Government Transfers and Growth: Is there Evidence of Genuine Effect?

Sefa Awaworyi\(^1\) and Siew Ling Yew\(^2\)

**Abstract**
This paper investigates how government transfers affect economic growth. Using meta-analysis techniques, we systematically review 24 primary studies with 164 estimates that examine the effect of government transfers on economic growth. After addressing heterogeneity and issues of publication bias in the existing literature, we find a negative association between government transfers and growth. This negative growth impact of government transfers also holds for developed countries. Meta-regression results also reveal that the effect size of reported estimates largely depends on individual study characteristics. In particular, data time period, measure of government transfers, econometric specification and underlying theoretical models are important factors that explain the variations in the empirical results.

**Keywords:** Transfers; Welfare policy; Social security; Taxes; Economic growth

**JEL Classification:** I38; H53; H55; O47; E62

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1. Introduction

Government (or public or social) transfers, either in cash or in kind, are transfers of income or services from one group to another in a society, either from the rich to the poor, from the young and active to the old or from the healthy to the sick, among others. Thus, government transfers, as a form of social protection, are often used as a policy tool to promote social and economic development. While the importance of government transfers as social protection for society is widely accepted, there is less agreement concerning the importance of government transfers for promoting economic growth. In addition, given the demographic transition is occurring at a very rapid pace in many countries – not only in the rich countries- and where the financial of social programs has become a major challenge, understanding the growth impact of government transfers is an important one. This paper investigates how government transfers affect economic growth using meta-analysis techniques. We compare and combine results from various empirical studies that investigate the effect of government transfers on growth to identify sources of diversity among those results and to derive a more precise effect size estimate than that derived in a single study.

Since there are various mechanisms through which government transfers can affect growth, the theoretical viewpoints on the growth impact of government transfers are controversial. Examples of those mechanisms are private savings, human capital, fertility, longevity, and inequality, among others. In general, how government transfers affect growth will depend on how government transfers affect those mechanisms which have important implications on the efficiency, equity and thus, growth of an economy (see e.g., Feldstein, 1974; Barro, 1974; Kotlikoff and Summers, 1981; Modigliani, 1988; Zhang, 1995; Ehrlich and Kim, 2005; Alesina and Rodrik, 1994; Persson and Tabellini, 1994; Aghion and Bolton, 1997; Keane and Prasad, 2002).

The empirical results of how government transfers affect growth are also mixed. Studies such as Barro (1989), Barro (1991), Perotti (1996), Bellettini and Ceroni (2000), Zhang and Zhang (2004), and Lee and Chang (2006), among others, find evidence that government transfers enhance growth, while others find otherwise (see e.g., Ehrlich and Zhong, 1998; Hansson and Henrekson, 1994; Ehrlich and Kim, 2005). Studies such as Landau (1986) find an insignificant association between government transfers and growth. Given the heterogeneity in the empirical results, it is therefore difficult to draw a general conclusion on the effect of government transfers on growth.

Since both the theoretical arguments and empirical evidence are inconclusive in the literature, an unambiguous answer to how government transfers affect growth is still absent. Based on the empirical evidence presented in the existing studies, this paper sheds new light on this relationship using meta-analysis techniques. We do so by formulating two hypotheses – (H1) The impact of government transfers on economic growth is negative, and (H2) The impact of government transfers on economic growth is positive in developed countries.

This study addresses two important issues and makes the following contributions. First, we examine if ‘genuine’ effect exists between government transfers and economic growth. Without having corrections for publication bias, an empirical literature which appears to present a given empirical effect could be misleading (Stanley, 2008) and therefore could impede policy formulation. We apply

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2 Transfers in cash include social pensions, unemployment insurance, sickness, maternity, family allowance, and workplace injury benefits. Transfers in kind consist of either the transfer of particular goods other than cash, or the provision of a service. Examples of transfers in kind are food subsidy and public housing.
meta-analysis tools to deal with issues of publication bias, and consequently provide a statistically valid conclusion on the effects of government transfers on growth. Second, we examine the sources of heterogeneity in the government transfers-growth literature. Given the disparity in the government transfers-growth literature, a general conclusion cannot be drawn on the growth effect of government transfers without addressing issues on heterogeneity. With the use of meta-regression analysis, we account for study-to-study variations and thus, determine a net effect of government transfers on growth. In the government transfers-growth literature, sources of heterogeneity include underlying theoretical models, econometric specification and data characteristics, among others.

To the best of our knowledge, this paper is the first to provide a detailed empirical synthesis of the government transfers-growth literature using a meta-analysis. An extensive literature review by Atkinson (1995) presents a good overview of pioneering evidence on the government transfers-growth relationship but the growth effect of government transfers (hereafter GTRAN) is overall inconclusive. In addition, with a surge in the number of studies since Atkinson (1995), it is worthwhile to re-examine this relationship using more thorough techniques.

The rest of the paper is organized as follow. Section 2 presents an overview of the theoretical and empirical literature. Section 3 provides a description of the data, the empirical methodology and results. Section 4 presents a discussion of results, and concludes with some suggestions for future research.

2. A Brief Overview of the Literature

This section reviews previous studies that examine the GTRAN-growth relationship. We focus on four key mechanisms that are relevant for this relationship: (1) The transfers-savings-growth mechanism, (2) The transfers-fertility-human capital-growth mechanism, (3) The transfers-inequality-growth mechanism, and (4) The transfers-longevity-growth mechanism.3

2.1. The Transfers-Savings-Growth Mechanism

According to Lewis (1954), a key fact of economic development is rapid capital accumulation. Since Lewis’s famous paper, several economic theories have been proposed to explore the effect of savings on economic growth. Solow’s growth model (1956) shows that a higher saving rate generates higher growth along a transitional path but has no effect on long-run growth. Endogenous growth models such as Romer (1986) and Barro (1990) show that long-run economic growth depends on capital accumulation.

However, the theoretical viewpoints about the impact of GTRAN on savings and thus, growth are divergent (see e.g., Feldstein, 1974; Barro, 1974; Kotlikoff and Summers, 1981; Modigliani, 1988; Zhang, 1995; Ehrlich and Kim, 2005). For instance, Feldstein (1974) argues that unfunded social security reduces savings and impedes capital accumulation and growth in a life-cycle model. Using an infinitely-lived agent model, Barro (1974) argues that unfunded social security should have no

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3 All those mechanisms may be related to each other and thus, government transfers could affect economic growth via all or some of those channels such as savings, fertility, human capital, and longevity together.
effect on capital accumulation and thus, growth when private intergenerational transfers are operative. Using an endogenous growth model, Zhang (1995) shows that unfunded social security is neutral to the saving rate but may promote growth.

The empirical results about how GTRAN affect private savings or/and growth are mixed as well. An empirical survey by Aaron (1982) concludes that there is no empirical evidence that supports the prediction that social security decreases private savings. A series of work by Cigno and Rosati find evidence that social security has a positive effect on savings (Cigno and Rosati 1992, 1996, and 1997). Barro (1989) finds that GTRAN (represented by all social security and social welfare expenditure) have a negative effect on savings but a positive effect on growth, though this growth effect is not statistically significant. Using cross-country panel data, Zhang and Zhang (2004) find evidence that supports the analytical results of Zhang (1995), i.e., social security increases economic growth but it has no statistically significant effect on the saving rate. By developing a model of family formation, Ehrlich and Kim (2005) find that social security reduces private savings and growth using panel data.

2.2. The Transfers-Fertility-Human Capital-Growth Mechanism

GTRAN may also affect growth through other mechanisms such as fertility and human capital accumulation.

GTRAN may affect growth by changing fertility and human capital investment. One example is through the trade-off between the quantity and quality of children in the spirit of Becker (1960), and Becker and Lewis (1973). According to Becker and Lewis (1973), an increase in the quality of children raises the marginal cost of the quantity of children, and vice versa. Galor and Weil (1999, 2000) and Galor and Moav (2002) stress the importance of the trade-off between the quantity and quality of children in explaining the transition from stagnation to growth.

Zhang (1995), using an endogenous growth model with endogenous fertility, shows that GTRAN via unfunded social security can promote growth through the quantity-quality trade-off of children by favouring the latter if bequests are positive and parents have a sufficiently strong taste for the welfare of children. In Ehrlich and Lui (1998), social security adversely affects at least one of the following- fertility, human capital, and growth- in a model that incorporates social security, the family, and endogenous growth.


2.3. The Transfers-Inequality-Growth Mechanism

Many economists are of the view that inequality is not only an outcome but also plays a central role in determining other aspects of economic development such as economic growth. Therefore, another mechanism through which GTRAN can affect growth is via its redistribution effect on inequality.
Redistributive transfers may hurt growth (see e.g., Okun, 1975) because higher distortionary taxes used to finance redistribution may dampen incentives to work and invest and so, there exists a trade-off between redistributive transfers and growth. On the other hand, redistributive transfers need not be inherently detrimental to growth when GTRAN benefit the poor and help offset capital market imperfections (see e.g., Benabou, 2000). In such cases, redistributive transfers could increase both equality and growth. Aghion and Bolton (1997) analytically show that in the presence of imperfect capital markets, redistribution of wealth from rich lenders to poor borrowers enhances the long-run efficiency of the economy and thus, economic growth.

From a political economy perspective, Keane and Prasad (2002) argue that social transfers play important roles in mitigating inequality, ensuring social stability and enhancing growth. However, through this politico-economic mechanism, it is also possible for redistributive transfers to reduce growth because of their disincentive effects on capital accumulation (see e.g., Alesina and Rodrik, 1994; Persson and Tabellini, 1994). Intuitively, a less equal society, for instance, will lead to more redistribution and higher taxation and thus, will cause less investment and growth.

The empirical results on how redistributive transfers affect growth are also divided. Studies that use taxes as an indicator of redistribution tend to suggest a negative effect of redistribution on growth (see e.g., Tanzi and Zee, 1997). Using social cash transfer as an indicator of redistribution, Keane and Prasad (2002) present cross-country evidence that redistribution reduces inequality and can enhance growth, consistent with the empirical finding of Perotti (1996).

2.4. The Transfers-Longevity-Growth Mechanism

Longevity or life expectancy is another mechanism that can affect growth. Intuitively, when longevity or life expectancy improves, people that expect to live longer will save more for old-age consumption, and will invest more in their own or their children's education, as the return to education rises with life expectancy. Higher saving and human capital therefore lead to faster growth (see e.g., Ehrlich and Lui, 1991; de la Croix and Licandro, 1999; Zhang, Zhang and Lee, 2001; Boucekkine, de la Croix and Licandro, 2002). Empirically, Barro and Sala-i-Martin (1995), Barro (1997), and Zhang and Zhang (2005), among others, find that life expectancy can have a positive effect on economic growth.

The effect of GTRAN on longevity and thus, growth, is often established through social security or public pension of an unfunded type. A mandatory old-age pension as a survivorship-contingent claim induces people to invest more of their resources in improving their longevity to acquire the promised pension and thus, may affect their incentive to save. For instance, using lifecycle models, Davies and Kuhn (1992) and Philipson and Becker (1998) show that social security, a longevity-contingent claim, induces more health spending for greater longevity and more lifecycle savings. In Zhang, Zhang and Leung (2006), public pension and health subsidies may raise longevity but may reduce savings and thus, future output. To the best of our knowledge, however, there is a lack of empirical investigation on the GTRAN-longevity-growth interaction.

Given the lack of consensus among theoretical arguments and empirical results concerning the effects of GTRAN on growth, as discussed above, further empirical work on this issue is apparently needed. The next section provides a further investigation to re-examine the effect of GTRAN on growth by conducting a meta-analysis.
3. Data and Methodology

Our study selection and estimates reporting process draw on guidelines proposed by the meta-analysis of economics research network (MAER-NET) which promotes transparency and best practices in meta-analyses (Stanley et al., 2013). Using various keywords for GTRAN and growth, we search for five electronic databases – JSTOR, EconLit, Business Source Complete, Google Scholar and ProQuest for relevant literature. We also conduct a manual search where we examine the references of identified studies to ensure that other relevant studies are included in our meta-analysis.

The included studies in our meta-analysis are based on a three-stage inclusion criterion. First, for a study to be included in this meta-analysis, it has to be an empirical study that examines the effects of GTRAN on economic growth. Second, to allow for comparability, we focus only on studies that capture expenditure as a share of GDP. Thus, GTRAN must be an independent variable and should be a share of GDP. Third, we focus on studies that use growth rate of GDP as dependent variable. In addition, given that partial correlation coefficients (PCCs) are calculated for each reported estimate, studies that satisfy all three criteria but do not report all relevant statistics to allow for PCC calculation are excluded (e.g., Weede, 1991; Zhang and Zhang, 2004).

Based on the above mentioned inclusion criteria, we select 24 primary studies with a total of 164 estimates for our meta-analysis. Table 1 presents a list of studies included in this meta-analysis along with the number of effect size estimates reported by each study and their corresponding simple and fixed effect weighted averages.

3.1. Partial Correlation Coefficients (PCCs)

We first calculate partial correlation coefficients (PCCs). PCCs are independent of the metrics used in measuring both dependent and independent variables and thus, they allow for compatibility of effect size estimates and studies. PCCs measure the association between GTRAN and GDP growth with other independent variables held constant. However, when primary studies do not control for all relevant covariates, effect sizes presented by primary studies may be biased and this may affect calculated PCCs. Even so, this does not render PCCs irrelevant as we later control for model characteristics such as omitted variables in our meta-regressions. This enables us to examine if models specified by primary studies have any systematic effects on reported effect sizes.

The PCC for each estimate reported in the primary studies and its associated standard errors are often calculated using equations (1) and (2), respectively (Greene, 2011; Alptekin and Levine, 2012; Ugur, 2013):

\[ r_i = \frac{t_i}{\sqrt{t_i^2 + df_i}} \] (1)

and


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\[
se_{ri} = \sqrt{(1 - r_i^2)/df_i}
\]  

(2)

where \(r_i\) and \(se_{ri}\) are PCC for each effect size estimate and its associated standard error, respectively. \(t_i\) and \(df_i\) represent the t-statistics and degrees of freedom associated with the estimates reported in primary studies, respectively.

### 3.2. Fixed Effect Weighted Means (FEEs)

We provide fixed effect weighted means (FEEs hereafter) for effect size estimates reported in each primary study. FEEs are more reliable than simple means, and compared to random effect weighted averages, they are less troubled by publication bias (Henmi and Copas, 2010; Stanley, 2008; Stanley and Doucouliagos, 2014).

The standard error \(se_{ri}\) in equation (2) represents the variation due to sampling error, and the inverse of its square is used here as weights to calculate the FEEs. Thus, FEEs take into account within-study variations given that lower weights are assigned to less precise estimates and vice versa. We calculate FEEs using equation (3):

\[
\bar{X}_{FEE} = \sum r_i \left( \frac{1}{se_{ri}^2} \right) / \sum \frac{1}{se_{ri}^2}
\]

(3)

where \(\bar{X}_{FEE}\) is the weighted average, and all other variables remain as explained before.

Table 1 presents FEEs per study for the association between GTRAN and growth. Of the 164 estimates drawn from 24 primary studies that examine the GTRAN-growth relationship, we find that 12 studies with a total of 58 estimates (35.36% of total reported estimates) present statistically insignificant weighted averages. Of the remaining 106 estimates that are statistically significant, 6 studies with a total of 55 estimates (33.54% of total reported estimates) are negative. Lastly, 51 estimates (31.10% of total reported estimates), drawn from 6 primary studies, are positive and statistically significant.

[INSERT TABLE 1 HERE]

The overall FEE for all 164 estimates is given as -0.0034. According to the guidelines proposed by Doucouliagos (2011), the overall effect size here represents a ‘small’ empirical effect.\(^5\) Thus, based on the calculated overall FEE, our results suggest that there is a weak negative association between GTRAN and growth. Furthermore, this weighted average is valid only if publication selection bias does not pose a problem. In the presence of publication selection bias and/or within-study dependence, FEEs cannot be considered as reliable measures of the association between GTRAN and economic growth (De Dominicis et al., 2008; Ugur, 2013; Alptekin and Levine, 2012). Therefore, we

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\(^5\) Doucouliagos (2011) argues that the guidelines presented by Cohen (1988) understate the economic significance of empirical effect when applied to PCCs. Thus, Doucouliagos (2011) proposes that PCCs larger than the absolute value of 0.33 can be considered ‘large’ while those above (below) 0.07 can be considered ‘medium’ (‘small’).
further examine if the reported estimates are tainted with publication selection bias and if they represent ‘genuine’ measures of GTRAN’s effect on economic growth in the next section.

3.3. Investigating Publication Selection Bias

First, we present a funnel plot as it helps visually inspect publication selection bias. Funnel plots are scatter plots of effect size estimates against their precision \(1/\text{se}_{ij}^2\). Funnel plots help detect the extent to which statistically significant results are treated more favourably by authors, reviewers and editors to justify model selection (Stanley, 2008). Publication bias is unlikely if a funnel plot is symmetric with an inverted funnel shape. Figure 1 is a funnel plot of effect size estimates against their precision \(1/\text{se}_{ij}^2\). A visual inspection of this plot suggests that publication bias may be an issue given that the funnel plot does show slight signs of asymmetry with reference to our baseline. However, a visual inspection alone does not guarantee that the GTRAN-growth relationship literature is free of, or tainted with publication bias. In addition, funnel plots cannot help us determine the direction and magnitude of bias, if there is any.

[INSERT FIGURE 1 HERE]

To investigate publication bias more thoroughly, we adopt the precision effect test (PET) and funnel asymmetry test (FAT) which involve the estimation of a bivariate model (Egger et al., 1997; Stanley, 2008) and are widely used in the meta-analysis literature (see e.g., Abreu et al., 2005; Efendic et al., 2011; Alptekin and Levine, 2012; Ugur, 2013).

We employ equation (4) for the PET-FAT analysis:

\[
t_{ij} = a_0 + \beta_0 \left(1/\text{se}_{ij}\right) + \varepsilon_{ij}
\]

where \(t_{ij}\) and \(\text{se}_{ij}\), respectively, are the t-value of the estimated effect size from estimate \(i\) reported by primary study \(j\), and the associated standard error, specified in equation (2). Stanley (2008) demonstrates that equation (4), based on the t-statistics, can be used to test for both publication selection bias (FAT) and for genuine effect beyond publication bias (PET).

The constant, \(a_0\), and the slope coefficient, \(\beta_0\), are tested to examine if they are statistically different from zero (i.e., PET tests if \(\beta_0 = 0\), and FAT tests if \(a_0 = 0\)). There is evidence of publication bias if, at conventional levels, \(a_0\) is statistically different from zero, in which case \(a_0\) gives the direction of bias. ‘Genuine’ effect exists if, at conventional levels, \(\beta_0\) is statistically different from zero. Furthermore, Stanley and Doucouliagos (2007) indicate that if PET-FAT results suggest that both publication bias and genuine effect exist, then there is a nonlinear relationship between reported effect-size and its standard error. In such instances, they propose that a corrected estimate of \(\beta_0\) can be obtained by using the precision effect estimation with standard errors (PEESE). The PEESE involves the estimation of equation (5) with suppressed constant:

\[
t_{ij} = a_0 \text{se}_{ij} + \beta_0 \left(1/\text{se}_{ij}\right) + v_{ij}
\]
We estimate models (4) and (5) using weighted least squares (WLS), cluster data analysis (CDA), and multi-level model (MLM). CDA corrects for the downward bias in standard errors (Wooldridge, 2002), and relaxes the assumption of data independence between reported effect sizes in a given primary study. Our preferred estimation method is the MLM given that it accounts for both within and between study variations. WLS and CDA are presented for the purpose of comparison only.

Table 2 reports the results from the PET-FAT analysis (Panel A) and PEESE analysis (Panel B). From Table 2 Panel A, the results of the PET-FAT from all estimation methods suggest that there is a negative association between GTRAN and economic growth with evidence of publication bias. Specifically, the result of MLM (Panel A, column 3) suggests that the coefficient of precision is -0.0781, with evidence of bias. This represents a ‘medium’ empirical effect based on the guidelines by Doucouliagos (2011).

Given that there is evidence of publication bias, we also report PEESE results. PEESE results (Panel B) are in line with the findings in Panel A (i.e., a negative association between GTRAN and growth) for all columns except column 3 which is statistically insignificant at conventional levels. Thus, beyond publication selection bias, there is still evidence of a negative effect of GTRAN on economic growth but this relationship is weaker, and is not robust to the control of both within and between study variations.

3.4. Meta-regression Analysis (MRA)

PET-FAT-PEESE estimations are relevant for making inferences about the existence or absence of publication selection bias and genuine effect. However, they assume that moderating variables that may be related to study characteristics are equal to their sample means and thus, they do not address issues of systematic heterogeneity. To address issues of systematic heterogeneity, we conduct a multivariate meta-regression analysis (MRA) to examine if the GTRAN-growth relationship is robust to the inclusion of moderating variables, and if differences in primary studies affect reported effect size estimates.

Issues of data dependency are likely to emerge given that primary studies often report more than one effect size estimate (De Dominicis et al., 2008). Thus, to account for issues of data dependency and the multi-level structure of our dataset, we estimate equation (6) which makes one of the error terms a study-level term:

\[ t_{ji} = \alpha_0 + \beta_0 \left(1/se_{jri}\right) + \sum \beta_k(\bar{Z}_{ki}/se_{jri}) + e_j + u_{ji} \]  (6)

where \( t_{ji} \) is the \( i \)th t-statistic from the \( j \)th primary study, and \( se_{jri} \) is the associated standard error. \( \bar{Z}_{ki} \) is a vector of moderating variables that are likely to account for variations in the GTRAN-growth evidence base, and \( k \) represents the number of moderating variables. \( e_j \) is the study specific error term, whereas \( u_{ji} \) is the independent and identically distributed error term.

We estimate MRA model (6) with WLS, CDA and MLM regressions. We report results for two estimation types – general and general-to-specific. The more general specification accounts for a
wider range of moderating variables. However, the inclusion of a large number of moderating variables can lead to issues of multicollinearity and over-determination. Thus, we also report results for a general-to-specific model where, one at a time, we drop variables with the most insignificant values (i.e., highest p-values) until included variables are statistically significant.\(^6\)

The MRA results are presented in Table 3. The odd columns (1, 3 and 5) show results from the general specification, while the even columns (2, 4, and 6) show the more specific results. Our inferences, however, are mainly drawn for the general specification, unless otherwise indicated.

The moderating variables included in the MRA are chosen based on a broad range of empirical, theoretical and methodological differences, as well as other dimensions and characteristics of primary studies that are likely to affect the GTRAN-growth relationship. Thus, we introduce three broad categories of moderating variables which are likely to influence effect size estimates. The first category captures differences in theoretical models and econometric specifications used by primary studies. The second category relates to the characteristics of data used in primary studies. The third category captures publication characteristics. Considering the reported R-squared in Table 3 for the general specification, these moderating variables account for about 59.42% of the variations observed in reported estimates. We present a list of included moderating variables and their descriptions in Appendix Table A1.

3.4.1. Theoretical Models and Econometric Specifications

The existing literature on the GTRAN-growth relationship suggests that primary studies base their analysis on certain theoretical models which help determine what econometric model to specify. Thus, we control for the underlying theoretical models adopted by primary studies. Specifically, we control for studies that use either a neo-classical or endogenous growth model (growth model), while leaving out other studies that do not use the growth model (for instance, simultaneous equation models) as base. Results from the general specifications suggest that the underlying theoretical model used by primary studies does not affect the nature of reported estimates.

We capture two areas of the econometric specifications- the control variables and the period of data averaging -used by primary studies.

First, with regard to the control variables, the existing literature on government expenditure and growth suggests that the set of control variables used in growth regressions can significantly affect the nature of results. According to Levine and Renelt (1992), variables such as human capital, investment share of GDP and population are major growth determinants. It has also been argued that generous transfers may reduce growth through the distortionary taxes required to fund this expenditure (de Groot, 2000; Nijkamp and Poot, 2004). Thus, in our MRA, we control for studies that include the above mentioned variables to examine if the inclusion of these control variables in primary studies affect the nature of reported estimates. We find that the inclusions of taxes and investment in primary studies as control variables do not present any significant variations in the GTRAN relationship. However, quite robustly, we find that studies that control for human capital in

their regressions tend to report positively on the effects of GTRAN on growth. A negative relationship is reported for studies that control for population but this is not robust across estimation types.

Second, with regard to the period of data averaging, different lengths of time periods over which data is averaged to estimate growth equations are a major source of heterogeneity in the existing literature. Various arguments have been presented to support the choice of data averaging period (see e.g., Kneller et al., 1999). The common practice is the use of the 5-year averaging. Thus, we introduce a dummy variable to capture studies that use 5-year averaging in order to examine if the period of data averaging affects the GTRAN-growth relationship. We use other averaging periods as base. Results suggest that the period of data averaging does not significantly alter the nature of GTRAN’s effect on growth.

3.4.2. Data Characteristics

We first examine if the GTRAN-growth relationship is time variant. We include dummy variables D60, D80 and D90 to capture studies that include data from the 1960s, 1980s and 1990s, respectively. We use other years as the base. Overall, we find that the effect of GTRAN on growth is time variant given that the dummies for data period are mostly statistically significant. Results suggest that the inclusion of data from the 1960s and 1980s is associated with more negative effect, whereas the 1990s is associated with a more positive effect of GTRAN on growth.

The GTRAN-growth relationship may be affected by country-type. This is because developed and less developed countries (LDCs) differ in many aspects including the nature of institutions, the level of economic development, the features of economic transformation, and financial sustainability. For instance, LDCs are unable to finance social transfers through payroll taxes as developed nations have done and thus, must find other means of obtaining revenue. Different means of financing GTRAN may give different growth impact. We therefore control for studies that use data on developed countries in our MRA. We find that the developed-countries dummy is significant and negative across all columns. This suggests that studies that use data on developed countries tend to report more negative effects of GTRAN on growth.

The type of data used (time series, cross-section or panel) is another potential factor that can affect reported estimates. Lee and Chang (2006) argue that the use of time series data may yield inconsistent and unreliable results given that most of these datasets have very short time spans, and are estimated using OLS. It has also been argued that cross-country data series are characterized by considerable heterogeneity which can lead to misleading inferences when ignored (Lee and Chang, 2006; Sarantis and Stewart, 2001). To examine the effects of data type on the GTRAN-growth relationship, we include a dummy for studies that use a single country (time series data) and those that use cross-section data, and treat panel data as the base in our MRA. We find that the data type used by primary studies does not present any significant variations in reported effect size estimates in the GTRAN-growth literature. Furthermore, the inclusion of the years of data used by primary studies reveals that an increase in the number of years of data is associated with more positive effects of GTRAN on growth.

Additionally, primary studies present various measures of government transfers. For instance, some studies examine only the effect of social security expenditure on growth, while a few others examine
the effects of subsidies on growth. There are also a few studies that capture the effects of overall GTRAN on growth, in which case social security, unemployment benefits, subsidies, and all other transfer related expenditures are captured in a single measure. To examine if the differences in GTRAN measures used have an effect on the GTRAN-growth relationship, we control for studies that report estimates using welfare benefits.\textsuperscript{7} We find that the coefficient of welfare benefits (social security and unemployment transfers) is statistically insignificant.

3.4.3. Publication Characteristics

With regard to publication characteristics, we consider two dimensions –journal rank and publication year.\textsuperscript{8}

To examine if journal rank presents any variations in the nature of reported estimates, we control for high-ranked journals to examine if this category of journals as opposed to low-ranked journals presents different estimates.\textsuperscript{9} We find that high-ranked journals are predisposed to report more negatively on the GTRAN-growth relationship.

Next, we control for publication year. As newer studies emerge with newer datasets and economic techniques, to challenge the status quo, it is worthwhile to examine if the nature of reported estimates in publications changes overtime. Mainly, three categories of publication year are observed for studies included in this meta-analysis (i.e., those published in the 1980s, 1990s and those in the 2000s). We control for studies published in the 2000s, while using the 1980s and 1990s as base.\textsuperscript{10} We find that the nature of reported estimates changes as newer publications emerges. Specifically, results suggest that relatively newer studies (those published in the 2000s) report stronger negative effects of GTRAN on growth.

We now turn to the net effect of GTRAN on growth after controlling for heterogeneity. The net effect is determined by the coefficient of the precision ($1/se_{ij}$) given in Table 3. Across all columns (estimation types), our results suggest that there is a negative effect of GTRAN on growth. Specifically, drawing on results from our preferred MLM model (columns 5 and 6), the net effect of GTRAN on growth is -0.6320 and -0.4053, for the general and the specific models, respectively. These effects represent large empirical effects based on Doucouliagos (2011).

4. Discussion and Conclusion

The results of our meta-analysis reveal that primary studies tend to report negative effects of GTRAN on growth. PET/FAT results indicate that GTRAN has negative effects on growth, but with evidence of publication bias. The negative effect is consistent with findings from the fixed effect weighted averages. Overall, our results support H1, i.e., a negative association between GTRAN and growth.

\textsuperscript{7} We define welfare benefits as the measure of government transfers that include social security expenditure and unemployment transfers only. We use other measures of government transfers as the base.

\textsuperscript{8} Given that most studies included in this meta-analysis are published in journals, we do not examine if journal papers produce systematically different results compared to other publication types such as working papers.

\textsuperscript{9} The Australian Business Dean’s Council (ABDC) and the Australian Research Council (ARC) present classifications for journal quality. Journals are ranked in descending order of quality as A*, A, B and C. Thus, we introduce a dummy for A* and A ranked journals (high-ranked) in our MRA, and use other ranks as base.

\textsuperscript{10} About 33.33% of studies included in this meta-analysis (8 studies) are published in the 2000s. We capture this category of studies to represent new studies.
Results from our MRA suggest that moderating variables consisting of study and data characteristics as well as underlying theoretical and econometric specifications have an influence on the GTRAN-growth effect. We find that negative effects are more pronounced than positive effects for developed countries. This suggests that H2, the impact of GTRAN on growth is positive in developed countries, is not supported.

The negative growth effect of GTRAN in developed countries suggests that the costs of GTRAN outweigh the benefits of GTRAN in promoting growth. This negative impact of GTRAN on growth may be due to the following reasons.

First, high GTRAN spending in developed countries may be an important factor. According to Prasad (2008), OECD high income countries spend 12.7% of GDP on GTRAN over the period 2000-2004. By contrast, developing countries such as sub-Saharan Africa spend only 1% of GDP on GTRAN over the same time period. It has been argued in the literature that distortionary taxes tend to distort saving decisions and lower growth when taxes are sufficiently large (see e.g., Barro, 1990). Thus, through the transfers-savings-growth mechanism, GTRAN tend to retard growth when sufficiently high GTRAN financed by distortionary taxes reduce savings.

Second, this negative growth effect of GTRAN in developed countries may also be explained by the transfers-longevity-growth mechanism. Since developed countries are ageing faster than LDCs, the elderly share of the population rises quickly in those countries. The increased GTRAN such as social security to the elderly that live longer now have caused increasing concerns about the growth impact of these transfers. Many view GTRAN such as social security as the major unproductive expenditure in the growth process (see e.g., Devarajan et al., 1996). As a consequence, increased transfers from the young and active to the elderly through social security may crowd out productive activities such as R&D, and investment in physical and human capital, and may adversely affect the composition of government expenditure and growth. The costs from this crowding out effect may exceed the benefits of social security in promoting longevity and incentives to save.

Third, through the transfers-human capital-growth mechanism, higher GTRAN may reduce growth if GTRAN results in a net discouraging effect on human capital accumulation. For instance, a higher social security tax rate reduces the after-tax wage and thereby reduces the return to human capital investment in education. With lower human capital accumulation, growth falls as a result. It may also be possible that GTRAN financed by payroll taxes discourage work effort and reduce the return to capital investment. Moreover, if a higher inequality motivates more GTRAN through the political process, GTRAN may reduce growth by lowering savings and capital accumulation.

The control variables used in primary studies also affect the nature of reported estimates. Specifically, positive effects are more pronounced for studies that control for human capital, while more negative effects are observed for those that control for population. These results suggest that the growth impact of GTRAN may change when human capital and population are omitted in primary study regressions. Since human capital and average growth rate of population are key

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11 Devarajan et al. (1996) show that the change in the composition of government spending affects a country’s economic growth.
growth determinants, the exclusion of such variables from the GTRAN-growth model specification may lead to biased results.

Some suggestions emerge for future research. First, a few studies are excluded because relevant statistics needed for inclusion in this meta-analysis are not reported. It is in the best interest of future research that standards be set to guide authors in the reporting of relevant statistics such as standard errors and/or t-values. Second, given that we exclude studies that use other measures of GTRAN expenditure other than as a percentage of GDP, a course of future research would be to examine the general effect of such studies as it could offer further insights. Third, we observe that very few studies include recent datasets (2000s) in their analysis. Given our findings which suggest that data periods affect the GTRAN-growth relationship, it would be worthwhile for future research to consider more time periods in their analysis. Lastly, owing to data constraints, we were not able to conduct separate meta-regressions for country samples - developing countries and LDCs. Preferably, H2 can be tested more thoroughly by conducting separate meta-regressions for a sample of developing countries only. Given an increase in the number of primary studies in the future, future research can examine more thoroughly the effects of GTRAN on growth considering country development levels.

References


### Table 1 - Overview of Evidence Base per Study (Simple & Fixed Effect Weighted Means)

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### Table 2 - PET/FAT and PEESE Results

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| Observations | 164 | 164 | 164 |

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Standard errors in parentheses
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Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1
Variables | Definition
--- | ---
Developed | Takes value 1 if the primary study data is from developed countries, otherwise 0
Single Country Study | Takes value 1 if a single time series analysis of a country is done, otherwise 0
Cross-section Data | Takes value 1 if cross-section data is used by primary study, otherwise 0
Growth Model | Takes value 1 if the model is based on economic growth model, otherwise 0
lnyears | Natural log of the number of years of data used in primary studies
5-yr Average | Takes value 1 if the data used in the primary uses 5-year data averaging, otherwise 0
Welfare Benefit | Takes value 1 if the primary study used Welfare Benefit, otherwise 0
D60 | Takes value 1 if the primary study includes data from 1960s, otherwise 0
D80 | Takes value 1 if the primary study includes data from 1980s, otherwise 0
D90 | Takes value 1 if the primary study includes data from 1990s, otherwise 0
Investment | Takes value 1 if the primary study control for investment, otherwise 0
Human Capital | Takes value 1 if the primary study control for human capital, otherwise 0
Population | Takes value 1 if the primary study control for population, otherwise 0
Tax | Takes value 1 if the primary study control for taxes, otherwise 0
Journal Rank | Takes value 1 if the primary study is published in high-ranked journal, otherwise 0
Pub Year (2000s) | Takes value 1 if the primary study was published in the 2000s, otherwise 0

All variables are divided by $SE_{rt}$