor. This test estimates how large an effect (in terms of increasing assignment to the control or treatment group) one or more omitted variables would have to be to overturn the estimated treatment effect. For this analysis, we re-estimate the treatment effect using the LDV approach after conducting one-to-one matching on the same set of covariates used above (details are provided in the on-line appendix in Section 8, under “Sensitivity analysis”). The results again indicate a statistically significant effect of the subsidy on preferences, with the estimated size of the effect (11.9%) being somewhat larger. The Rosenbaum bounds test shows that one (or more) omitted variable would have to increase the likelihood of respondents receiving the subsidy by 16%, after having already accounted for the rich set of covariates we use as controls, in order to overturn the finding. There is no agreed standard for evaluating the results of Rosenbaum bounds. We can, however, compare the results to other known factors that affect assignment. Looking at the marginal effects of factors found to be statistically significant in our analysis of targeting (Figure 1), we find that those who had experienced an illness or death in the household in the last two years were 7.8% less likely to benefit, and households headed by females were 10.4% less likely to receive it. Thus, to overturn the positive finding on the effect of the subsidy on preferences, one or more omitted variables would have to exert a larger effect on assignment than these covariates for which we have measures, after having accounted for these factors and all others included in the estimation. While not impossible, it seems unlikely that our findings are due to omitted variable bias, given that none of the measured covariates exerts an effect of this magnitude.

A second limitation relates to the short duration of the period under study. Ideally we would like to know the full extent to which the program affected partisan attachments and electoral behavior. Our data, of course, only allows us to examine the effects of the program across a two-year time span. It is reasonable to believe that the effects we identify likely hold more broadly across the program's implementation. However, it is also possible that the effects will diminish over time as the program becomes a more routine aspect of Malawian life. It is also possible that the program will have less of an effect during periods when the party system is more stable. It is particularly important to note with regard to the enduring effects of the subsidy that the death in office of president Bingu wa Mutharika in 2012, and the subsequent struggle over succession created a disruption to the political continuity of claiming credit for the AISP (Dionne and Dulani 2013), even though the program itself continued. In the 2014 election, two of the main parties promised to continue some version of the AISP going forward. Mutharika's brother and the DPP presidential candidate Peter Mutharika boasted during the 2014 election campaign that his party had a “good track record” in managing the program and promised not just to continue the AISP, but to abolish the coupon program and expand the subsidy so as to “make the subsidized fertilizer available to every maize subsistence farmer who needs it” (Democratic Progressive Party 2014). Then-president Joyce Banda and her ruling party proposed during the election campaign that in the next administration the fertilizer program would be scaled back, offering fertilizer loans instead of subsidies (People's Party 2014). Though covered in major party manifestoes, the AISP was not a central issue in the 2014 election campaign. Instead,

voters and politicians alike were pre-occupied with a major corruption scandal implicating the Banda administration (Dulani and Dionne 2014). There is no indication that different proposals for the AISP's continuation had any impact on the election outcome.

In relating our findings to prior scholarship, it is noteworthy that we found no evidence of targeting with regard to partisanship or ethnicity in our survey area. Our results should not be taken to mean that Malawi's AISP was untargeted with regard to ethnicity or partisanship at the national level. Because our survey data comes from three of Malawi's 28 districts, it is not well suited for evaluating possible district-level targeting. Moreover, our primary goal was to examine targeting within villages, not targeting across larger geographic units. Nonetheless, they are similar to those reported by Brazys, Heaney, and Walsh (2015), which show no evidence of partisan targeting across constituencies using data from around the same time in Malawi. These results contrast with studies conducted elsewhere that found evidence of targeting toward opposition supporters in Ghana (Banful 2011) and core supporters in Zambia (Mason, Jayne, and van de Walle 2013). The difference likely stems from the unit of analysis; their findings reflect targeting larger geographic units, while ours demonstrate the absence of targeting at the individual level, findings that are not inconsistent, given that targeting across space is likely to be easier than targeting individuals within villages. Targeting ought to be much easier across higher-level units because leaders can use voter records and ethnic information to identify core or swing *areas*. Targeting is harder to accomplish with regard to individuals at the village level. So, we might expect to see targeting across geographic units (or ethnic groups) without seeing evidence of such targeting across individuals. We hesitate to draw strong inferences about what our results might mean for larger debates about core and swing targeting, given that the unit of analysis is different (most studies of targeting examine distribution across districts or other units) and we cannot speak to those findings with our individual-level results. Our results regarding ethnic targeting contrast with Brazys, Heaney, and Walsh (2015), who provide evidence of ethnic targeting consistent with a model in which some groups may have been more receptive to Mutharika's efforts to court swing voters. Again, as with partisanship, our null results with regard to ethnicity likely stem from the fact that we test for individual-level targeting within villages while Brazys, Heaney, and Walsh (2015) explore targeting across geographic units, the level at which we would expect targeting to be more easily accomplished.

Beyond political and ethnic factors, our results support an emerging consensus in studies both from Malawi and elsewhere that, despite the stated goal of reaching the worst off, subsidy programs often fail to target marginalized groups and those with greatest need: female-headed households, poorer farmers, and those who have recently experienced economic shocks (e.g., Chirwa and Dorward 2013; Mason, Jayne, and van de Walle 2013). Like other studies, our findings also suggest that those in positions of power at the local level were more likely to obtain benefits (e.g., Pan and Christianson 2012; Kilic, Whitney, and Winters 2015)

With regard to the estimated effects of the subsidy on support for the incumbent, our results cohere with those from Brazys, Heaney, and Walsh (2015), which shows a positive association between subsidy coupons and vote share at the constituency level in Malawi. Our

results complement these findings by providing rigorous micro-level evidence with data that is better suited for estimating the treatment effect in the context of a targeted program where confounding is a central challenge. The findings, however, contrast with those from Mason, Jayne, and van de Walle (2013) who show that a similar subsidy program in Zambia had no effects on support for the incumbent. While we can only speculate, it is possible that the Zambian program failed to win new supporters because it was targeted toward existing strongholds or because ethno-regional patterns of voter support exerted a stronger effect on voters than in Malawi during the period we study. The effects we identify in Malawi may be unusually large due to the volatility in the party system during the period under study. As noted earlier, the period between Malawi's 2004 and 2009 elections witnessed a dramatic reorientation of party alignments due to the fragmentation of the elite coalition that had brought Mutharika to power in 2004. While fluid elite coalitions are not uncommon in Africa's personalistic party systems, the effects we observe from the subsidy program are likely to be smaller in a more stable party system.

Finally, it is also important to contextualize these effects. In Malawi's case, the agricultural subsidy program is one of several factors that help to explain the reorientation of political preferences after the 2004 election. Other factors identified in prior research, including elite coalitions (Arriola 2012), symbolic gestures toward opposition supporters, and other policy choices likely also account for Mutharika's ascent during this period (Ferree and Horowitz 2010), as does the weakly institutionalized nature of the Malawian party system (Rakner, Svåsand, and Khembo 2007).

8. Conclusion

This paper examines the political effects of a major anti-poverty initiative in Malawi, the Agricultural Input Subsidy Programme, and shows that the subsidy increased support for the ruling party over the two-year period of the study. What do these findings imply for the larger literature on distributive politics and African political economy? First, the findings suggest that scholars of distributive politics ought to pay greater attention to non-particularistic programs. Nearly all of the literature on material transfers starts from the assumption that leaders seek to maximize the political returns on such programs by targeting benefits toward particular segments of the electorate. We suspect, however, that demographic and other constraints often inhibit targeting, as in the case explored in this paper. Thus, while recent work on clientelist exchanges has greatly advanced understanding of when and how parties direct patronage flows, examining the logic of non-targeted programs (and their political effects) would enhance appreciation of the fuller range of distributive strategies available to political leaders.

Second, the findings reported in this analysis have implications for theories of voter behavior in Africa's emerging democracies. Standard accounts stress the importance of ethnic and regional identities, emphasizing the ways in which identity cements in place partisan ties. Our findings suggest, however, that even in settings where ethnic and regional factors are politically salient, implementing programs that are popular with citizens can yield political rewards. These implications are not specific to anti-poverty initiatives, but suggest other

agricultural policies that benefit many and multiple groups of citizens – whatever the political or pragmatic intention – can generate support to the ruling party.

Finally, our findings are also relevant to the broader study of electoral competition and partisanship in Africa. Recent scholarship on electoral outcomes and voter behavior in Africa has emphasized the importance of elite coalitions (Arriola 2012), ethnic identities (Posner 2005; Ferree 2010), party strategies (Resnick 2013), historical legacies (Riedl 2014), patronage transfers (Jablonski 2014), and service delivery (Harding 2015). As anti-poverty programs – including agricultural initiatives – become increasingly common across the continent, our results suggest that they may help to explain patterns of party affiliation and vote choice, particularly where traditional patterns of partisan affiliation related to ethnic or regional identities weaken.

Our study stops short in exploring the potential strategic interaction between government and voters. Our analytical framework begins after the government has decided to provide a widely distributed agricultural subsidy. Building on our findings and other work examining the political economy of agricultural interventions more broadly (e.g., Anderson 2010; Mockshell and Birner 2015), future research could examine agricultural subsidy programs in the context of the core/swing debate, identifying the relevant conditions under which one approach will be preferred to the other. Given our results that Malawi's AISP increased support for the ruling party and the evidence in Brazys, Heaney, and Walsh (2015) that the previous Mutharika government politically targeted subsidy distribution to certain districts, it will be interesting going forward to see if and how the new Mutharika government – and other leaders in Africa – will target future subsidy programs.

On-line Appendix

1. Research location

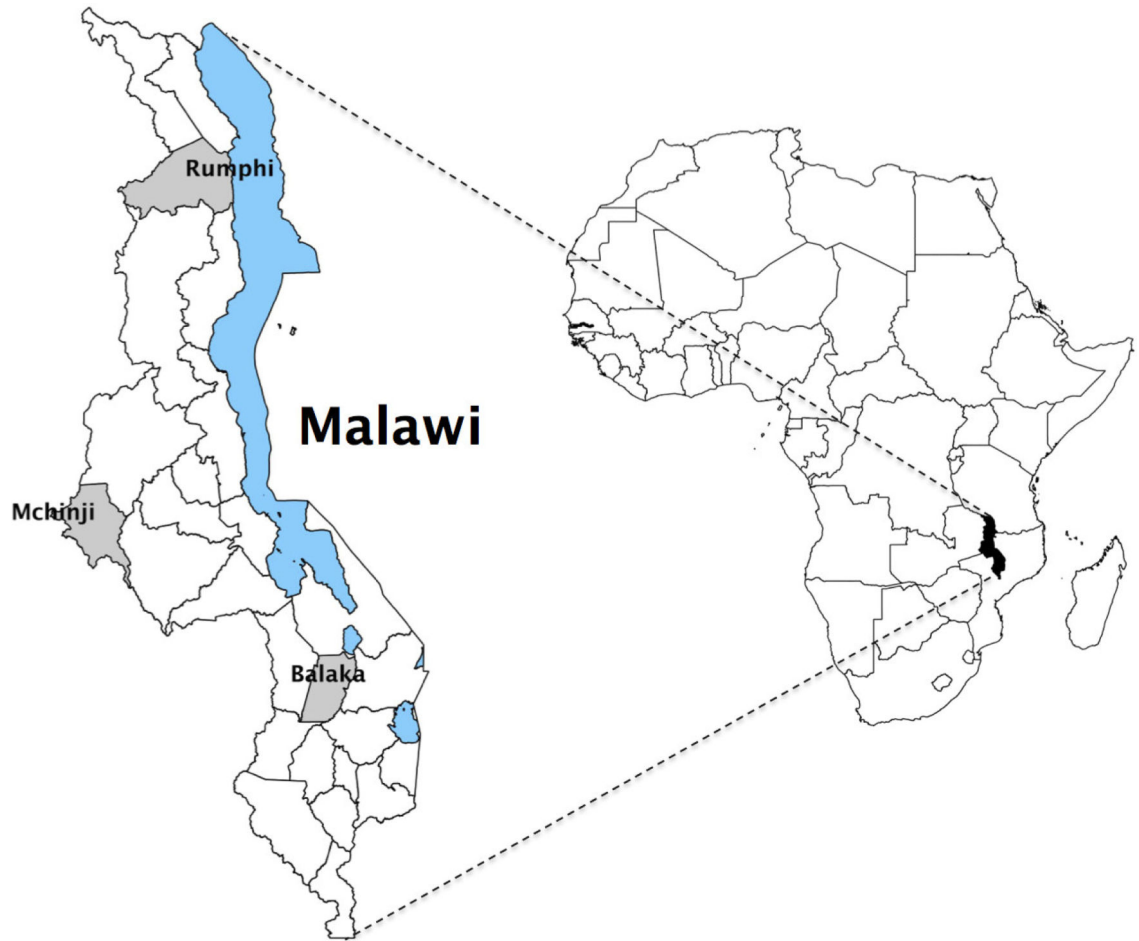


Figure A1.
Research Sites

2. Full results for models of the subsidy's effects on partisanship

	Full Sample				Matched Sample			
	No subsidy	subsidy	t-test p-val	KS-test p-val	No subsidy	subsidy	t-test p-val	KS-test p-val
DPP partisan	0.348	0.353	0.837	1.000	0.351	0.351	1.000	1.000
Opposition partisan	0.187	0.174	0.497	1.000	0.166	0.166	1.000	1.000
No partisan	0.465	0.474	0.747	1.000	0.484	0.484	1.000	1.000
Minority partisan in village	0.078	0.072	0.657	1.000	0.076	0.065	0.434	1.000
Received subsidy in 2008	0.329	0.229	0.000	0.002	0.336	0.225	0.000	0.000
Death or serious illness of adult, 2008	0.093	0.060	0.013	0.828	0.045	0.045	1.000	1.000
Loss of income source, 2008	0.119	0.113	0.725	1.000	0.133	0.116	0.338	1.000
Loss of crop / livestock, 2008	0.072	0.055	0.163	1.000	0.068	0.056	0.357	1.000
Death or serious illness of adult, 2009	0.188	0.196	0.713	1.000	0.196	0.201	0.804	1.000
Loss of income source, 2009	0.430	0.318	0.000	0.000	0.424	0.314	0.000	0.001
Loss of crop / livestock, 2009	0.248	0.229	0.396	1.000	0.249	0.231	0.418	1.000
Village Development Committee	0.264	0.329	0.008	0.096	0.316	0.316	1.000	1.000
Chief's Council	0.087	0.115	0.084	0.936	0.106	0.114	0.658	1.000
District Development Committee	0.142	0.161	0.338	1.000	0.161	0.155	0.733	1.000
Female head of household	0.148	0.090	0.000	0.184	0.068	0.068	1.000	1.000
Age	39.508	40.105	0.475	0.077	38.175	39.450	0.115	0.079
Education	1.021	1.001	0.651	0.973	1.049	1.004	0.286	1.000
Wealth index	0.069	0.158	0.407	0.671	0.253	0.164	0.421	0.316
Farm size	3.284	3.549	0.092	0.231	3.433	3.532	0.528	0.476
Lomwe	0.033	0.028	0.576	1.000	0.028	0.028	0.980	1.000
Yao	0.296	0.223	0.001	0.042	0.232	0.229	0.916	1.000
Chewa	0.247	0.295	0.044	0.383	0.297	0.290	0.752	1.000
Tumbuka	0.366	0.392	0.319	0.972	0.386	0.393	0.795	1.000
Minority group in village	0.152	0.150	0.883	1.000	0.151	0.149	0.919	1.000

Table A2

Logit Models of 2010 DPP Support

	Full Sample	Matched Sample
Received subsidy in 2009/10	0.311 ^{**} (0.033)	0.287 [*] (0.053)
DPP partisan, 2008	0.965 ^{***} (0.000)	0.909 ^{***} (0.000)
Opposition partisan, 2008	-0.099 (0.565)	-0.068 (0.725)
Received subsidy in 2008/09	0.202 (0.128)	0.100 (0.477)
ES1 2008: Death or serious illness of adult	0.051 (0.832)	-0.098 (0.741)
ES2 2008: Loss of crop / livestock	-0.120 (0.492)	-0.109 (0.559)
ES3 2008: Loss of income source	-0.257 (0.272)	-0.123 (0.610)
ES1 2009: Death or serious illness of adult	-0.214 (0.153)	-0.213 (0.167)
ES2 2009: Loss of crop / livestock	0.093 (0.429)	0.096 (0.441)
ES3 2009: Loss of income source	0.007 (0.960)	0.023 (0.875)
Participant in food distribution program	-0.247 (0.258)	-0.121 (0.611)
Participant in food-for-work program	0.319 [*] (0.088)	0.098 (0.623)
Participant in inputs-for-work program	-0.385 (0.120)	-0.313 (0.213)
Recipient of scholarship for secondary education	-0.035 (0.934)	0.104 (0.820)
Recipient of scholarship for tertiary education	-0.051 (0.931)	0.126 (0.843)
Participant in cash transfer program	-0.120 (0.719)	-0.455 (0.183)
Member of Village Development Committee	0.263 [*] (0.079)	0.265 (0.106)
Member of Chief's Council	0.016 (0.925)	0.067 (0.705)
Member of District Development Committee	-0.223 (0.190)	-0.241 (0.194)
Female head of household	-0.397 ^{**} (0.049)	-0.436 [*] (0.071)
Age	0.002 (0.628)	0.002 (0.594)
Education	-0.001 (0.988)	0.018 (0.785)
Wealth index	0.004 (0.893)	-0.016 (0.652)
Farm Size	-0.031 (0.145)	-0.027 (0.270)
Lomwe	0.067 (0.895)	-0.202 (0.740)
Yao	-0.179 (0.727)	-0.026 (0.961)
Chewa	-0.708 ^{**} (0.030)	-0.438 (0.178)
Tumbuka	0.600 (0.212)	0.893 [*] (0.074)
Minority ethnic group in village	0.353 (0.225)	0.455 (0.135)
Minority partisan in village	0.090 (0.660)	0.068 (0.786)
Constant	-0.777 (0.335)	-1.109 (0.171)
Village fixed effects	Yes	Yes
Observations	1,751	1,655
Pseudo R-squared	0.135	0.134

Notes: The dependent variable in all models is a binary measure equal to 1 if respondents reported feeling close to the DPP in the 2010 survey. Coefficients reported with robust p-values in parentheses; standard errors clustered by household.

p<0.01
**
p<0.05
*
p<0.1

Table A3

Difference-in-Difference Results

Treatment group * Round 2	0.062 ^{**} (0.031)
Treatment group	0.007 (0.025)
Round 2	0.025 (0.031)
Received subsidy in 2008	0.032 (0.027)
ES1 2009: Death or serious illness of adult	-0.048 (0.029)
ES2 2009: Loss of crop / livestock	0.019 (0.025)
ES3 2009: Loss of income source	-0.008 (0.028)
Participant in food distribution program	0.009 (0.029)
Participant in food-for-work program	-0.025 (0.029)
Participant in inputs-for-work program	-0.017 (0.046)
Recipient of scholarship for secondary education	-0.090 (0.069)
Recipient of scholarship for tertiary education	-0.019 (0.126)
Participant in cash transfer program	-0.006 (0.051)
Constant	0.351 ^{***} (0.022)
Observations	3,622
R-squared	0.009

Results from OLS two-way fixed effects model. Robust standard errors, clustered by respondent in parentheses.

* p<0.1

p<0.01

** p<0.05

3. Partisanship by region and ethnic group, 2008

We note in the text that at the time that allocation decisions for the 2009/10 subsidy round were being made, the traditional patterns of ethno-regional partisanship were temporarily upended. Data from a question in the Round 4 Afrobarometer survey that asked “Do you feel close to any particular political party?” illustrates this point.

Table A4

Partisanship by Region, October-November, 2008 (share)

	Population share	DPP	MCP	UDF	Other	None
North	.13	.68	0	.02	.03	.28
Center	.41	.43	.16	.08	.02	.32
South	.47	.50	.004	.11	.02	.36
Total		.50	.07	.09	.02	.33

Table A5

Partisanship by Ethnic Group, October-November, 2008 (share)

	Population share	DPP	MCP	UDF	Other	None
Chewa	.36	0.45	0.15	0.06	0.01	0.33
Lomwe	.17	0.59	0.01	0.06	0.01	0.33
Ngoni	.12	0.51	0.06	0.14	0.01	0.28
Yao	.12	0.33	0.01	0.30	0.01	0.36
Tumbuka	.11	0.63	0.01	0.02	0.05	0.30
Mang'anja	.06	0.53	0.02	0.05	0.00	0.41
Sena	.05	0.47	0.00	0.07	0.04	0.42
Tonga	.02	0.57	0.00	0.10	0.00	0.33

Notes: This table includes all ethnic groups larger than 1% of the sample.

4. Attrition and sample bias

Table A6 presents data on attrition by comparing the full 2008 sample to the 2010 sample used for the analysis in this paper. The differences between the two samples are attributed to panel attrition and the removal of mismatches (individuals who were interviewed twice but did not match on basic demographic variables). The table examines differences across demographic factors used in the analysis of the subsidy's effects on partisanship and identifies statistically significant differences on a number of covariates. However, the substantive size of the differences is in most cases relatively small. The one notable exception is that the final 2010 sample contains nearly 10% more Tumbukas than the original 2008 sample. It should be noted, however, that the original sample was not meant to be perfectly representative of the national population. The initial sample chose three representative districts, one in each of Malawi's three regions in order to produce a sample that would reflect the country's diversity.

Table A6

Attrition from 2008 to 2010 (all variables measured in 2008)

	2008 full sample (N=3909)	2010 analytic sample (N=1846)	Diff.	p-value
Female	0.578	0.597	0.019	0.024
Female head of household	0.127	0.106	-0.021	0.000
Age	41.462	39.948	-1.514	0.000
Education	0.964	1.007	0.043	0.003
Wealthindex	0.001	0.135	0.134	0.000
Farmsize	4.673	3.480	-1.193	0.000
Lomwe	0.043	0.029	-0.014	0.000
Yao	0.264	0.242	-0.021	0.004
Chewa	0.314	0.282	-0.032	0.000

	2008 full sample (N=3909)	2010 analytic sample (N=1846)	Diff.	p-value
Tumbuka	0.289	0.385	0.097	0.000

To see the extent to which the sample used in this paper matches the overall population, Table A7 compares the analytic sample (using measures from the 2008 wave) to survey data collected by the Afrobarometer around the same time. The table shows that relative to the national population, our sample is disproportionately non-partisan, female, Yao, and Tumbuka. For these reasons caution should be exercised in generalizing the results presented in this paper to the larger population.

Table A7

Comparison of Sample to National Sample (Afrobarometer Round 4)

	Sample mean	National mean (Afrobarometer)
DPP partisan (in 2008)	35.2	49.4
Opposition partisan	17.7	17.5
No partisan	47.1	33.1
Age	39.9	35.5
Education	Some primary	Some primary
Lomwe	2.9	16.9
Yao	24.2	11.8
Chewa	28.2	34.5
Tumbuka	38.5	11.0

Notes: All variables measured in 2008

5. Partisanship and vote choice

We note in the text that the measure of partisanship used in this paper likely serves as a good proxy for voting behavior. Data from the Afrobarometer Round 4 survey, conducted in Malawi around the time that our survey data was collected, supports this conclusion. Table A8 shows voting intentions for supporters of the three main parties at the time of the 2008 survey. Voting intentions are measured with a question that asked, “If a presidential election were held tomorrow, which party's candidate would you vote for?” The measure of partisanship comes from the same question used in our survey data, which asked, “Do you feel close to any particular political party? [If yes] Which party is that?” The data in Table A8 reveals a close correspondence between partisanship and voting intentions.

Table A8

Voting Intentions by Partisan Orientation (percentages)

	DPP	MCP	UDF	Other	DK	RTA	Would not vote
DPP partisans (49.5%)	93.1	0.5	0.7	0.5	0.9	3.6	0.2

	DPP	MCP	UDF	Other	DK	RTA	Would not vote
MCP partisans (6.5%)	13.2	76.3	1.3	2.6	1.3	5.3	0
UDF partisans (9.2%)	5.6	0.9	85.2	1.9	0.9	5.6	0
None (33.1%)	42.5	2.6	7.7	1.0	7.5	31.2	7.0

7. Controlling for prior subsidy reception

In our main lagged-dependent variable model (Table A2) we control only for whether respondents received the subsidy in the prior year, 2008/09. We do not include controls for receipt of the subsidy in earlier years for three reasons. First, a large body of literature suggests that voters tend to give greater weight to more recent events (e.g., Achen and Bartels 2004; Huber et al. 2012; Healy and Lenz 2014), suggesting that we should expect to see effects mainly from the most recent subsidy round and perhaps the prior year. Second, we do not have sufficient data to examine subsidy targeting with respect to partisanship prior to 2008, making it difficult to determine whether these variables might produce spurious results if they co-vary with demographic factors due to targeting. Third, the difference-in-difference model accounts for these time-invariant factors (receipt of these prior-year subsidies occurred before the 2008 wave and are therefore accounted for by individual fixed effects). Nonetheless, as a robustness test we re-run the main lagged dependent variable model including controls for additional prior years and find that the magnitude of the estimated effect of the 2009/10 subsidy remains similar (6.2% vs. 6.9%), though the significance of the coefficient falls just shy of conventional levels ($p=.114$). The increase in the p-value may stem from the smaller sample size due to missing data on subsidy reception in prior years (the model includes 1,556 observations, relative to 1,751 in the main model in Table A2).

Table A9

Logit Models of 2010 DPP Support

Received subsidy in 2009/10	0.249 (0.114)
DPP partisan, 2008	0.909 ^{***} (0.000)
Opposition partisan, 2008	-0.078 (0.676)
Received subsidy in 2008/09	0.203 (0.154)
Received subsidy in 2007/08	0.069 (0.733)
Received subsidy in 2006/07	0.201 (0.190)
Received subsidy in 2005/06	-0.290 ^{**} (0.028)
ES1 2008: Death or serious illness of adult	0.045 (0.859)
ES2 2008: Loss of crop / livestock	-0.136 (0.475)
ES3 2008: Loss of income source	-0.422 [*] (0.095)
ES1 2009: Death or serious illness of adult	-0.290 [*] (0.071)
ES2 2009: Loss of crop / livestock	0.010 (0.935)
ES3 2009: Loss of income source	0.027 (0.846)
Participant in food distribution program	-0.361 (0.114)

Participant in food-for-work program	0.387 [*] (0.056)
Participant in inputs-for-work program	-0.383 (0.126)
Recipient of scholarship for secondary education	-0.047 (0.909)
Recipient of scholarship for tertiary education	0.034 (0.955)
Participant in cash transfer program	-0.094 (0.778)
Member of Village Development Committee	0.258 [*] (0.088)
Member of Chief's Council	-0.004 (0.984)
Member of District Development Committee	-0.217 (0.233)
Female head of household	-0.363 (0.106)
Age	0.004 (0.367)
Education	-0.004 (0.962)
Wealth index	0.016 (0.635)
Farm Size	-0.042 [*] (0.062)
Lomwe	0.199 (0.724)
Yao	-0.005 (0.993)
Chewa	-0.622 [*] (0.071)
Tumbuka	0.587 (0.286)
Minority ethnic group in village	0.427 (0.164)
Minority partisan in village	0.092 (0.665)
Constant	-0.818 (0.356)
Village fixed effects	Yes
Observations	1,556
R-squared	0.135

Notes: The dependent variable in all models is a binary measure equal to 1 if respondents reported feeling close to the DPP in the 2010 survey. Coefficients reported with robust p-values in parentheses; standard errors clustered by household.

p<0.01

**

p<0.05

*

p<0.1

8. Sensitivity analysis

To examine the extent to which our main findings may be subject to possible omitted variable bias, we calculate Rosenbaum bounds (Rosenbaum 2002). As noted in the text, this approach requires creating matched pairs, which we generated using Coarsened Exact Matching (CEM) without replacement. For one-to-one matching, CEM randomly selects matches when there are multiple options within a stratum – e.g., when a stratum contains one control observation and three treatment observations. As a result, the estimated treatment effect may vary according to the matched pairs that CEM generates. While it is possible to specify a distance measure for use in matching within strata, there is no agreed standard for the appropriate measure to employ. We therefore prefer to use random matching within strata.

Given that the results of one-to-one matching with CEM vary according to how the random pairs are generated, we sought to identify the matched pairs that minimized imbalance

between the treated and untreated groups. To do so, we ran the CEM match 10,000 times, increasing the “seed” (the number with which Stata starts its algorithm to generate pseudo-random numbers) by 1 each time (ranging between 1 and 10,000). Using the L1 measure of global imbalance (see Iacus, King and Porro 2012), we found that imbalance was minimized with the seed set at 3722. All results are based on matched pairs generated using this seed.

The results produced a 442 matched pairs, dropping 44 untreated and 918 treated individuals. Matching was done using the same set of variables as used to match with replacement: region, prior partisanship, membership in the village development committee, whether respondents experienced an illness or death in the family within the last two years, female head of the household, and age. Balance statistics for this sample, shown in Table A10, indicate a high degree of balance across all covariates.

Table A10

Balance Statistics after Matching without Replacement

	No subsidy	subsidy	t-test p-val	t-test p-val
DPP partisan	0.355	0.355	1.000	1.000
Opposition partisan	0.176	0.176	1.000	1.000
No partisan	0.468	0.468	1.000	1.000
Minority partisan in village	0.075	0.075	1.000	1.000
Received subsidy in 2008	0.321	0.183	0.000	0.000
Death or serious illness of adult, 2008	0.054	0.054	1.000	1.000
Loss of income source, 2008	0.127	0.100	0.203	0.997
Loss of crop / livestock, 2008	0.068	0.066	0.879	1.000
Death or serious illness of adult, 2009	0.190	0.208	0.501	1.000
Loss of income source, 2009	0.437	0.314	0.000	0.003
Loss of crop / livestock, 2009	0.250	0.240	0.726	1.000
Village Development Committee	0.258	0.258	1.000	1.000
Chief's Council	0.088	0.104	0.425	1.000
District Development Committee	0.143	0.131	0.625	1.000
Female head of household	0.115	0.115	1.000	1.000
Age	38.224	39.111	0.409	0.644
Education	1.036	0.948	0.105	0.999
Wealth index	0.158	0.023	0.319	0.172
Farm size	3.266	3.388	0.518	0.809
Lomwe	0.029	0.041	0.361	1.000
Yao	0.299	0.274	0.414	0.999
Chewa	0.247	0.242	0.876	1.000
Tumbuka	0.369	0.385	0.628	1.000
Minority group in village	0.147	0.155	0.738	1.000

We then estimated the treatment effect on the matched pairs using the same methods as used above for the full matching estimation. The results, presented in Table A11, indicate an estimated effect of 11.9% (full results are shown in Table A12). To conduct sensitivity

analysis, we then employed propensity score matching with the CEM-matched data. This step is necessary because CEM does not identify the observations that matched (as required to generate Rosenbaum bounds), whereas propensity score matching (using `psmatch2` in Stata) does. We use the full set of covariates to estimate propensity scores. Given the high degree of balance already achieved with the CEM routine, all observations were retained with propensity score matching. Finally, we calculate Rosenbaum bounds using the propensity-score matched data.

Table A11

Estimated Treatment Effect

	CEM w/out replacement
Estimated treatment effect (%) (Standard error)	11.9 ^{***} (4.2)
$n_{Treated} / n_{Untreated}$	409/394

** p<0.05

* p<0.1

p<0.01**Table A12**

Logit Results on CEM-Matched Data (w/out replacement)

Received subsidy in 2009/10	0.506 ^{***} (0.006)
DPP partisan, 2008	0.982 ^{***} (0.000)
Opposition partisan, 2008	-0.570 [*] (0.051)
Received subsidy in 2008	-0.042 (0.846)
ES1 2008: Death or serious illness of adult	-0.184 (0.648)
ES2 2008: Loss of crop / livestock	-0.271 (0.324)
ES3 2008: Loss of income source	0.044 (0.899)
ES1 2009: Death or serious illness of adult	-0.043 (0.843)
ES2 2009: Loss of crop / livestock	0.219 (0.257)
ES3 2009: Loss of income source	0.275 (0.263)
Participant in food distribution program	-0.230 (0.542)
Participant in food-for-work program	-0.046 (0.888)
Participant in inputs-for-work program	-0.382 (0.364)
Recipient of scholarship for secondary education	-0.148 (0.827)
Recipient of scholarship for tertiary education	-0.073 (0.943)
Participant in cash transfer program	0.024 (0.967)
Member of Village Development Committee	0.671 ^{**} (0.029)
Member of Chief's Council	-0.073 (0.780)
Member of District Development Committee	-0.027 (0.923)
Female head of household	-0.481 (0.153)
Age	0.005 (0.426)
Education	0.097 (0.321)
Wealth index	-0.026 (0.606)
Farm Size	-0.125 ^{**} (0.011)

Lomwe	-0.358 (0.633)
Yao	-0.068 (0.918)
Chewa	-0.890* (0.081)
Tumbuka	1.532* (0.064)
Minority group in village	0.437 (0.373)
Minority partisan in village	0.218 (0.570)
Constant	-3.274*** (0.003)
Village fixed effects	Yes
Observations	808
Pseudo R-squared	0.186

The dependent variable is a binary measure equal to 1 if respondents reported feeling close to the DPP in the 2010 survey. Coefficients reported with robust p-values in parentheses; standard errors clustered by household.

p<0.01
**
p<0.05
*
p<0.1

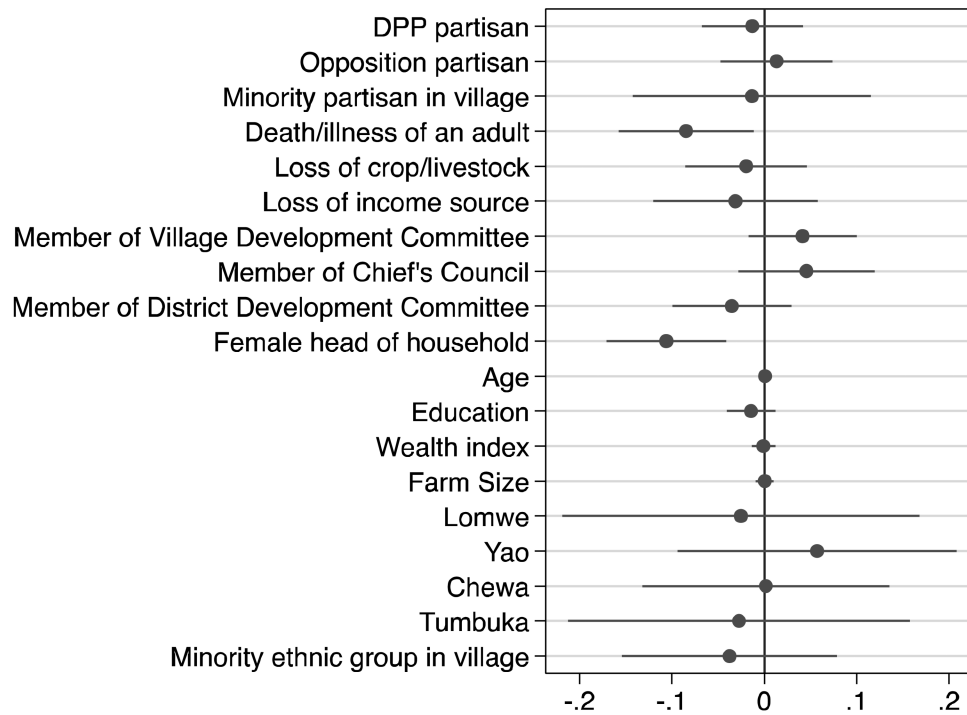
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Notes: This figures shows the marginal effect of each variable on the predicted probability of receiving a subsidy coupon during the 2009/10 growing season. The estimations are based on the logit results in Table 1. Marginal effects are calculated holding other covariates at their mean values. Error bars show 95% confidence intervals.

Figure 1.
Marginal Effects of Covariates on the Predicted Probability of Subsidy Reception in 2009/10

Table 1

Logit Model of Individual-level Targeting

DPP partisan, 2008	-0.073 (0.643)
Opposition partisan, 2008	0.075 (0.664)
Minority partisan in village	-0.074 (0.840)
Death or serious illness of adult	-0.474 ** (0.024)
Loss of crop / livestock	-0.109 (0.559)
Loss of income source	-0.174 (0.495)
Member of Village Development Committee	0.232 (0.170)
Member of Chief's Council	0.256 (0.222)
Member of District Development Committee	-0.196 (0.292)
Female head of household	-0.593 *** (0.002)
Age	0.005 (0.269)
Education	-0.080 (0.281)
Wealth index	-0.005 (0.883)
Farm Size	0.004 (0.896)
Lomwe	-0.141 (0.797)
Yao	0.320 (0.461)
Chewa	0.010 (0.979)
Tumbuka	-0.152 (0.773)
Minority ethnic group in village	-0.210 (0.534)
Constant	1.313 (0.134)
Village fixed effects	Yes
Observations	1,695
Pseudo R-squared	0.0869

Coefficients reported with robust p-values in parentheses; standard errors clustered by household.

* p<0.1

p<0.01

**
p<0.05

Table 2

DPP Support, 2008 to 2010

	2008	2010	Change
Did not receive 2009 subsidy (n=486)	34.8%	38.2%	+3.4%
Did receive 2009 subsidy (n=1,360)	35.3%	44.3%	+9%
<i>Difference</i>			<i>5.6%</i>

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Table 3

Estimated Effect of Subsidy Reception on DPP Support

	LDV (logit) ^a	LDV w/matched sample (logit) ^a	Diff-in-Diff (OLS) ^b
Estimated treatment effect (%)	7.5 ^{**}	6.9 [*]	6.2 ^{**}
(Standard error)	(3.5)	(3.6)	(3.1)
<i>n</i> Treated / <i>n</i> Untreated	1263/488	1213/442	1301/504

*** p<0.01

Notes: This table shows the marginal effect of subsidy reception on preferences (the predicted probability of expressing support for the incumbent party, DPP, at the time of the analysis). Full results for the three models are shown in Tables A2 and A3 in the online appendix. Marginal effects for each model are calculated holding other covariates at their mean values.

^aRobust standard errors, clustered by household, are shown in parenthesis.

^bRobust standard errors, clustered by respondent, are shown in parenthesis.

**
p<0.05

*
p<0.1