

Dell'Anno, Roberto; Schneider, Friedrich G.

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## Estimating the underground economy by using MIMIC models: A response to T. Breusch's critique

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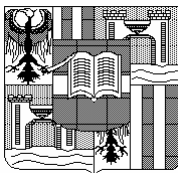
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**Estimating the Underground Economy by Using MIMIC  
Models:**

**A Response to T. Breusch's critique**

by

**DELL'ANNO, Roberto and SCHNEIDER, Friedrich<sup>\*)</sup>**

Working Paper No. 0607  
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**Johannes Kepler University of Linz  
Department of Economics  
Altenberger Strasse 69  
A-4040 Linz - Auhof, Austria  
[www.econ.jku.at](http://www.econ.jku.at)**

friedrich.schneider@jku.at  
phone +43 (0)70 2468 -8210, -8209 (fax)

***Estimating the Underground Economy by using MIMIC Models: A Response to T.  
Breusch's critique by***

Dr. Roberto Dell'Anno  
Department of Economics and Statistics  
University of Salerno  
Via Ponte Don Melillo 1, 84084  
Fisciano (SA) / ITALY  
[rdellanno@unisa.it](mailto:rdellanno@unisa.it)

Prof. Dr. DDr.h.c. Friedrich Schneider  
Department of Economics  
Johannes Kepler University Linz  
A-4040 Linz-Auhof / AUSTRIA  
Phone: 0043-732-2468-8210, Fax: -8209  
[friedrich.schneider@jku.at](mailto:friedrich.schneider@jku.at)

**Abstract**

*Breusch (2005b) critically addresses an important and challenging question: Is it reliable to use the Multiple Indicators Multiple Causes model to estimate Shadow Economy? Breusch concludes that this approach is not suitable for the purpose. Breusch's paper highlights the different procedures and hypotheses about the estimation of the size and development of the shadow economy in three papers: Giles and Tedds (2002a), Dell'Anno and Schneider (2003) and Bajada and Schneider (2005). In this paper, we will react and provide some answers to Breusch's criticisms with special reference to the criticisms of the Dell'Anno and Schneider (2003) paper. According with us, the MIMIC model is still one of the best approaches to this purpose.*

**Keywords:** Shadow economy, MIMIC model, Structural modelling.

**1. Introduction**

In a recent paper (Breusch, 2005b)<sup>1</sup> Trevor Breusch critically addresses an important and challenging question: Is it reliable to use the **Multiple Indicators Multiple Causes** model to estimate Underground Economy? Breusch concludes that this approach is not suitable for the purpose. Breusch's paper highlights the different procedures and hypotheses about the estimation of the size and development of the shadow economy in three papers: Giles and Tedds (2002a), Dell'Anno and Schneider (2003)[D-S, 2003] and Bajada and Schneider (2005). Considering the specific results of these papers, he puts forward several critical questions on the general methodology of the estimation method and about the specific ways how the estimations were undertaken in these three papers. In the following, we will react and provide some answers to Breusch's criticisms with special reference to the criticisms of the D-S (2003) paper.

We would like begin with an overall statement about the MIMIC method in order to strengthen our efforts to support MIMIC method. According to Popper's thought the growth of human knowledge gains from

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<sup>1</sup> It is available for download from a public site: <http://econwpa.wustl.edu/eps/em/papers/0507/0507003.pdf>

problems and from attempts to solve them. These attempts require the formulation of theories which must go beyond existing knowledge and therefore require creativity and imagination. In the shadow economy research, where the step of the estimation is particularly challenging, researchers are forced to use some kind of “imagination” because existing estimation procedures are not convincing and complications are still numerous and available.

It is widely accepted by scholars estimating the shadow economy (SE) is a “minefield”. We want to reply to Breusch’s and other criticisms and we follow the statement by Giles and Tedds “*Whatever methodology one adopts in a situation of this kind, assumptions must be made and data issues must be resolved, and many of these assumptions and issues are open to debate*” (Giles and Tedds, 2002b, p. 1662). In general the estimations of a SE are vulnerable and quiet often there empirical results of the SE are sensible and no one can’t really claim to be confident of the full reliability of his/her estimates. Usually economists like the accurateness and exhaustiveness of their estimates and their efforts are addressed to find reasonable assumptions to get reasonable figures. All the specialists in this field of estimating the shadow economy know quite well, estimating SE is an exercise that does not allow to apply well-defined methods and common accepted assumptions. That said it is quite understandable why there are so much replications, and criticisms of the various econometric methods to estimate SE<sup>2</sup>.

We do not argue that replications of empirical results have not scientific value, at the contrary, they represent a great opportunity for the academic community to share knowledge and open a debate about methodologies and assumptions. Criticisms are welcome because such a dialectic process is the essence of scientific progress to have an incentive to develop their own understanding.

The goal of this paper is a rejoinder to the critique in the paper by Breusch (2005b) in order to estimate the SE and provide constructive suggestions to improve MIMIC method.

In the following chapters we follow the structure of the paper by Breusch (2005b) in order to illustrate our point of views on his criticism of our paper (Dell’Anno and Schneider 2003).

## **2. General Critique of the MIMIC Approach:**

Breusch (2005b) states:

*“The MIMIC model can be interpreted and estimated as a standard econometric model of linear simultaneous equations” (p. 6).*

In our opinion, this statement although true is a) is not original b) does not contribute to reduce the researchers implementation troubles, c) is not opportune, according with the methodological guidelines that should be followed by the scientific research.

**Ad a)** This statement is well-known to MIMIC model users. In the famous paper by Joreskog and Goldberger (1975), the relationship between the simultaneous equations systems and the MIMIC was already shown “*In term of the multivariate regression of the indicators on the causes, the [MIMIC] model implies*

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<sup>2</sup> Just to quote the authors, who criticise MIMIC method: Helberger and Knepel (1988) replicated Frey and Weck-Hannemann’s paper, Breusch (2002), Hill (2002), Breusch (2005a) replicate to Giles and Tedds (2002a).

restrictions of two types: (i) the regression coefficient matrix has rank one; (ii) the residual variance-covariance matrix satisfies a factor analysis model with one common factor” (abstract, p. 631). And later “[...] the first sort of restriction is of the type which arises in the simultaneous-equation models of econometrics [...] The second sort of restriction is of the type which arises in the factor analysis models of psychometrics, where the incorrelations of observable variables are assumed to be accounted for by common factors or unobservable latent variables” (p. 632).

Again, an interesting and comprehensive analysis at this topic provided already Goldberger (1972). In this paper (par. 6 - Unobserved variables in economics) there is a quite detailed comparison which tries to answer similarities and differences between *Structural Equation Models* and *Simultaneous Equation Models*. As after more than thirty years Goldberger’s arguments seem still a very valid answer, we refer to his own word when he states that: “...there are numerous incentives for econometricians to break through those fences which still separate the social sciences”(p. 999).

**Ad b)** As already argued in a) the MIMIC model is not a "standard" linear simultaneous equations model but a model with composite error terms and/or errors-in-variables terms. With this characteristic and accepting some additional efforts to interpret and calculate coefficients and parameters, the size of the SE can certainly be estimated with the help of econometric standard packages. In our opinion, to treat a MIMIC model as simultaneous equation model and estimating it with standard econometrics package has not particular advantages. As Joreskog and Goldberger (1975) already state that the MIMIC procedure is designed by choosing two different strategies (econometric and psychometric), therefore we don’t find any reasonable motivation to force it into an econometrics framework.

**Ad c)** It is universally accepted that a pattern that includes as special case other models is scientifically preferable. In line with this argument we believe that one can consider the MIMIC framework, as special case of Structural Equation Model instead of a particular specification of linear simultaneous equations system. We have two kinds of motivations: the first, refers to the advantages derived by a open-mindedness and multidisciplinary approach desirable for the economic research, the second concerns specifically the MIMIC model:

(1) For the first reason, we think that there isn’t a value-added for the scientists to favour a specific estimation method over another one just because one is more familiar with software and its conventions. At the contrary, it is a sense of duty for a researcher, who is broad-minded, to accept (critically) contributions from other fields. Therefore for a social scientist it is not suitable to consider the multiple regression analysis as a special case of MIMIC model or Structural Equation Model (by constraining errors, factor loadings, etc.) just because it can be estimated with a specialist LISREL or AMOS software, also for an economist it is useful to spend much more time of his research to learn about other tools. In economics the econometricians should be offer assistance to adapt methods for specific features of estimation problems instead to propose some manipulations in order to work with more familiar techniques.

(2) With respect to the usefulness to consider a model with latent variable(s) as a simultaneous equations system, we are convinced that the most appropriate statistical framework is still the Structural Equation

Model or when we have an economic time series case the Dynamic Structural Equation Modelling proposed by Cziraky (2005).

The MIMIC procedure is examined closely in D-S (2003, par. 4), and if the main problem with MIMIC procedure really is that most of the exogenous "causes" of the latent variable (the shadow economy) are actually latent variables, too, than this case requires a SEM or a DSEM model if there are dynamics (which is almost certainly the case), but these things are a natural extension of the MIMIC model rather than a totally different approach or another special case of simultaneous equations system.

### **3. A Defence of the Estimation of the Underground Economy in Dell'Anno and Schneider (2003)**

#### **3.1. Replication of our Estimation Results**

In this section we reply to Breusch's criticisms against the D-S (2003) estimation analysis.

As first, we are confident that Breusch's careful replications of our MIMIC estimation is a good sign, i.e. that he was able to relate our results. Hence it would be interesting to see if there are any differences in estimated coefficients and statistic tests. Unfortunately, Breusch did not provide any to check this.

As he does not prevent any empirical results, he asserts:

*"I rely on my own careful replications of the calculations to determine what is actually done to the data (p. 8)"*

We suppose that the dataset used for replication is that exposed in table 2 (D-S-study (2003), 3, p. 9) and footnote 6, thus we note that to replicate D-S (2003) study Breusch's replication uses a different dataset demonstrated by the following points:

(1) The cause variable "taxes" is not "taxes collected from households and business, and indirect taxes" but "revenue of direct taxes, indirect taxes and social security contributions received by government" This difference is important because Breusch did not consider the Social Security Contributions (SSC) payments. The omission of the SSC payments is very relevant in Italian economy as up to 1992 it is nearly always the most relevant component of the overall tax burden.

(2) Our fifth and sixth causes (...) are missing in Breusch's paper and

(3) He states that the second indicator variable (currency in circulation outside banks, C) is taken from Bank of Italy on-line database. We have doubts that the series TDA00100-S787365M (from 1962 to 1980) is available from on-line database.

#### **3.2 Normalizing the MIMIC model:**

Other argument, which is used by Breusch (2005b) against the reliability of MIMIC model, refers to D-S, 2003 choice to identify the system of equations by normalizing the MIMIC model with a negative coefficient ( $\lambda = -1$ ). Precisely, Breusch states:

*"The "theoretical and empirical arguments" for this decision are not made explicit, but it seems that the objective is to ensure that key coefficients in the structural equation for the latent variable have the desired sign. (p.18)"*

The answer for Breusch's censure is already provided in D-S (2003, p.106) where, it is stated that:  
*"in the literature there is no agreement about the effects of the shadow economy on economic growth. In our analysis, we find some evidence to support the hypothesis of a negative relation between Italian shadow economy and official growth rate of GDP "*.

The choice of the sign of the scale coefficient is a key issue in the estimation strategy. It refers not only to econometric issue of the identification of a system of equations but refers also to the economic assumptions behind the model. As Stapleton (1978) points out: *"while fixing the scale of the unobserved variables arbitrarily limits quantitative interpretations of the coefficients, the choice of scale will not affect qualitative results"*. (p. 60). The impossibility to adopt a widely accepted expected sign for the relationship between official and unofficial economy requires additional economic assumptions to assign a value to the scale coefficient.

We proposed a strategy that can be considered as a constructive approach to reduce the arbitrariness of the researcher's choice. In particular, following Dell'Anno (2003), the sign of  $\lambda_1$  is determined through a "reductio ad absurdum". In the MIMIC model the vector of structural coefficients is in constant relation to the scale coefficient. When the "sign" of the coefficient of scale ( $\lambda_1$ ) is changed from positive to negative, all the structural parameters of the causes change from positive to negative and vice versa (keeping the same absolute values), e.g., if a positive value is assigned to  $\lambda_1$  the relationship between tax burden and SE it becomes than negative. In our view, these results completely diverge from well-known theories and empirical studies that assert a "positive" link between underground economy and these variables. In Italian case the hypothesis about a negative relation between changes of SE and growth rate of GDP was accepted as more theoretically based one.

Our procedure is not very different from the calibration approach. It uses parameters that have been estimated for a similar model into one's own model. According to us, this is one of the ways to propose a more economic-based utilization of Lisrel methodology to economic goals.

#### **4. Benchmarking or calibration**

Breusch (2005b) surveys the three different benchmarking procedures used by Giles and Tedds (2002a), D-S (2003), and Bajada and Schneider (2005). He concludes that:

*"The data are typically transformed in ways that are not only undeclared but have the unfortunate effect of making the results of the study sensitive to the units in which the variables are measured."*(p.28)

In agreement with Breusch, we believe that different benchmark methods lead to different estimates of SE and certainly, it is a drawback when are evaluated MIMIC outcomes.

This limitation depends on complications tackled by researchers to convert the index of Shadow Economy estimated by MIMIC model in cardinal values. It is not an easy task, as the latent variable and its unit measure are not observed. The model provides just a set of estimated coefficients from which one can calculate an index which shows the dynamics of the unobserved factor.

The standardization of any methodology requires experimentation, comparisons and a wide academic debate, although, at this stage of research on the MIMIC approach, it is not clear which benchmark method is the best or most reliable one. In which way to proceed here is still problematically and unexplored hence every suggestion about this aspect of technique is welcome.

Taking Breusch's remarks as a starting point, we present the "state of art" on the benchmark procedures. In the following we report four different benchmarking strategies which have been applied to convert the index of the SE estimated by MIMIC 3-1-2b into absolute values. Further a fifth one is presented in order to take in account exposed limitations.

The base-year is the 1978 because this is the year in which there are several estimates of the Italian SE. This assists us to building an average of the estimates, (D-S, 2003: 19.7%).

We use the following symbols:  $\hat{\eta}_t$  indicates the value of index estimated at time t;  $\hat{\gamma}'_q$  the vector of structural coefficient estimated by MIMIC model and  $x_{qt}$  the value of cause q observed at time t.

(1) D-S (2003) solve the system of  $t-1$  linear equations:

$$\hat{\eta}_t = \eta^* + \hat{\eta}_{t-1} + \hat{\gamma}'_q \Delta x_{qt} \quad [1]$$

by choosing  $\eta^*$  in order to satisfy the following condition:  $\hat{\eta}_{1978}/GDP_{1978} = 0.197$ . The last step is calculate the other  $t-1$  ratios ( $\hat{\eta}_t/GDP_t$ ).

(2) The benchmark strategy applied by Giles and Tedds (2002a) is:

$$\frac{\eta_t}{GDP_t} = 0.197 * \left( \frac{\hat{\eta}_t}{\hat{\eta}_{1978}} \right), \quad [2]$$

where  $\hat{\eta}_{1978}/GDP_{1978} = 0.197$  is the SE estimated by an auxiliary method.

(3) The benchmark strategy applied by Bajada and Schneider (2005) is:

$$g_t^{SE} = g_{1978}^* + \hat{\gamma}'_q \Delta x_t, \quad [3]$$

where  $g_t^*$  is chosen in order to satisfy the condition  $g_{1978}^{SE} = g_{1978}^*$ ,  $g_t^{SE}$  indicates the growth rate of SE and  $g_{1978}^*$  is the growth rate of SE estimated in the benchmark year by an auxiliary method.

(4) An alternative calibration process is exposed in Tedds (2005). According to author, she is able to preserve the proportional relationships found in the original index series. In symbols:

$$\frac{\hat{\eta}_t}{GDP_t} = a_t \frac{\hat{\eta}_1}{GDP_1} \quad [5]$$

and making use the average ratio, she has the additional relationship:

$$\sum_{t=1}^T \left( \frac{\eta_t^*}{GDP_t} \right) / T = \frac{1}{T} \frac{\hat{\eta}_1}{GDP_1} \left( 1 + \sum_{t=2}^T a_t \right), \quad [6]$$

where  $\eta_t^*$  indicates the level of SE estimated by an auxiliary method.



(5) We propose a different benchmark procedure. It requires instead of growth rate of GDP as reference variable ( $Y_1$ ) an alternative indicator:  $(GDP_t / GDP_{1978})$ . According with the identification rule ( $\lambda_1 = -1$ ) the index of the shadow economy as a percentage of GDP in the 1978 is linked to the chain index of real GDP as follow:

$$\text{Measurement equation: } \frac{GDP_t - GDP_{t-1}}{GDP_{1978}} = - \frac{\hat{\eta}_t - \hat{\eta}_{t-1}}{GDP_{1978}}. \quad [7]$$

The estimates of structural model are used to obtain an ordinal time series index for latent variable:

$$\text{Structural Equation: } \frac{\hat{\eta}_t}{GDP_{1978}} = \gamma' x_{qt} \quad [8]$$

The index is scaled to take up to a value of 19.7 percent in 1978 and further transformed to the SE as ratio of current GDP. These operations are show in the following benchmark equation<sup>3</sup>:

$$\frac{\hat{\eta}_t}{GDP_{1978}} \left[ \frac{\eta_{1978}^*}{GDP_{1978}} \frac{GDP_{1978}}{\hat{\eta}_{1978}} \right] \frac{GDP_{1978}}{GDP_t} = \frac{\eta_t}{GDP_t} \quad [9]$$

where:

- I.  $(\hat{\eta}_t / GDP_{1978})$  is the index calculated by eq. [8];
- II.  $(\eta_{1978}^* / GDP_{1978} = 19.7\%)$  is the exogenous estimate of SE;
- III.  $(\hat{\eta}_{1978} / GDP_{1978})$  is the value of index estimated by eq. [8] in 1978;
- IV.  $(GDP_{1978} / GDP_t)$  is able to convert the index of SE as changes respect to base year in SE respect to current GDP;
- V.  $(\eta_t / GDP_t)$  is the estimated SE as a percentage of GDP.

The product of II and III meets with the constraint that estimated SE/GDP in 1978 is equal to 19.7% and the last addend of LHS converts the index of changes respect to the base year into a ratio between SE and current GDP.

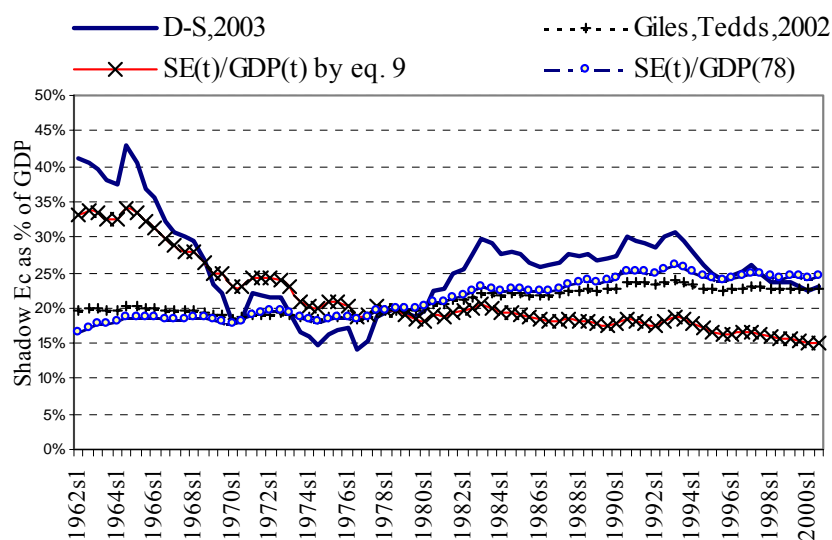
This method shares the proportional properties of Tedds (2005) and Giles, Tedds's (2002a) benchmark strategy and it avoid the Breusch's criticism to assign the monetary unit of measure from the reference indicator to the latent variable.

Figure 1 presents the outcomes of the Italian SE calculated by three different methodologies<sup>4</sup> (D-S, 2003; Giles and Tedds, 2002a; equation 9) with the D-S, 2003 dataset. We show also the outcomes of  $SE/GDP_{1978}$ <sup>5</sup> in order to reveal similarities with other estimates.

<sup>3</sup> As the variables are all differenced to same degree, to calculate the levels of the latent variable multiplying the structural coefficients for raw (unfiltered) data, it is equivalent to compute the changes in the index by multiplying coefficients for the differenced causes and then to integrate them.

<sup>4</sup> In this comparison the calculations of the methods proposed by Tedds (2005) and Bajada and Schneider (2005) are not shown. This reason is, that we have not the exogenous estimates of the average ratio of SE over the sample (Tedds'

**Figure 1:** Alternative Benchmarking procedures – Calculating the Italian Shadow Economy



Source: own calculations

According with our calculations, the size and development of the Italian SE doesn't change strongly over time, except for the sixties, for D-S, (2003), Giles and Tedds (2002a) and the index of SE in term of the GDP of 1978.

It is worthwhile to point out that the different calibration procedures could be motivated not only by the absence of a prevailing dominant strategy but also differences in economic assumptions with reference to the measurement relationship between observed and unobserved economy.

In particular each strategy can be justified by different economic hypotheses:

- (1) According to D-S (2003), a change in the level of the shadow economy is reflected in the growth rate of measured real GDP.
- (2) Giles and Tedds (2002a) assume a more or less fixed relationship between growth rates of formal and informal economy. Using a benchmark they calibrate the index of underground economy as ratio of official GDP proportional to an exogenous estimate of the SE as ratio of GDP. Their strategy preserves the proportional relationships between the two measurements.
- (3) According to Bajada and Schneider (2005) their relationship is again a ratio between growth rates of official and unofficial economy. In this case, benchmarking means to anchor the index estimated by MIMIC to go through an auxiliary estimate of the growth rate of the SE as ratio of GDP in a certain benchmark year.
- (4) According to Tedds' (2005) formula, a change of the SE using the benchmark year should be proportional to a change of the measured GDP with respect to the same benchmark year:

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method) and of growth rate of SE (Bajada and Schneider's method) provided by an auxiliary method. The here-proposed benchmark procedure (n. 5) required to re-estimate MIMIC model with the scaled indicator.

<sup>5</sup> It means to apply equation [9] without the ratio  $(GDP_{1978}/GDP_t)$ .

$(\hat{\eta}_t / \hat{\eta}_1 = a_t GDP_t / GDP_1)$ . This is equivalent to state that the growth rates of official and unofficial economy are equal for each period except for the logarithm of ratio between the coefficients of the two last periods:  $g_t^{SE} = \ln \frac{a_t}{a_{t-1}} + g_t^{GDP}$ .

(5) Finally, according to here proposed benchmark, the changes of official economy respect the GDP measured in the base year is linked to changes in the SE as percentage of official GDP in that year. Its advantages are:

- (i) the index estimated by eq. [8] is measured as changes respect the same period and holds proportional relationship between the measurement in different years.
- (ii) Reference indicator is not considered with a monetary unit of measure, therefore it is directly comparable with the resulting index of latent variable.

Finally, we confirm the choice operated in D-S (2003) in order to make easier the understanding of the relative importance of structural coefficients and their quantitative effects on the SE. It considered causes  $(x_{qt})$  measured as percentage points, because the estimated structural coefficients are measured by variables which have the same scale.

Another criticism of Breusch opposed to D-S, 2003 is the following:

*“The variables are uniformly first differenced for estimation, apparently without prior testing (p.16)”*.

The readers of the Journal of Public Finance and Public Choice where D-S (2003) is published, are much more interested in the implications for economic policy than the statistical procedure and properties of MIMIC method. As cited in our paper, and confirmed by Breusch (2005b, fn. 34, p. 16), a much more detailed statistical analysis of the dataset, multinormality, intertemporal stability of coefficients and also tests of unit roots, has been provided by Dell’Anno (2003)<sup>6</sup>.

## 5. Effects of the Deviations-from-mean

In another criticism Breusch’s (2005b) claims that in the replicated papers it is undocumented that the variables are transformed to deviations from their means.

According to him this is a relevant omission since: *“prediction of the latent variable is biased, because the means of the variables are removed for estimation but included when forming the predictor. (p.22)”*.

However, we argue that in D-S (2003) the index of SE is not biased by using deviations. This becomes clear comparing the index estimated by eq. [1]:  $\hat{\eta}_t - \hat{\eta}_{t-1} = \eta^* + \hat{\gamma}'_q (x_t - x_{t-1})$

with the other index that includes the deviations from means:

$$(\hat{\eta}_t - \bar{\eta}) - (\hat{\eta}_{t-1} - \bar{\eta}) = \eta^* + \hat{\gamma}'_q [(x_t - \bar{x}) - (x_{t-1} - \bar{x})]. \quad [7]$$

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<sup>6</sup> In particular, in that working paper there are several pages devoted to the unit root analysis in Appendix D.

It is obvious that eq [1] and eq. [7] produces the same predictions ( $\hat{\eta}_t$ ).

We give also an economic explanation for this transformation. It means that variations of the shadow economy are determined by changes of causes and not by levels of variables.

Another argument brought forward by Breusch is that the MIMIC applications omit to estimate an intercept. In D-S (2003) model, we did not estimate an intercept because benchmarking procedure consisted to add a constant ( $\eta^*$ ) in order to set the estimated SE in one period equal to an exogenous estimate of SE. Hence, we supposed that was worthless to estimate an intercept because it must be replaced when the index is converted to cardinal values.

Although, Breusch is in the right to argue that every transformation of variables has to be documented, we argue that according our strategy of benchmarking, the predictions are not influenced by the use of deviations-from-mean.

With reference to Breusch's speculation that "*authors are unaware of making such transformations (p.3)*", we reply that this is not the case. In D-S (2003) we do not give a lot of references to statistical features of the MIMIC model, but in Dell'Anno (2003) where the statistical details of analysis are provided, this transformation is documented quietly well: "*...all variables are taken to have expectation zero (p.7)*".

Finally, considering to Breusch's criticism to analyze at the estimation process, too, and not only the predictions, we suggest that models containing means and intercepts could be provided in advance for applications in this field. As Hayduk (1987, p. 321) states, with the inclusion of means: a) the model is consistent with a wider range of evidence than if the mere covariances were used; b) the structural coefficients are more reliable because they must now be consistent with a wider range of evidence. In the Structural Equation Model it is possible to include means and intercepts a further are advisable.

## **6. Differencing and cointegration**

This type of criticisms raised by Breusch (2005b) refers to the usual practice to differencing data in order to take into account the non-stationarity in the observed variables. He states that:

*"The purpose of differencing is not always clear, but I show that the treatment is not an effective solution for any problem that matters and may in fact cause serious problems (p. 3)."*

Concentrating the analysis on D-S (2003) case, Breusch seems to be in agreement with us when he affirms: "*The model in the differences will be consistently and efficiently estimated by maximum likelihood in this case. The latent variable will be stationary because it is a linear combination of stationary variables... (p.25)*". According to Breusch a problem arises when the index is calculated from the model of differences in order to be used as predictor in models with levels.

We argue against the Breusch's criticism that at least in D-S (2003) paper, his argument is not valid, because it comes (probably) from a misunderstanding about the strategy followed in our paper. Breusch (2005b) states that D-S (2003) strategy is "*to integrate the latent variable from the differences model to become the predictor of the levels form of the normalizing indicator. Now the latter variable has a unit root*

*and the integrated latent variable will have a unit root, but there is nothing to connect these two unit roots – the two variables will not be cointegrated (p.25)”.*

Breusch’s misinterpretations are based on the following two points:

- 1) In our paper it isn’t assumed that the level of SE is a predictor of the level of the official GDP. Our reference indicator is the growth rate of GDP, and not the level of observed GDP, therefore the target of our predictor (index in differences - stationary) is the growth rate of official GDP (indicator in log-differences - stationary).
- 2) It isn’t assumed that these two variables are cointegrated<sup>7</sup>.

As already argued the estimation procedure assumes a relationship between changes in SE and growth rate of official GDP. This relationship between two stationary variables is satisfactory for our aim and, in our view, the absence of cointegration it is not relevant.

One could argue further stage, to convert the index to levels, it is just a mathematical transformation that not requires any hypothesis of cointegration between variables in level which respect to our results, we know, that the two variables in the form of level (observed and unobserved economy) are cointegrated. Anyway, we could consider the cointegration case as an issue of minor relevance, in fact, it is always possible to test cointegration between the series of the shadow economy in level and the level of official GDP. Finally, as Giles and Tedds (2002a, p.149) have shown, the absence of cointegration doesn’t affect the validity of model. This model has an important economic meaning and thus has relevant implications for economic policy.

A further reason to eliminate unit roots from variables by using of the Maximum Likelihood estimation procedure, assumes the multivariate normality hypothesis of the sample. Since means and variances of the variables are needed to parameterize a multivariate Gaussian distribution completely then stationarity should be hold separately for any observed variables, therefore is desirable to transform variables into stationary time series. We argue that differencing was necessary for variables containing unit roots, and we do not exclude that official and unofficial economy are cointegrated, at the contrary, economic arguments suggest that there exists a long-run relationship between latent variable and the indicators.

That being stated, we invite all interested researchers to undertake further constructive contributions on this topic in order to provide some statistical procedure to improve the cointegration analysis. However, to Breusch’s criticism, we invite econometricians to stress deeply the consequences (and remedies) to use non-stationary variables in the Structural Equation Model. For instance, on this issue we recommend Cziráky (2005) he argues that it should be considered a new class of SEM much more adaptable to economic time series, the Dynamic SEM. This procedure is able to model correlated residuals.

We as purely applied researchers look forward for results on this line of research which could be very constructive contribution on this topic.

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<sup>7</sup> Obviously these variables cannot be cointegrated because they are stationary.

## 7. Is the MIMIC model appropriate?

According to Breusch, the statistical nature of MIMIC model is unsuitable for economic questions/problems as this approach was designed to psychometric application and “*to measuring intelligence seems far removed from estimating the underground economy (p.26).*”

Although, we agree that is (still) problematical to apply this methodology to an economic dataset and to specify macroeconomic model through the MIMIC framework, we are not convinced by the Breusch’s arguments that the real problem lies in the strong difference between economic (here to shadow economy) and the psychological variable.

Breusch’s argues: “*The level of underground income is some number, calculated on a well-defined system of measurement. It cannot be open to the researcher to slide or stretch this calculation to fit whatever scale is found to be convenient.*(Breusch, 2005b, p. 27)”.

We admit that only a few hypotheses are commonly accepted in this filed, one of those is that the underground economy is a very controversial issue and extremely difficult to measure. Several problems and questions should be tackled, when we define what should be considered to measure the SE. Just to give an intuition of these kinds of difficulties, it is sufficient to refer to the problems, measurement errors, for example, that deal with the observed side of production, the “official” GDP. As Lequiller<sup>8</sup> and Derek (2004) state: “*If ever there was a controversial icon from the statistics world, GDP is it*”. Very shortly, Gross Domestic Product is defined, as the sum of all goods and services produced in a country over time. Double accounting problems provide some handicaps in order to get a consistent figure. This important single figure is used to investigate whether an overall economy is expanding or contracting. In order to make an aggregated figure one has to define quite recently what production of goods and services can make or not however the statisticians’ conventions may sometimes look quiet arbitrary.

If it is so quiet problematic to measure correctly observed (official GDP), we cannot agree with the conclusion that the uncertainty on the size of SE is caused by the researcher’s convenience “*to slide or stretch*” the own estimates. Recently, to reduce the uncertainty in this field, Statistical Institutions (e.g. EUROSTAT) use a conventional definition for the not-observed production created by economic activities, it is called Non Observed Economy (NOE), that could be considered when measuring the Shadow Economy.

In particular OECD (2002) outlines a definition and method as well as complications to take in account in the national account the SE estimate. Again, there are many attempts when using different kinds of methodologies (national account based and econometrically supported ones) to estimate SE (e.g. Dell’Anno and Piirisild, 2004). Hence it is not rational, by considering the “state of art” to get ride of the theoretical and empirical abstracts as a matter of “researcher’s convenience”. Finally, we argue that not using the MIMIC model at this stage of macroeconomic research is not only premature, but the contrary, we believe that further

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<sup>8</sup> Lequiller heads the National Accounts of the OECD, and is a former chief of national accounts at the National Institute for Statistics and Economic Studies (INSEE).

research and a greater opening among disciplines could provide constructive comments to overcome these difficulties.

*“It doesn’t even make sense to suppose that some transformed versions of observed GDP and currency holdings are measurements of the same unobserved entity, whether or not that entity is called the underground economy (p.27).”*, states Breusch (2005b).

This criticism is quite common when latent estimation procedures are used. They refer to the reliability of “causes” and “indicators” in explaining the variability of the SE and the ambiguous meaning of unobservable variable. In D-S (2003) we tackle this problem: these criticisms are difficult to overcome, the obstacles depend on the nature of the statistical MIMIC approach.

The Structural Equation Modelling is a largely confirmatory rather than exploratory technique. That means a researcher is more likely to use SEM to determine whether a certain model is valid, rather than using it to “find” a suitable model. Therefore, it is possible that the theoretical construct defining “shadow economy” could include other potential definitions, (namely socio-economic development, welfare state, etc.).

Two supplementary criticisms raised by Breusch (2005b). They refer to both economic hypotheses behind the MIMIC model and the Jöreskog and Goldberger (1975) observation that given the causes  $x_1, \dots, x_q$  and the latent variable  $\eta$ , the indicators  $y_1, \dots, y_p$  are mutually *uncorrelated*.

We agree with Breusch’s statement that the MIMIC model specification is a very simplified description of the functioning of the economic system, but according with us his critique doesn’t affect the general appropriateness of MIMIC method. The MIMIC approach allows to test alternative economic theories according with scholars hypotheses.

The researcher, by using Structural Equation Modelling, has to solve at least two relevant trade-offs. The first one, between (increasing) complexity of the model and (decreasing) degrees of freedom<sup>9</sup>; the second one is between the (increasing) number of variables and the (increasing) sample size<sup>10</sup>.

Where is the best equilibrium to proceed here, it is open to debate and hence every economic theory could be questioned. What care us about at this time, it is to show that MIMIC approach doesn’t imply necessarily the assumption what:

*“...all of the connections that the indicator variables have with the causal variables, and with each other, are carried through the latent variable” (Breusch, 2005b, p. 28).*

Breusch, in order to better explain his thinking, disassembles previous statement in two parts:

(i) *“the indicators  $y$  are conditionally independent of the causes  $x$ , given the latent variable  $\eta$ ”;*

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<sup>9</sup> A sufficient number of degrees of freedom is need to test if the proposed model specification is confirmed by data.

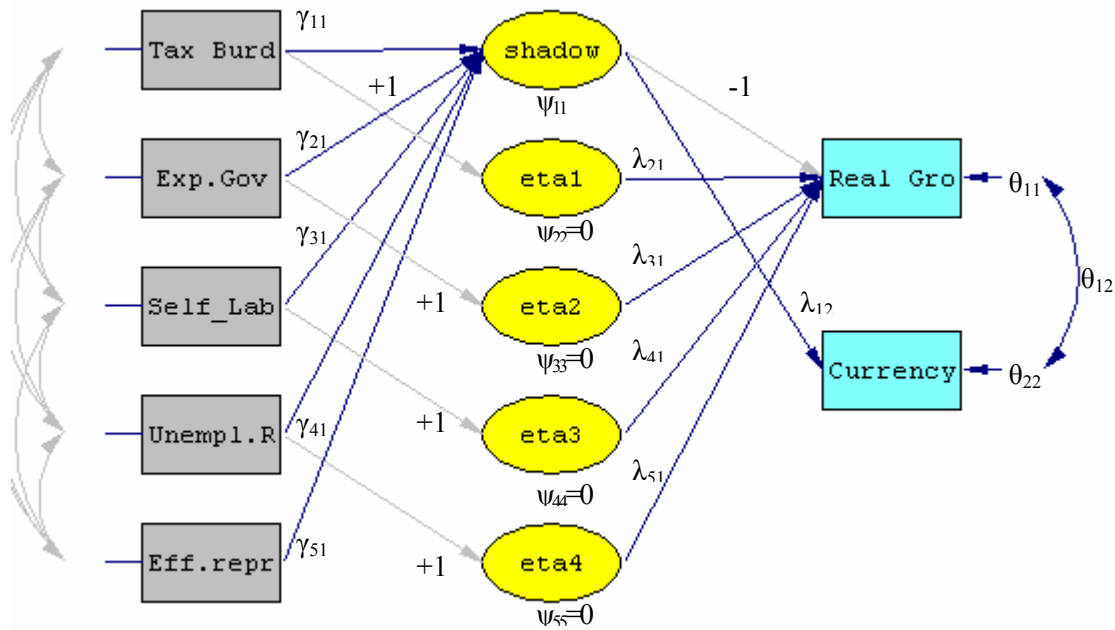
<sup>10</sup> There are several rules of thumb in the literature (Garson, 2005): the sample size should be at least 50 more than 8 times the number of variables in the model. Another one, based on Stevens (1996), is to have at least 15 cases per measured variable or indicator. Bentler and Chou (1987) recommend at least 5 cases per parameter estimate (including error terms as well as path coefficients). The researcher should go beyond these minimum sample size recommendations particularly when data are non-normal or incomplete.

(ii) “the indicators  $y_1, \dots, y_p$  are mutually independent, given the latent variable  $\eta$ ”;

As response to point (i), we point out that it is possible to estimate the connection between indicators and causes by providing an alternative specification of the MIMIC model.

To make clear our view, it is presented, as example, an alternative MIMIC model specification able to neutralize the general content of Breusch’s critique. In particular, we could consider a more complex specification of MIMIC 5-1-2 estimated by D-S (2003): the MIMIC 5-6-2. The path diagram of MIMIC 5-6-2 is shown in figure 1.

**Figure 1:** Path diagram of MIMIC model 5-6-2



In this model, some observed causes ( $x_q$ ) are doubled. In fact they are once considered as causes of latent variable (the “standard” MIMIC model specification), once more are replaced with  $s$  additional latent (endogenous) variables ( $\eta_1, \eta_2, \eta_3, \eta_4$ )<sup>11</sup>. This framework allows to relate the various causal factors that (could) influence the indicator(s) both with ( $\lambda_{1p}$ ) and without ( $\lambda_{21}, \lambda_{31}, \lambda_{41}, \lambda_{51}$ ) through of the latent variable (SE).

By generalizing, the MIMIC model q-1-p (single latent variable) is specified as a MIMIC q-m-p, where the number of latent variables  $m$  is equal to  $1+s$ , with  $s$  equals to the number of observed causes that are supposed to be (directly) correlated to the indicator(s). In this formulation the  $s$  supplementary latent variables are constrained to “mirror” exactly the corresponding observed causes ( $x$  variables) via specification of 1.0  $\gamma_{mq}$  and by fixing to zero the measurement errors of structural model ( $\psi_{mm}$ ). In symbols, the equation systems are as follow:

<sup>11</sup> For instance, by fixing  $\eta_1 = x_1 + 0$  then  $\lambda_{21}$  provide an estimate of the effect that changes in  $x_1$  (Tax Burden) have on  $y_1$  (Real Growth of GDP).



For the structural model:

$$\begin{pmatrix} Sh.Ec. \\ \vdots \\ \eta_m \end{pmatrix} = diag(0, \dots, 0) \begin{pmatrix} Sh.Ec. \\ \vdots \\ \eta_m \end{pmatrix} + \begin{pmatrix} \gamma_{11} & \dots & 0_{1q} \\ \vdots & 0 & \vdots \\ 1_{m1} & \dots & 0_{mq} \end{pmatrix} \begin{pmatrix} x_1 \\ \vdots \\ x_q \end{pmatrix} + \begin{pmatrix} \zeta_1 \\ \vdots \\ 0_m \end{pmatrix}$$

For the measurement model:

$$\begin{pmatrix} y_1 \\ \vdots \\ y_p \end{pmatrix} = \begin{pmatrix} -1 & \dots & \lambda_{1m} \\ \vdots & \ddots & \vdots \\ \lambda_{p1} & \dots & \lambda_{pm} \end{pmatrix} \begin{pmatrix} Sh.Ec. \\ \vdots \\ \eta_m \end{pmatrix} + \begin{pmatrix} \varepsilon_1 \\ \vdots \\ \varepsilon_p \end{pmatrix}$$

With reference to the covariances among errors of Structural model (matrix PSI in Lisrel syntax): it should be constrained as follow:

$$\Psi = diag(\psi_{11}, 0, \dots, 0)$$

With reference to the Breusch's point (ii), the reply could be considered both in a context of "standard" MIMIC specification and in the MIMIC q-m-p.

Breusch's critique (probably) refers to the standard assumption of MIMIC model where the measurement errors are assumed to be independent of each other (THETA-EPS matrix is diagonal), but this restriction could be relaxed. (see Stapleton, 1978, p. 53 and Dell'Anno, 2003, pp. 7-8). In the paper of D-S (2003), a great number of MIMIC models are estimated, the covariance between the real growth rate of GDP ( $Y_1$ ) and the growth rate of currency ( $Y_2$ ) is not statistically different from zero. Yet, in the models where this assumption is relaxed the changes in the estimates of structural coefficients are minor, therefore the standard restriction is hold in order to have more degrees of freedom. Confirmatory evidence that for the Italian analysis the correlation between official GDP and currency holdings is not relevant can be inferred by comparison of the estimated coefficients and outputs of the SIMIC model with the other MIMIC models (see in D-S, 2003: Table 3, Figure 2).

In symbols, the previous arguments imply to consider a not-diagonal THETA-EPS matrix as in the standard specification. Therefore the covariances among errors of Measurement Model (THETA-EPS matrix in Lisrel syntax) are as follow:

$$\Theta = \begin{pmatrix} \theta_{11} & \dots & 0 \\ \vdots & \ddots & \vdots \\ \theta_{p1} & \dots & \theta_{pp} \end{pmatrix}$$

## 8. Conclusions

This paper tries to give a response to several points of criticisms made by Breusch (2005b) against the MIMIC model used in three different papers including the Dell'Anno and Schneider paper (2003) too. Based on the direct knowledge of procedure and hypotheses in the paper by D-S (2003), special attention is paid to reply to criticism against the MIMIC method applied to estimate shadow economy in our paper.

It is shown that several points of Breusch's criticisms are misguided while others effectively highlight weaknesses of MIMIC approach. The latter, in our opinion should be considered mainly caused by the still "initial stage of research" in this field instead than the inappropriateness of the technique or trickeries of authors that "*stretched or shortened the results to fit the bed of prior belief. (p. 28)*".

The main difficulties go back to the features of SEM that is designed to use as a quantitative method in the social science and our attempt to adapt this procedure for econometric analysis of an economic problem. This means that is quite difficult to implement a time series analysis with this method and there are not specific guidelines to convert the index provided by MIMIC method to real figures.

In our opinion, the most attractive and important arguments of criticisms by Breusch are related to the process of benchmarking (i.e. calculating the absolute values of the shadow economy) and the (potential) questionability of the economic hypotheses behind the MIMIC specification.

On the benchmark process, we use his analysis as starting point to resume methodologies and synthesize our economic assumptions behind the calibration procedure. Further, we propose an alternative benchmark strategy that could be a step forward greater reliability of MIMIC estimates.

On the reliability of the economic hypotheses behind the MIMIC approach. We show that, in exchange for lower statistically reliability, it is possible to consider more complex versions of the measurement and structural framework. They are able to take in account alternative researcher's views. These extensions of the "standard" MIMIC method are further evidences of still largely under evaluated potential of SEM approach in Economics.

In general terms, with reference to the overall question of the appropriateness of estimates provided by MIMIC model, as Dell'Anno and Piirisild (2004) state there does not exist any commonly accepted methodology for estimating the underground economy. The estimates are always subjective and depend on the quality of the dataset the methods applied and the subjective decisions of the researcher. SE estimates are never very stable and absolute, and there is always space for questions, discussion and critique. Breusch's comments have revealed some weakness and differences in assumptions and procedures in our earlier study, and this will be most helpful for future researchers.

Furthermore we would like to highlight that, at the contrary of Breusch assumption, the authors of the three-replicated paper intended to draw attention how challenging and debatable are the estimates of SE. From this point of view the Breusch's criticism is superfluous. We also agree with Tedds and Giles (2005) conclusions that the MIMIC approach, which is distinct from the specific researcher's choice of variables of benchmarking and of transformations, "*still provides a fruitful way of estimating the principal features of a very broadly defined underground economy (p.394)*".

Finally, we'd like thank Breusch for his critical papers because the efforts and the additional research time need to reply and provide better arguments to our assumptions helped us to improve not only our knowledge in this field but also to compare the different strategies in the MIMIC applications.

A general consideration is, that quite often the vast majority of the audience interested in shadow economic papers is much less interested in the econometric methodologies<sup>12</sup> but much more in the development of this aggregate. Although it is valuable information to be able to know the errors of the value of SE but this is only one goal of researchers in this area. To measure the SE is one important aspect for a more general economic analysis but economists are mainly interested to understand the economic phenomenon of “shadow activities” rather than measure them. For policy makers and (macro-) economists it is much more attractive to be aware which are the main causes for the dynamics of the SE compared with a detailed knowledge to know with full reliability what may be values of the SE over time. According to us, these reasons and the lack of more reliable methods, suggest considering the (DY-) MIMIC approach a very useful tool for our economic profession to estimate the size and development of the SE.

Finally we would like to invite econometricians to develop new methods to estimate the shadow economy. We recognize that it is important and critical to eliminate false theories and estimation procedures. The scientific research in the field of the SE requires constructive contributions to improve estimation methods, or propose alternative techniques. (DY-) MIMIC users are usually aware of the limited reliability of final estimations but before to abandon the MIMIC method they need a better one to substitute for it, but unfortunately, at this time, there are no promising alternatives.

We accept to defend “our” method not only, because we are hit with some very critical statements in our professional but mainly to invite colleagues to focus their research to improve (DY-) MIMIC method. In the future we are waiting for some advise what kind of method should be used by economists to get information about dynamics and size of the SE. In our opinion, for this, the MIMIC model is still one of the best approaches to this purpose.

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<sup>12</sup> Quiet often the editorial strategy of economic journals has the main focus of drawing attention of an article by substituting econometrics analysis with economic analysis and implications for economic policy.

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## **Appendix: Some further remarks to Breusch's paper:**

In the following, we reply the criticism rose in Breusch's appendix against D-S (2003). According with Breusch (2005b) these additional findings are not essential to understanding the MIMIC model in general but illustrate just some anomalies discovered while replicating the paper.

We show that the most part of them are not authors' mistakes and (probably) depend on differences in dataset and misinterpretation.

Preliminarily, we believe that the replication exercise requires very carefulness and accuracy. In the paper by D-S (2003) the original MIMIC model included six causes and 2 indicators, while in the Breusch's paper replicated version two causes (index of efficacy of judicial system and index of illegality) are omitted; a different definition of tax burden is used; for one of two indicators (currency) probably a different source is considered. Hence only the half of the observed variables implemented by Breusch's replication are the same employed by D-S (2003).

### **(1) Negative variance:**

*In Dell'Anno and Schneider (2003), the problem parameter is the variance of the latent variable [...] The LISREL output file in these cases includes the prominent message "WARNING: PSI is not positive definite"(Breusch 2005b, p. 31).*

Although, this is an effective problem for economic application of MIMIC model and we explicitly state a p.113: *"The model approach may encounter indefinite matrix problems. As Monte Carlo studies demonstrate, when the data provide relatively little information (small sample size, few observed indicator variables, small factor loadings, missing values), non-positive definite matrix troubles arise frequently [Bollen and Long, 1993]. Unfortunately, these are the usual obstacles met by this kind of estimation procedures."* we didn't find these problems. We report the estimated (positive) variance of latent variable as shown in the LISREL output files:

Table:1 Variances of latent variable

<b>Model</b>	<b>Mimic 6-1-2</b>	<b>Simic 6-1-1</b>	<b>Mimic 5-1-2</b>	<b>Mimic 4-1-2</b>	<b>Simic 4-1-1</b>	<b>Mimic 3-1-2a</b>	<b>Mimic 3-1-2b</b>
Variance of latent variable (PSI)	92.46	1.74	106.49	191.53	1.42	106.60	291.54

### **(2) Goodness of fit:**

*"It seems the MIMIC model is not a good description of the data in either of these applications, despite the many measures of goodness-of-fit and the extensive diagnostic testing that are reported with the estimation results"(Breusch 2005b, p.31).*

We think to report the two most important measures of goodness-of-fit. The p-value of ‘chi-square’ and the Root Mean Square Error of Approximation (RMSEA) test are not “*many measures*”. About the “*extensive diagnostic testing*” we are not able to find in our paper this information.

### **(3) Definitions of variables:**

Breusch (2005b) is right when he points out that the second cause of our MIMIC model is the “*real government consumption (in percent of GDP)*” and not Government Consumption/deflator of Government consumption as stated wrongly in our paper to the *appendix 1*.

### **(4) Data period:**

Breusch (2005b) states:

*“The data period for estimation is unstated in the paper, but the results are replicated by using 1960s1 to 2000s2(although effectively the data begin in 1962s1 due to the creation of lags and missing observations in the currency variable), (Breusch 2005b, p. 35)”*

Although Breusch states that in D-S (2003) the period for estimation is unstated, he is able to understand correctly it. We think that his words belie his critique.

Anyway, we argue in the section 5.1 that:

- 1) “*Following the pioneering work of Frey - Weck-Hanneman [1984], the Italian shadow economy over the last forty years is estimated using the model approach and semi-annual data (D-S, 2003, p. 112).*”

As stated above the period of estimation covers the time from 1960s1 to 2000s2; i.e. 40 years. The slight difference between the estimation period and the sample used for the calculation of index is motivated by missing data and considering the first differences.

In particular, the first four data points (1960:1 to 1961:2) are not considered in the estimation period because there are missing observations<sup>13</sup> in one of the causes (the index of efficacy of judicial system) and one indicator (the currency in circulation outside of banks). Another observation (1962:1) is lost because are considered variables in first differences.

It is worthwhile to underline that differences in the sample do not have a significant effect on the estimated coefficients and statistical significance of model. As stated at p. 110 in the D-S (2003), Dell’Anno (2003, p. 36) shows an intertemporal stability of the MIMIC model coefficients for reduction of sample less than 37 percent (30 obs.).

### **(5) Exogenous estimate of Shadow Economy:**

Breusch (2005b) criticises the external estimate of SE as ratio of GDP using the index obtained by MIMIC model in order to determine cardinal values.

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<sup>13</sup> The listwise procedure is adopted in software PRELIS<sup>®</sup> to take in account missing values.

*“the overall level of the final product of Dell’Anno and Schneider is obtained as the simple average of five other estimates by various methods (p.35)”.*

We believe that an average of different exogenous estimates of SE is a reasonable method to calculate the level of SE as ratio of GDP. The average is lesser influenced by preference of researcher than the choice of just one than numerous figures available in literature.

*“... Most of these prior estimates come from an unpublished working paper by Schneider and Enste (2000), where they are documented as “own calculations” (p.35)”.*

About cited working paper, it is published on the Journal of Economic Literature (see table 8 p. 107), moreover no one of these values is indicated as “own calculation” but they are quoted with the complete references.

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