

Blomquist, Johan; Nordin, Martin

**Working Paper**

## Do the CAP Subsidies Increase Employment in Sweden? Estimating the Open Economy Relative Multiplier Using an Exogenous Change in the CAP

Working Paper, No. 2013:41

**Provided in Cooperation with:**

Department of Economics, School of Economics and Management, Lund University

*Suggested Citation:* Blomquist, Johan; Nordin, Martin (2013) : Do the CAP Subsidies Increase Employment in Sweden? Estimating the Open Economy Relative Multiplier Using an Exogenous Change in the CAP, Working Paper, No. 2013:41, Lund University, School of Economics and Management, Department of Economics, Lund

This Version is available at:

<https://hdl.handle.net/10419/260097>

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# Do the CAP Subsidies Increase Employment in Sweden? Estimating the Open Economy Relative Multiplier Using an Exogenous Change in the CAP

Johan Blomquist  
Martin Nordin

December 2013



**LUND**  
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# **Do the CAP Subsidies Increase Employment in Sweden? Estimating the Open Economy Relative Multiplier Using an Exogenous Change in the CAP**

Blomquist, Johan and Martin Nordin

## **Abstract**

This study evaluates the impact of agricultural subsidies (CAP) on unemployment and employment outside the agricultural sector. For the CAP subsidies to have an effect outside the agricultural sector, the subsidies must have a second-order effect. Thus, the Open Economy Relative Multiplier for Sweden is estimated with aggregate municipality data for the years 2001 to 2009. A side-effect of the decoupling reform in 2005 was that Sweden was forced to introduce a grassland support which redistributed the payments among the regions. This exogenous redistribution of the CAP is the identifying assumption in this study. The subsidy creates private jobs at a cost of about \$20,000 per job, which is consistent with earlier estimates based on US data.

**JEL classification:** E62, E24, Q18

**Key words:** Government Spending, Multiplier, Employment, Unemployment, CAP, Agricultural Subsidies

Correspondence to:  
Martin Nordin  
Department of Economics, Lund University  
Box 730 – Scheelevägen 15 D  
220 07 Lund  
Sweden

**Acknowledgements:** We would like to thank participants in seminars at the Department of Economics, Lund University and Agrifood for helpful comments and suggestions.

## 1. Introduction

The *Common Agricultural Policy* (CAP) is the EU-wide subsidy system directed at the agricultural sector. Besides the direct impact on agriculture, the subsidies may stimulate the rural economy in general, and increase employment in sectors other than agriculture. Although the agricultural sector is small, the CAP subsidies are large, and, in comparison to the total cost of labour market programmes in Sweden, which amounted to 8 billion in 2010 (Sibbmark 2011), the CAP payment is 25 percent higher. Investigating whether the CAP affects employment is thus important, since subsidizing basically well-functioning farms may have a large impact on the rural economy. For the CAP subsidies to have an effect on employment outside the agricultural sector, the subsidies must have a second-order effect, a so-called fiscal multiplier effect. It is the purpose of this paper to examine the size of this multiplier.

The effect of government spending on income and employment is a classical and unresolved question in economics. A recent review of the literature is provided by Ramey (2011). While some studies argue that fiscal stimulus can increase output and employment by more than the increase in spending (a multiplier larger than one), others report the multiplier to be much smaller, sometimes close to zero. The inconclusive results partly stem from the difficulty of estimating the fiscal multiplier. To analyze the effect of policies on economic outcomes, the policy variation has to be exogenous (Besley and Case, 2000), which is rarely the case when it comes to fiscal policy. One solution is to use some sort of natural experiment, which can provide pseudo-random variation in fiscal stimulus. Depending on the identification strategy, one finds a wide range of estimates of the fiscal multiplier. Because military spending is assumed to be the most exogenous component of government expenditures, variation in military spending is often used for estimating aggregate multipliers. However, wars are relatively infrequent, which implies that there might be insufficient variation in military spending to identify the multiplier effect (Barro and Redlick, 2011). A potential solution to this problem is to use variation in military spending across regions within a country. As pointed out by Nakamura and Steinsson (2013), regional military spending provides more variability than aggregate time series data.

There are at least two additional advantages of using regional data to examine the fiscal multiplier. Firstly, confounding countrywide factors (tax changes, monetary policy, and other macroeconomic shocks) are likely to be independent of the relative economic outcomes across regions. Secondly, even if government spending, such as military spending, may be

endogenous to the business cycle at the national level, the variation in military spending across regions is more likely to be exogenous to military spending decisions. Hence, the use of regional panel data helps us to identify the source of exogenous variation in government spending. Besides adding to the literature on agricultural subsidies by estimating the CAP's impact on rural employment (and unemployment), our study contributes to the literature on fiscal multipliers by using an exogenous subsidy redistribution that originates from the agricultural sector (instead of the military sector).

As noted by Ramey (2011), regional studies of the fiscal multiplier estimate the effect that government spending, in one region *relative* to another, has on *relative* economic outcomes. Nakamura and Steinsson (2013) coin the term “open economy relative multiplier” to distinguish it from the more familiar aggregate multiplier. Focusing on relative multipliers is interesting for at least two reasons. First, national government spending on the local level usually has a redistributive aspect in that spending varies across regions. For example, increasing the unemployment benefit, payments increase in regions with a high unemployment level relative to regions with a low unemployment level. Knowledge about how transfers from the national government to lower-level governments impact regional economies is therefore central to designing fiscal policy.

Second, as pointed out by Nakamura and Steinsson (2013), the open economy relative multiplier is an effective tool for distinguishing between macroeconomic models. While standard neoclassical models predict a small fiscal multiplier, the size of the multiplier in New Keynesian models depends critically on the monetary policy response (Woodford, 2011). A low aggregate multiplier arises if the central bank reacts to the inflationary effects of an expansionary fiscal policy by actively raising real interest rates. On the other hand, a less aggressive monetary policy can generate a large fiscal multiplier. The variation in values for the multiplier, depending on the monetary policy response, makes it difficult to use estimates of the aggregate multiplier to distinguish between competitive macroeconomics models. However, as Nakamura and Steinsson (2013) point out, the open economy relative multiplier is not sensitive to the specifications of monetary policy. This follows from the fact that relative monetary policy between regions is held fixed in a monetary union. Thus, by using regional panel data to estimate the relative multiplier, we can examine whether government spending generates large effects (consistent with New Keynesian models) or small effects (consistent with neoclassical models).

The financial crisis in 2008 has placed fiscal policy and multiplier effects in the focus of research, and recent literature estimates open economy relative multipliers using exogenous

variation in regional government spending (see e.g. Chodorow-Reich et al., 2010; Cohen et al., 2011; Fishback and Kachanovskaya, 2010; Serrato and Wingender, 2010; Shoag, 2010; and Wilson 2012). This literature has focused to a large extent on employment effects of fiscal stimulus, which is particularly interesting in times of high unemployment rates. Most papers find positive employment effects, with a cost of about \$25,000 to \$35,000 for each job created. Another interesting result of these studies is that the multiplier is significantly higher during times of higher unemployment (Shoag, 2010, Serrato and Wingender, 2011, Nakamura and Steinsson, 2013). This indicates that redistributing resources from low unemployment regions to high unemployment regions may result in sizable aggregate multipliers.

The aforementioned studies focus on fiscal stimulus in the U.S., and studies for other countries are scarce. For Sweden, Lundqvist, Dahlberg and Mörk (forthcoming) analyze whether a relative change in the grants to governments increases public (but not private) employment. Because a specific cost-equalizing grant compensates local governments with a net out-migration of above two percent, they use this discontinuity in a regression kink design (RKD) to identify the causal effect of government grants. They find that the grants increase public administration, but total public employment is unaffected. Criscuolo et al. (2012) investigate the Regional Selective Assistance (RSA) program, which gives grants to firms for investing in disadvantaged areas in Britain. By exploiting changes in the EU-wide rules for calculating the RSA, they find that at the regional level the program increases employment in the targeted areas.

This study uses an exogenous redistribution of the CAP subsidy in 2005 to investigate changes in employment outside the agricultural sector. The redistribution was truly exogenous since it was: (i) decided and funded at the European level and, (ii) a side-effect of a change in the EU-wide regulation for the CAP (the decoupling reform). With respect to (ii), the decoupling reform made Sweden introduce a grassland support, which implied that direct payments to farmers became directly related to their amount of grassland (more on this below). Our identifying assumption is that Sweden neither agreed to follow the new EU regulation, nor forced the EU to implement the new regulation, because regions that received disproportionate increases in payments were doing poorly relative to other regions. Interestingly, the decoupling reform changed the subsidy scheme dramatically, and the payments doubled in some municipalities (Nordin, 2014).

Thus, whereas most policies are targeted at industries or regions in difficulties (Rodrik, 2007), the redistribution of the CAP had no such aim. On the other hand, knowing that agriculture is constantly in a process of structural change, the redistribution of the subsidy

resembles the case of a targeted subsidy. For this reason our paper contributes key evidence for understanding the mechanism of multipliers. That is, by stimulating the agricultural sector in some regions (and depressing it slightly in other regions), we can estimate the effect of a relative increase in spending on relative employment and unemployment.

Apart from Fishback and Kachanovskaya (2010), our study seems to be the first to estimate open economy relative multipliers for government spending targeted to the agricultural sector. Fishback and Kachanovskaya examine the effects of farm grants to farmers to take land out of production. The authors find an insignificant negative effect of agricultural grants on personal income, but no effect on employment. A potentially important difference in our study is that we do not consider a “windfall” or “manna from heaven” transfer. Instead, a special feature of the decoupling reform in 2005 implied that farmers were getting paid to produce environmental services with the purpose of keeping land in good agricultural and environmental condition (cross-compliance requirement), which is costly in terms of labor (The Swedish Board of Agriculture, 2011). As a consequence, gross agricultural employment increased by as much as 9 percent in regions with much grassland (Nordin, 2014). The focus on environmental services is an advantage of our study, since macroeconomic models of the multiplier typically assume that government spending is separable from private consumption (see e.g. Woodford, 2011).

## **2. The Common Agricultural Policy (CAP)**

CAP is a system of agricultural subsidies and support to environmental and rural development. The CAP budget has decreased some since the 2000s, when it represented almost 50% of the EU budget, and in 2010 it represented 31% of the EU budget (€43.8 billion). The direct payments (Pillar I) account for around three-quarters of CAP, and the environment and rural programmes (Pillar II) account for one-quarter. In 2009 the Swedish board of agriculture (Jordbruksverket) paid out SEK 10.2 billion in programmes and direct payments to farmers.

The direct payment scheme was dramatically changed with the full decoupling reform in 2003, and implemented in Sweden in 2005. Decoupling is to prevent overproduction, overuse of pesticides and fertilisers, and global market disturbances. The direct payments to the farmers have not been coupled with the production of crops since 2004, but farmers are responsible for keeping the land in good condition. Notably, because the decoupled payments are based on historical production, the payments are higher in more productive regions. An

effect of the decoupling is that land prices and land rent prices have increased in Sweden (Brady et al. 2008).

As mentioned above, the reform also implied that Sweden introduced a grassland support since the reform covered all farmland.<sup>1</sup> In fact, the Swedish government memoranda (Ds, 2004) proposed a payment scheme without redistribution of the direct payments between regions in Sweden. However, in comparison to the proposed payment scheme the granted petition (Skr, 2003) included a last minute change to the reform. The petition was concerned that farms in some regions may lose more than 20% of their support, and the granted payments to these regions were therefore higher than in the memorandum. Thus, even if the intention was to reduce a potential redistribution of the payments, it may have caused the observed redistribution.

The redistribution of the direct payments is illustrated in Figure 1. The figure shows the percentage change in the direct payments between 2004 and 2005 for regions with different shares of grassland (ratio between hectares of grassland and hectares of total farmland). For regions with more than 50 percent grassland the payments increased in 2005, and for regions with less than 50 percent grassland the payments decreased. Because the regions with less than 50 percent grassland received a much larger share of the direct payments, a relatively small decrease in the payments for these regions implies a large increase in the payments for regions with more than 50 percent grassland. The relatively small decrease in payments for regions with small shares of grassland is because the support per hectare is generally much higher in these regions.<sup>2</sup>

### **Figure 1 about here**

The CAP subsidies are generally supposed to decrease agricultural employment. A decoupled subsidy is a non-labour market improvement of farm income, which has a negative income effect on farm labour (Ahearn et al., 2006). Also, when invested in labour-shedding technology, the CAP has decreased employment rather than protecting jobs in agriculture (Petrick and Zier, 20011). However, as discussed above, the special feature of the decoupling reform in 2005 had the consequence of increasing labor-intensive production of agricultural

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<sup>1</sup> Grassland on arable and semi-natural pastures was made eligible for support. The most important change was to make grassland on arable land eligible, because 35 percent of all arable land in Sweden is grassland on arable land (about a million hectares). Semi-natural pastures covers about 0.5 million hectares. In this paper, we define grassland as only grassland on arable land, because it is the eligibility of grassland on arable land that causes the redistribution of the direct payments.

<sup>2</sup> The arable support is almost 40% higher in the most productive regions in Sweden compared to the average arable support in the country.



and environmental services in order to fulfill the cross-compliance requirements. These requirements are in place to keep land in good agricultural and environmental condition; they relate to soil protection, preventing deterioration of habitats, and protection of water resources. As such, the aim of this paper is to examine the effects of these subsidies on employment outside the agricultural sector.

### **3. Data**

For the period 2001 to 2009 we have information on CAP subsidies on the municipality level. By excluding 29 of Sweden's 290 municipalities, we end up with a sample of 2,349 observations. 24 of the excluded municipalities are located in the metropolitan areas of Sweden (large-Stockholm, Gothenburg or Malmö) where the agricultural sector is very small and employs less than 0.5 percent of the labour force, and 5 are either very small or have been created during the time period.

Data on the subsidies has been received from the Swedish Board of Agriculture. For Pillar I there is information on the payments before (acre and animal subsidies) and after the decoupling reform (single farm payments and some animal subsidies). For Pillar II we have information on every single type of subsidy.

The employment data at municipality level has been collected from Statistics Sweden and we have information about the employment level and sectoral employment. Based on register data (RAMS), Statistics Sweden reports the number of people employed in each sector of every municipality. An employed individual is one who has worked an average of at least one hour per week in November. The classification is based on the sectoral classification of the firm (the firm's SNI2007 code), and the individuals are classified according to their main employment.

The unemployment measure is from The National Labour Market Board in Sweden, and based on the average number of unemployed individuals, aged 16-64, registered as "job-seeker" (at the National Labour Market Board) on a certain date each month during the year. Figure 2 illustrates the mean unemployment level and the mean (logarithmic) private, agricultural and public employment in the municipalities. We find that unemployment and private and public employment are fairly stable up until 2006. The decrease in the unemployment level and the increase in private and public employment in 2007 are due to a boom in the economy. In 2009 the great recession hit the labour market in Sweden, which is evident from the large increase in the unemployment level and the decrease in private and public employment. Agricultural employment seems to decrease up until 2004, and after that

agricultural employment fluctuates around a mean logarithmic employment level of 5.25. For more descriptive statistics, see Table A1, which lists the variables used in this study.

#### 4. Empirical specification and covariates

To estimate the relative multiplier effect of agricultural subsidies on employment, a fixed effect model (FE) is the standard. That is, with a full set of time and area-dummies, the within-municipality variation in direct payments (DP) is used to identify the subsidy effect on employment. Without municipality fixed effects, we mainly pick up the correlation between the size of the subsidies and the employment level, i.e. the employment level is lower in rural regions where the agricultural sector and the CAP subsidies are larger.

While we would like to have a measure of the change in DP that is due to the eligibility of grassland, we only have data on total DP at the municipality level. To overcome this problem, we use the share of grassland in a municipality to predict the actual grassland support. By doing so, we can use a fixed effect IV-model to identify the marginal effect of the grassland support on employment.<sup>3</sup> Technically, as DP is not a binary variable, IV estimates a local average partial effect (see Wooldridge; 2002).

Usually a policy change is not exogenously assigned, but by exploiting an instrument that causally determines the policy change, but is unrelated to the outcome, causal inference is achieved. In non-experimental policy evaluation, IV estimation is therefore often the standard (see for example Angrist et al. 1996, Heckman et al. 1999, for surveys). As with most studies on fiscal multipliers, the issues of endogeneity and reversed causality are problematic when it comes to agricultural subsidies. For example, if regional economic conditions affect the probability of applying for subsidies, an estimated association between agricultural subsidies and the employment level may run from employment to subsidy payments. However, in our particular policy evaluation we argue that it is the change in DP in 2005 that is exogenously distributed, and the regional differences in grassland shares (the instrument) are purely, but effectively, used for identifying the change. The second-stage equation is thus:

$$Employment_{it} = \alpha_i + \delta_t + \gamma_i + \beta Predicted Ln DP_{it} + \rho X_{it} + \varepsilon_{it} \quad (1)$$

In this model,  $i$  and  $t$  are indices for municipality and year, respectively. The time fixed effects,  $\delta$ , capture the common time trend, and  $\alpha$  represents the municipality fixed effects.  $DP$  is the logarithm of the direct payments (in 2008 prices) and the DP effect is estimated on: the

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<sup>3</sup> This is not a conventional use of the IV approach, but it resembles the case when using IV to correct for measurement errors in the independent variable. In fact, if we assume that DP is used as a proxy for the grassland support, we use IV to remove measurement errors that comes from other variations in the payments.

unemployment rate, the logarithm of the number employed in i) agriculture ii) the private sector (excluding agriculture), and ii) the public sector. Because the subsidy effect is not a multiplier effect when estimated on agricultural employment, we refer to it as a DP effect hereafter. In the main specification we estimate the average DP effect between 2005 and 2009, and in section 5.5 we take a closer look at the dynamics of the DP effects.

Since it is the relation between total farmland and grassland that predicts the regional redistribution of DP (i.e. redistribution depends mainly on the introductions of grassland support, but the decrease in the arable support matters too), the share of grassland is a much stronger instrument than hectares of grassland. With a relative instrument, the analysis has to be performed in relative terms, and therefore we use logarithms of the DP and relative employment measures.<sup>4</sup> The vector  $X$  contains covariates and Pillar II subsidies. The farmer has to apply for Pillar II subsidies, and if the employment level and the decision to apply for Pillar II subsidies are related to certain regional farm characteristics, the Pillar II subsidy effect is likely to be biased.

The instrument, grassland share, takes the value zero before 2005; thereafter it equals the average grassland share in the municipality over the entire time period (2000 to 2009).<sup>5</sup> In section 5.2 we see that the instrument represents the redistribution of DP, caused by the introduction of the grassland support, well. Thus, the first stage equation is:

$$DP_{ij} = \alpha_i + \gamma_t + \delta \text{Grassland share}_{ij} + \rho X_{it} + \varepsilon_{it} \quad (2)$$

where  $X$  refers to all of the covariates used in equation 1, and  $\alpha_i$  and  $\gamma_t$  are again the municipality and time fixed effects. Once the first-stage results are obtained, the predicted value of DP will replace the observed DP in the second stage, namely, equation 1.

With the municipality fixed effect we capture the time invariant municipality characteristics. To capture municipality characteristics that vary, we add a set of demographic and socioeconomic variables. The demographical covariates are the *Share of men*, *Share with foreign background* (including both first and second generation immigrants), *Migration* (number of migrants in and out of the municipality divided by the population size), *Logarithmic population density* (inhabitants per km<sup>2</sup>), *Logarithmic population size* and the demographic age structure.

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<sup>4</sup> We could have followed Wilson (2012) and Nakamura and Steinsson (forthcoming) and used per capital measures instead. However, in our study the first-stage is better modeled with logarithms, although the estimated DP effect is almost the same regardless of the choice of transformation.

<sup>5</sup> We have also tried using the grassland share in 2005 or the yearly grassland share, but it does not change the results. Still, assuming random measurement errors in the reported hectares of grassland, the average grassland share should be the best measure. Moreover, taking account of the regional differences in the payments (with a set of regional grassland share variables) does not improve the model.

Socioeconomic variation is captured with the income and education level. Two variables measure the education level of the population; the *Share with higher education* and the *Share with high school education*. We include the *Logarithm of income*, which is a measure of the mean per capita income (gross-income for individuals aged 20 or older) in the municipality deflated with the Consumer Price Index. The income level captures economic fluctuations as well. To capture regional policy changes, the *Logarithm of expenditures on education*, the *Logarithm of expenditures on social aid* and the *Logarithm of expenditures on culture and leisure activities* are included. Data on the expenditures is per capita and deflated with the Consumer Price Index.

Starting in 2004, one-person businesses with a negative income from business were classified as active. If the owner received most of his income from the business he was therefore included in the employment measure. Because these firms were not included before 2004, there is an increase in agricultural employment from 2004 onwards. Besides the agricultural sector (with many one-person businesses), employment in other sectors was only marginally affected by the change (SCB, 2005). If the increase in the employment measure varies between municipalities, the results for the agricultural sector may be biased. For agricultural employment we therefore use a trend-adjusted variable, where we assume that the change in employment between 2003 and 2004 follows the trend (calculated as the average yearly regional employment change between 2000 and 2003). An alternative way of removing this statistical artefact is to add another set of municipality fixed effects, but a set with zeros for the pre-2004 observations. With these post-2004 fixed effects, the measurement change in the employment statistics is captured, and a potential bias is thus removed. Both methods give basically the same results, a decrease in the DP effect on agricultural employment. We prefer the trend-adjustment, because adding another 261 fixed effects increases the standard errors considerably. Moreover, when adding the post-2004 fixed effects to the models with private or public employment, the DP effect increases rather than decreases, which definitely shows that the measurement change is not a problem for the other employment variables (for unemployment the measurement change has no implication what so ever).

## **5. Results**

We begin by presenting the main results of the study, and then we explore the strength of the instrument. After that we do some additional sensitivity tests.

### *5.1 The DP effect on unemployment and private, agricultural and public employment*

Table 2 shows the second stage IV-estimates for the predicted DP on unemployment, private employment, agricultural employment and public employment. To receive comparable estimates for private, public and agricultural employment, the estimates have been recalculated to show also the absolute DP effect (calculated at the median employment level for each measure) on the number of jobs created.<sup>6</sup>

Column (1) of Table 2 shows a point-estimate of -0.74 for DP on unemployment, which implies that a 0.1 log point increase in the DP decreases the unemployment rate by 0.074. The DP effect on private employment and agricultural employment is 0.035 and 0.354, respectively (shown in columns (2) and (3)). For public employment the effect is negative, -0.022.<sup>7</sup>

When re-calculating the relative point-estimates into absolute job creating effects, a 0.1 log point increase in the DP increases private employment by 13.8 workers and agricultural employment by 7.6 workers, and decreases public employment by 5.0 workers. Thus, the impact of DP is larger for private employment than agricultural employment.

Because the effect on private employment (and agricultural employment) and public employment goes in different directions, it indicates that these types of employments are substitutes, which means that private employment supplants public employment. A similar negative relationship between government employment and private employment has been documented in other studies (Ramey, 2012, Wynne, 1992, Finn, 1998; Cavallo, 2005; Pappa, 2009; Gomes, 2010) and, theoretically, the negative relationship builds on the labour resource constraint (Ramey, 2012):

$$N^{Priv} = T - N^{Gov} - L$$

where  $N^{Priv}$  and  $N^{Gov}$  are private and public employment, respectively.  $T$  is the time endowment and  $L$  is leisure. However, the impact on private employment varies with the type of government spending; that is, government purchases increase private employment whereas government employment decreases private employment (Ramey, 2012). In section 5.5 we analyze this issue further.

## 5.2 Is the instrument strong?

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<sup>6</sup> That is, DP effect in absolute terms = (ln Median employment level + Relative subsidy effect) - Exp(ln Median employment level).

<sup>7</sup> We have also tried using a variable measuring the employment share. The employment level is from the database STATIV, based on the total number of people, in the age group 20 to 64, who have been employed during the year, and divided by the total population in the age group. This variable provides us with the same result as the other measures.

Besides satisfying the exclusion restriction, which is to be discussed in the next section, the instrument has to be relevant and strong. Column (5) in Table 2 shows the results of the first-stage regression. Because it is mainly the hectares of grassland<sup>8</sup> (in relation to the hectares of arable land) that determines the change in DP, the instrument is certainly relevant. Increasing the grassland share by 10 percent increases the DP by 6 percent after 2005. A weak instrument that gives biased estimates and underestimated standard errors (Murray, 2006; Stock et al. 2002) is therefore not a problem. The weak IV-test statistic is 168.9, which indicates a very strong instrument (a rule of thumb is that the test-statistic should be above 10).

### *5.3 Including linear time trends and testing the endogeneity of the covariates*

The instrument must also be exogenous; that is, not related to the dependent variable after controlling for relevant covariates. In this case, where the instrument is used primarily for predicting the relevant change in DP, we argue that it is the change in DP, per se, that is exogenous.

However, a regional trend in the grassland regions, for example a strong economic improvement or structural change, may bias the DP effect. Such a trend is unlikely, since the particular trend may be explained by our covariates. Nonetheless, to test if a regional time trend is causing our direct DP effect on agriculture, and the second-order effects on unemployment and employment outside agriculture, municipality-specific time trends are added to the model (261 linear time trends). Table 3 shows that the DP effects decrease by about 30 percent, with municipality-specific time trends included. Yet, a problem with this test is that we do not know whether the decrease in the estimates is from an unobserved trend correlated with the grassland support, or whether the decrease is from attenuation in the estimate. Because the grassland support is a permanent stimulant of the regional economy, with separate multiplier effects for each yearly payment, the employment effect may increase over time. By including linear time trends, part of this increase may be removed, and in that case including time trends may bias the DP effect downward. We therefore estimate the DP effect dynamically in the next section. Nevertheless, the overall conclusion is that the DP effect remains large and significant and that the lion's share of the effect is not caused by a regional time trend.

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<sup>8</sup> Since the size of the grassland support varies over the country, the region has some consequences for the redistribution of the DP as well. But taking the regional variation in the size of the grassland support into account, when modeling the first stage (by adding separate instrument for each region), does not increase the strength of the instrument.

Another concern is that some of the added covariates are endogenous. For example, the average income level might also be affected by subsidies, and controlling for the income level simultaneously may reduce the total DP effect. Thus, in Table 4 the results are shown when removing the, potentially, endogenous covariates (we keep only the demographical covariates). The table shows that the DP effects increase for all outcomes except public employment, where the subsidy effect decreases and becomes insignificant. While this indicates that the effect in Table 2 may be underestimated, the relatively small change in the DP effect when removing the economic indicators also indicates that the change in DP is truly exogenous, i.e. it is not driven by changes in the regional economic activity.

#### *5.4 Dynamic DP effects*

Because most studies estimate the impact of temporary stimulus packages on a yearly outcome, our DP effect is both quantitatively and qualitatively different. Qualitatively, it is different because of its permanent nature, i.e. when firms and individuals expect the subsidy increase to be lasting, the impact is likely to be larger. Quantitatively, it is different because our DP effect is a measure of the average multiplier effect of a five year increase in DP, where each yearly payment has a first instantaneous multiplier effect, and up to four additional rounds of effects (that is, the subsidy payment in 2005 also has a second round effect in 2006, a third round effect in 2007 etc.). Thus, in contrast to the impact of a temporal subsidy change, which may fade out with time, a permanent subsidy increase is assumed to have a permanent impact on employment; if there are additional rounds of multiplier effects, the total impact may rise over time until the employment level is settled at its new equilibrium level. On the other hand, if employment in the private sector has no rebound effect on the agricultural sector, the impact of a permanent subsidy on agricultural employment should be constant over time.

Hence, in this section we investigate the dynamics of the DP effects. To do this we include three lags of the predicted change in the subsidy after 2004.<sup>9</sup> We translate the current subsidy effect as the instantaneous multiplier effect and the lags as the additional round of multiplier effects of a one-year subsidy payment. Figure 3 shows the estimated lags of the DP effect and the total DP effect (where we stepwise add the instantaneous and lagged effects). To show comparable estimates, we report the number of created jobs in each sector (of a 0.1

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<sup>9</sup> A slight complication arises when including lagged explanatory variables in the IV-estimation. In STATA's standard IV-routine, the lagged variables are included in the first stage regression, which implies that we lose one year of the sample period for each lag included. To overcome this problem, we manually perform a two-stage least squares regression where the exogenous DP change after 2004 is predicted by  $\delta \times \text{grassland share}$ .

log point increase in DP). The instantaneous DP effect on private employment is basically zero, which is very reasonable since DP, generally, is paid out late in the year. The first and second round multiplier on number of employed in the private sector is 11.7 and 8.8 respectively, which equals an average cost per job estimate of around \$20,500 for these rounds. As the third round multiplier effect is insignificant, it indicates that the DP effect reaches an equilibrium level after three years.

The DP effect on agricultural employment consists of only the instantaneous DP effect, which is expected. The figure also shows that the negative DP effect on public employment is caused by the instantaneous DP effect and the first round multiplier. The decrease in public employment is probably in response to the increase in private and agricultural employment. However, an obvious concern is that the mechanism goes the other way around; that is, as a negative impact of public employment on private employment. In the next section we therefore analyse the relationship between the sectoral employment measures further, and estimate its impact on the DP effect.

#### *5.5 The DP effect on private employment when including employment in other sectors*

To test whether the DP effect on private employment is related to the increase in agricultural employment or the decrease in public employment, we add these measures to the private employment equation.

In column (1) of Table 5, the DP effect on private employment is shown when including public employment, and in column (2) both agricultural employment and public employment are included. By including two measures of agricultural employment (the years before and after 2005), we allow the decoupling reform and its positive impact on agricultural employment to have a specific effect on private employment.

The association between private and public employment is large and significantly negative; when public employment decreases by one percent, private employment increases by almost 0.3 percent. Yet, the DP effect on private employment remains nearly the same when adding public employment, which demonstrates that the DP effect is not related to changes in public employment. Moreover, we find an insignificant relationship between agricultural employment and private employment, and the relationship does not seem to vary either before or after the decoupling reform. The result shows that the DP effect on private employment is not caused by changes in agricultural employment, rather indicating that the DP effect on private employment results from agricultural purchases and production, and in



that respect resembles the relationship between private employment and government spending.

In columns (3) and (4) we conduct a similar exercise for public employment. That is, in column (3) we first add private employment to the public employment equation, and in column (4) agricultural employment is added as well. Column (3) shows that private employment decreases the DP effect on public employment by 60 percent, but, when agricultural employment is included, the DP effect on public employment disappears. From this we can conclude that the negative DP effect on public employment is caused by its positive impact on private and agricultural employment.

#### *5.4 Does the DP effect vary with the unemployment rate?*

The evidence so far is consistent with Keynesian macroeconomic models in which demand shocks have potentially large effects on employment and unemployment. That is, if output and employment are below their potential levels due to insufficient demand, government spending can raise employment and output. On the other hand, high demand cannot raise the employment level forever. When resources are fully employed, even Keynesian models predict government spending to crowd out private economic activity. Hence, theory suggests that the effects of government spending are state dependent, with demand shocks being less likely to crowd out private activity in an economy with slack. Empirical studies on the US and other OECD countries confirm that the fiscal multiplier is larger in recessions (see e.g. Auerbach and Gorodnichenko, 2012a; Auerbach and Gorodnichenko, 2012b).

An interesting issue is whether a similar line of reasoning applies to regions with high unemployment rates. Intuitively, in these regions, there is potential for a large multiplier effect as crowding out is less likely to be a problem. On the other hand, differences in unemployment rates across regions may reflect distinct equilibrium levels of unemployment. In this case, we do not expect the fiscal multiplier to be larger in high unemployment regions. Thus, whether or not the DP effect varies systematically across regions is an interesting empirical question.

The results for regions with high and low unemployment rates are presented in Table 6. The upper panel of Table 6 shows the DP effects for regions with an unemployment rate below the median, and the lower panel shows the DP effects for regions with an unemployment rate above the median. The table reveals that the DP effects on unemployment and private employment are large in high unemployment regions, whereas no impact is found in low unemployment regions. In line with the earlier result, that it is the rise in private

employment that causes the decrease in public employment, a negative DP effect on public employment is found in high unemployment regions only.

As expected, the DP effect on agricultural employment is about the same in low and high unemployment regions, i.e. since this is a direct effect of the cross-compliance conditions, and not a multiplier effect, whether or not output and employment are below their potential levels should not matter for the result.

## **Conclusion**

The goal of this paper is to investigate whether agricultural subsidies have an impact on unemployment and employment outside the agricultural sector. In order to estimate the open economy relative multiplier of agricultural subsidies, we have used aggregate municipality data for Sweden and an exogenous redistribution of the subsidy in 2005. We conclude that the regional redistribution of the subsidy had a large impact on unemployment and private employment outside the agricultural sector. For example, the reform created private jobs at a cost of about \$20,000 per job, which is fairly consistent with results elsewhere in the literature. However, the relatively large effect may have been due to the permanent nature of the subsidy, and that the subsidy was, presumably, redistributed to regions with weak labour markets. By including regional time trends and economic indicators, we show that the multiplier effect is not related to structural changes in the regions receiving increased subsidies.

Our finding is in line with New Keynesian theory, where demand shocks, such as government spending shocks, have significant effects on the labour market. Evidence of a particularly large multiplier in regions with high unemployment provides support to the notion that local stimuli have larger impacts in regions where employment is below its potential level. This is interesting for policy makers aiming to reduce the negative effects of unemployment. There is substantial evidence that long spells of unemployment lead to losses of human capital. If the effects of fiscal stimulus are larger in regions with high unemployment rates, policy makers may want to allocate funds to regions with depressed labour markets.

An overall conclusion from our study is that the distribution of national government spending on the local level may have implications for aggregate employment. By transferring resources from regions with strong labour markets to regions with relatively high unemployment rates, the net effect on aggregate employment may be positive. On the other hand, our results also indicate a negative association between private and public employment.

To disentangle the interplay between private and public employment at the local level would be an interesting topic for further research.

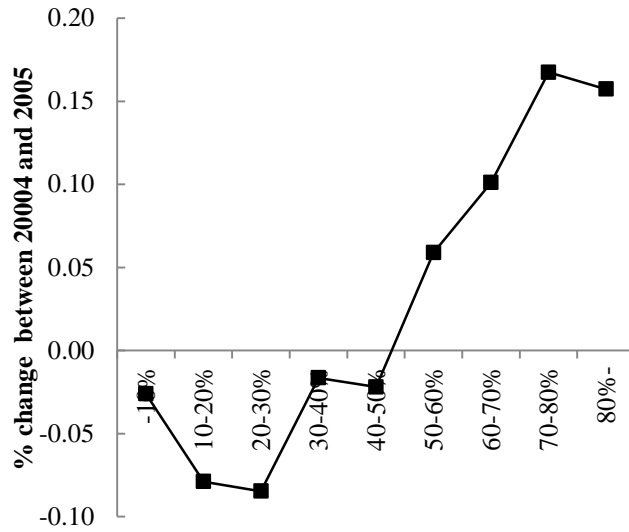
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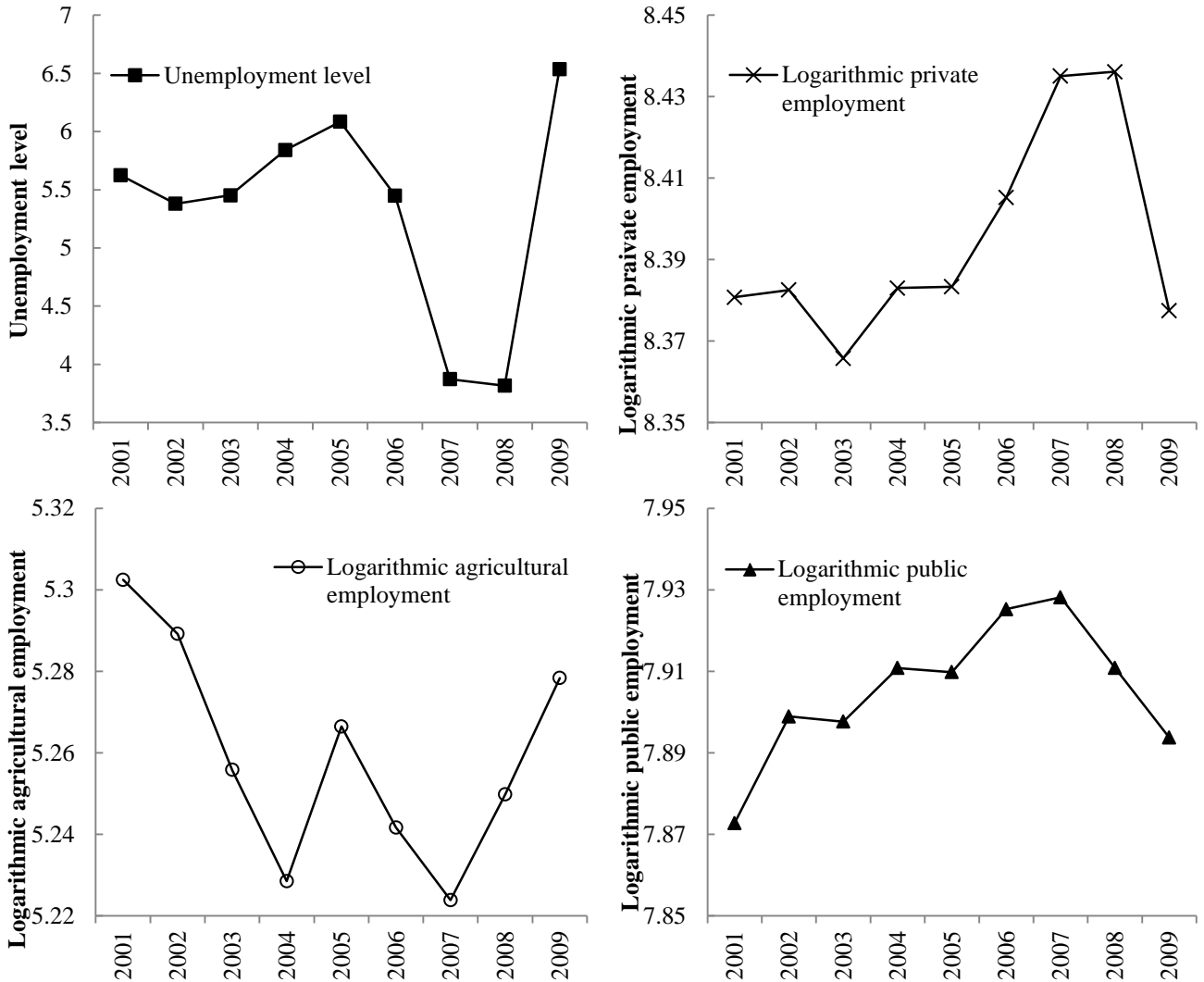
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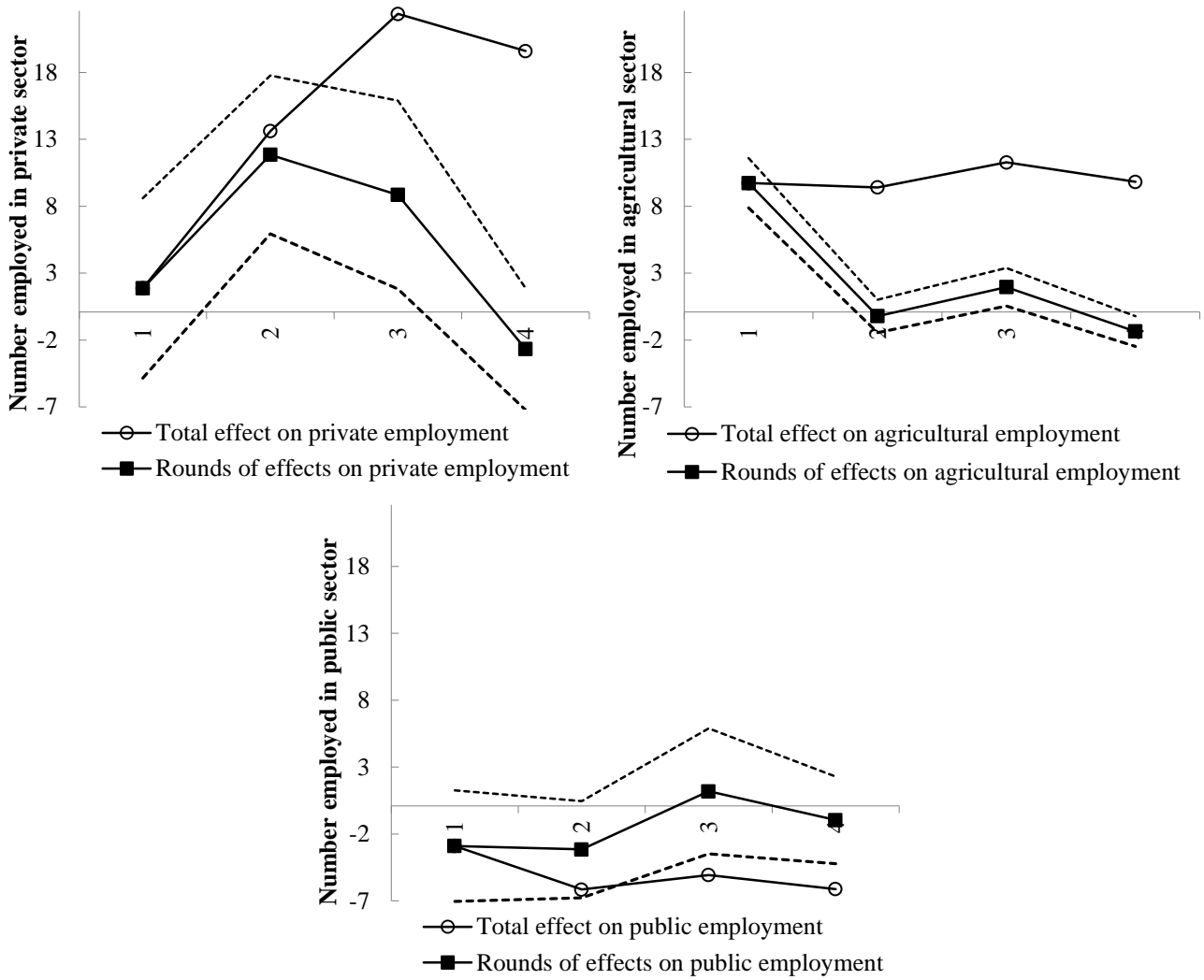
## Tables and figures



**Figure 1.** The percentage change between 2004 and 2005 in the direct payments for regions with different grassland shares.



**Figure 2.** Mean unemployment and sectoral employment in the Swedish municipalities, 2001-2009.



**Figure 3.** The total DP effect and the rounds of DP effects of a one-year subsidy payment on number of jobs created in the private, agricultural and public sectors.

**Table 2.** The first and second stage results when estimating the relationship between direct payments (DP) and the unemployment rate, and the sectoral employment rates, when using grassland share as the instrument.

	Second-stage results:				First stage:
	Unemployment rate	Ln number employed in private sector	Ln number employed in agriculture	Ln number employed in public sector	Ln DP
Ln DP	-0.742** (0.317)	0.0348*** (0.0123)	0.354*** (0.0591)	-0.0219* (0.0123)	
DP effect on number employed	11.87	13.78	7.63	-4.99	
Grassland share					0.610*** (0.0469)
Ln Pillar II	-0.0862 (0.0939)	-0.00267 (0.00323)	0.0405* (0.0210)	-0.00292 (0.00350)	-0.0239 (0.0181)
Share with higher education	-0.103 (0.105)	-0.000138 (0.00422)	-0.00139 (0.0175)	0.00574 (0.00371)	0.0114 (0.0203)
Share with high school education	-0.0263 (0.0742)	0.000787 (0.00292)	-0.0364*** (0.0136)	0.00372 (0.00270)	0.0375*** (0.0132)
Ln Population size	9.958*** (2.311)	0.697*** (0.0962)	0.868** (0.412)	1.107*** (0.0889)	-1.182*** (0.364)
Ln Income	-17.94*** (2.541)	1.157*** (0.0885)	-0.123 (0.493)	0.152 (0.0983)	1.529** (0.649)
Ln, of expenditures on education	1.805*** (0.587)	-0.0679*** (0.0221)	0.0661 (0.116)	0.0388 (0.0247)	-0.236 (0.155)
Ln, of expenditures on social aid	0.128 (0.153)	-0.00543 (0.00556)	0.00101 (0.0263)	0.00397 (0.00537)	0.0596** (0.0261)
Ln, of expenditures on culture and leisure	-0.199 (0.232)	-0.00519 (0.00799)	-0.0523 (0.0363)	0.0173** (0.00832)	0.00932 (0.0429)
Population shares	yes	yes	yes	yes	yes
In and outflow	0.00309 (0.00344)	-0.000176 (0.000125)	-0.000420 (0.000487)	-3.14e-05 (0.000117)	-0.000109 (0.000476)
Ln, population density	-0.0134 (0.0131)	0.000725 (0.000476)	-0.0109*** (0.00355)	-7.22e-05 (0.000473)	0.0112*** (0.00306)
Share of men	-0.192 (0.149)	0.00722 (0.00543)	0.00795 (0.0223)	-0.00894* (0.00537)	0.0252 (0.0259)
Share with a foreign background	-0.0462 (0.0590)	-0.00236 (0.00272)	0.0138 (0.0109)	-0.000646 (0.00208)	-0.00721 (0.0120)
Observations	2,349	2,349	2,349	2,349	2,349
Municipalities	261	261	261	261	261
R-squared	0.713	0.727	0.161	0.639	0.695
Weak IV-test (Kleibergen-Paap F statistic)					168.870

Notes: The second-stage dependent variables are, at the municipality level, the unemployment rate (percent) and the logarithm of the number employed in the private, agricultural and public sectors, respectively. In the first stage the dependent variable is the logarithm of the direct payments, and the instrument is the share of grassland at the municipality level. Year- and municipality fixed effects are added in every specification. Robust clustered standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



**Table 3.** Estimating the direct payments (DP) effect with linear municipality-specific time trends included.

	Second-stage results:			
	Unemployment rate	Ln number employed in private sector	Ln number employed in agriculture	Ln number employed in public sector
Ln Direct Payments	-0.599** (0.289)	0.0214*** (0.00782)	0.176*** (0.0374)	-0.0191* (0.00995)
Observations	2,349	2,349	2,349	2,349
Municipalities	261	261	261	261
R-squared	0.857	0.897	0.742	0.833
Weak IV-test (Kleibergen-Paap F statistic)	108.802			

Notes: The second-stage dependent variables are, at the municipality level, the unemployment rate (percent) and the logarithm of the number employed in the private, agricultural and public sectors, respectively. In the first stage the dependent variable is the logarithm of the direct payments, and the instrument is the share of grassland at the municipality level. Year- and municipality fixed effects, the covariates in Table 2, and the municipality-specific time trends are added in every specification. Robust clustered standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 4.** Estimating the direct payments (DP) effect with endogenous covariates removed.

	Second-stage results:			
	Unemployment rate	Ln number employed in private sector	Ln number employed in agriculture	Ln number employed in public sector
Ln Direct Payments	-0.960*** (0.294)	0.0495*** (0.0130)	0.425*** (0.0489)	-0.0174 (0.0114)
Observations	2,349	2,349	2,349	2,349
Municipalities	261	261	261	261
R-squared	0.688	0.652	0.334	0.630
Weak IV-test (Kleibergen-Paap F statistic)	173.436			

Notes: The second-stage dependent variables are, at the municipality level, the unemployment rate (percent) and the logarithm of the number employed in the private, agricultural and public sectors, respectively. In the first stage the dependent variable is the logarithm of the direct payments, and the instrument is the share of grassland at the municipality level. Year- and municipality fixed effects, and the demographical covariates in Table 2 are added in every specification. Robust clustered standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 5.** Estimating the direct payment (DP) effect on employment when including employment in other sectors.

	Second-stage results:			
	Ln number employed in private sector	Ln number employed in private sector	Ln number employed in public sector	Ln number employed in public sector
Ln Direct Payments	0.0269** (0.0115)	0.0287** (0.0122)	-0.00989 (0.0122)	-0.000615 (0.0141)
Ln Number employed in agriculture (-2004)		0.000555 (0.0100)		
Ln Number employed in agriculture (2005-)		0.00278 (0.0102)		-0.0202** (0.00957)
Ln Number employed in public sector (2001-2009)	-0.360*** (0.0487)	-0.359*** (0.0493)		-0.0204** (0.00975)
Ln Number employed in private sector (2001-2009)			-0.346*** (0.0533)	-0.350*** (0.0530)
Observations	2,349	2,349	2,349	2,349
Municipalities	261	261	0.684	0.688
R-squared	0.761	0.762	261	261
Weak IV-test (Kleibergen-Paap F statistic)	168.796	120.155	177.573	120.071

Notes: The second-stage dependent variables are, at the municipality level, the unemployment rate (percent) and the logarithm of the number employed in the private, agricultural and public sectors, respectively. In the first stage the dependent variable is the logarithm of the direct payments, and the instrument is the share of grassland at the municipality level. Year- and municipality fixed effects and the covariates in Table 2 are added in every specification. Robust clustered standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table 6.** Estimating the direct payments (DP) for municipalities with a low or high unemployment rate.

	Unemployment rate below the median			
	Unemployment rate	Ln number employed in private sector	Ln number employed in agriculture	Ln number employed in public sector
Ln Direct Payments	0.113 (0.459)	-0.0142 (0.0164)	0.531*** (0.0865)	-0.00114 (0.0220)
Observations	1,170	1,170	1,170	1,170
R-squared	0.742	0.812	0.206	0.705
Municipalities	130	130	130	130
Weak IV-test (Kleibergen-Paap F statistic)	95.518			
	Unemployment rate over the median			
	Unemployment rate	Ln number employed in private sector	Ln number employed in agriculture	Ln number employed in public sector
Ln Direct Payments	-1.288*** (0.340)	0.0603*** (0.0128)	0.413*** (0.0590)	-0.0399*** (0.00984)
Observations	1,179	1,179	1,179	1,179
R-squared	0.762	0.733	0.451	0.612
Municipalities	131	131	131	131
Weak IV-test (Kleibergen-Paap F statistic)	84.282			

Notes: The second-stage dependent variables are, at the municipality level, the unemployment rate (percent) and the logarithm of the number employed in the private, agricultural and public sectors, respectively. In the first stage the dependent variable is the logarithm of the direct payments, and the instrument is the share of grassland at the municipality level. Year- and municipality fixed effects, and the demographical covariates in Table 2 are added in every specification. Robust clustered standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A1. Descriptive statistics.**

<i>Dependent Variables</i>	Mean	Std. Dev.	Between Std. Dev.	Within Std. Dev.	Source
Total Unemployment (percent)	5.34	2.02	1.70	1.09	National Labour Market Board
Private employment (number)	6752.61	7511.82	7514.67	386.66	Statistics Sweden
Agricultural employment (number)	248.74	189.10	188.22	21.26	Statistics Sweden
Public employment (number)	4282.78	5176.57	5180.83	217.40	Statistics Sweden
<i>Independent Variables</i>					
Direct payments (millions)	24.30	28.20	28.10	3.56	Swedish Board of Agriculture
Pillar II (millions)	14.40	16.20	15.90	2.90	Swedish Board of Agriculture
Share with higher education (percent)	10.15	3.84	3.69	1.09	Statistics Sweden
Share with high school education (percent)	19.04	2.11	1.42	1.56	Statistics Sweden
Ln Income	5.22	0.09	0.08	0.05	Statistics Sweden
Ln, of expenditures on education	9.48	0.10	0.09	0.05	Statistics Sweden
Ln, of expenditures on social aid	7.60	0.33	0.29	0.16	Statistics Sweden
Ln, of expenditures on culture and leisure	7.52	0.26	0.25	0.09	Statistics Sweden
Population size	24902.86	27482.21	27518.28	772.20	Statistics Sweden
In and outflow	93.16	19.90	18.49	7.43	Statistics Sweden
Population density	43.03	70.09	70.16	2.44	Statistics Sweden
Share of men (percent)	50.21	0.73	0.71	0.17	Statistics Sweden
Share with a foreign background (percent)	10.71	6.00	5.92	1.04	Statistics Sweden
Share 00-14 (percent)	17.04	1.86	1.61	0.94	Statistics Sweden
Share 15-19 (percent)	6.95	0.64	0.42	0.49	Statistics Sweden
Share 20-24 (percent)	4.98	1.09	1.03	0.36	Statistics Sweden
Share 25-34 (percent)	10.10	1.69	1.59	0.59	Statistics Sweden
Share 35-44 (percent)	13.23	1.18	1.13	0.34	Statistics Sweden
Share 45-54 (percent)	13.43	0.75	0.61	0.44	Statistics Sweden
Share 55-64 (percent)	14.14	1.28	1.06	0.72	Statistics Sweden
Share 65-74 (percent)	10.01	1.53	1.39	0.64	Statistics Sweden
Share 75-84 (percent)	7.29	1.43	1.41	0.26	Statistics Sweden
Share 85- (percent)	2.66	0.62	0.59	0.19	Statistics Sweden