Inflation targeting did make a difference in industrial countries' inflation and output growth

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Abstract

I reevaluate the treatment effect of inflation targeting (IT) in industrial economies that adopted this regime in the 1990s through dynamic panel regressions to show that IT had significant enhancing effects on realized inflation and GDP growth. I also refine the propensity score matching of Lin and Ye (2007) and Ball and Sheridan's (2005) cross-section regressions to show that their conclusion of IT irrelevance can be overturned. By analyzing other samples that extend theirs, I provide further evidence of the pioneering IT systems good performance among developed countries.

JEL classification: E31, E52; E58.
Keywords: Inflation targeting; Inflation; Inflation-output growth short-run tradeoff

Resumo

Este artigo reavalia o efeito das metas de inflação (IT) nas economias industriais que adotaram este regime durante os anos 1990 através de painéis dinâmicos para mostrar que IT tiveram um efeito benéfico sobre a inflação e o crescimento do produto realizados. O trabalho também aprimora as aplicações de *propensity score matching* de Lin and Ye (2007) e de regressões seccionais de Ball and Sheridan (2005) para mostrar que suas conclusões de irrelevância da adoção de IT podem ser revertidas. Analisando outras amostras que estendem as deles, são apresentadas evidências adicionais do bom desempenho dos pioneiros sistemas de IT nos países desenvolvidos.

Palavras-chave: Metas de Inflação; Inflação; Escolha de curto prazo entre inflação e crescimento

1. Introduction

Has inflation targeting (IT) made monetary policy more efficient in developed economies? Have their pioneering IT systems, implemented in the early 1990s, resulted in better realizations of inflation and output growth? Or, has IT subsequently spread around the world just for fad, without an initial track record of good performance?

Although industrial central banks that have primarily adopted IT seem happy with their choice, to the point of having influenced the IMF recommendations and many other countries in a subsequent wave of IT introductions after the mid 1990s, the existing evidence in terms of realized inflation and output growth does not seem to support their optimism.[1, 2,3]

According to Ball and Sheridan (2005) [denominated BS hereafter], IT caused no significant improvement on realized rates of inflation and output growth in a sample of major industrial economies. They use cross-section difference-in-difference ordinary least squares regressions to compare the first seven countries that adopted IT (named ITers) against thirteen non-ITers observed until 2001. Although IT had a lowering effect on inflation and a positive impact on GDP growth, they were not significant, once the outcome processes are controlled for mean-reversion.

This irrelevance view of IT is shared by Lin and Ye (2007) [denominated LY hereafter], who apply the propensity score matching (PSM) approach to control for the

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[1] King (2002) and Bank of Canada (2006) are two good examples of Central Bank(er)s optimism with IT. IMF (2006) is a document that pleads IT adoption.

[2] See Walsh (2009) for a survey of the IT literature. He concludes that "… macroeconomic experiences among both inflation targeting and non-targeting developed economies have been similar …"

[3] Studies like Crowe (2010) and Roger and Stone (2005) conclude that IT improved transparency and accountability, but not realized macroeconomic aggregates, as inflation and output growth.

self-selectivity bias of policy adoption – a drawback of BS and most of the literature, according to LY. They confirm that IT had no significant effects on lowering inflation in an annual 1984-1999 panel of 22 major industrial countries, with the same 7 IT pioneers.

In this paper, I show that IT did matter for realized inflation and output growth in both BS's and LY's samples and in other samples that extend theirs. For example, by using the same LY's data, I show, through a dynamic panel, that the cumulative effect of IT on the annual inflation rate was an average reduction among ITers of 1.4 percentage points (shown below, in Table 4, column (5)). Although small, compared to the worldwide inflation reduction observed during the 1980s and 1990s, it is relatively large and significant, given that the average inflation among industrial economies in the late 1990s was close to 2% per year. In this same LY's sample, a dynamic panel for the per capita GDP growth rate shows that IT also had a positive impact on the average ITers' annual growth rate (in Table 5, columns (5) and (10)). In terms of the inflation-output tradeoff, modeled within an adaptive Phillips curve, the credibility bonus of IT policy amounted to a very significant cumulative effect of 1.6 percentage point reduction in the long run annual mean inflation of ITers (in Table 3, column (4)).

What has caused this qualitative change in the BS's and LY's IT irrelevance results, distancing the latter from my stated IT enhancing effects?

As Gertler (2005) warns, BS use a cross-section difference-in-difference OLS approach that is not sharp enough to evaluate the IT policy efficiency. First, BS's cross-section set-up ends up making inference from a sample of just 20 observations (one per country), which raises doubts over small sample robustness. In such a small sample, results change from insignificance to significance depending on how one treats outliers.

Second, given IT was not simultaneously adopted by all ITers, the “pre-IT” and “post-IT” time windows are different among countries, and the time effect variation cannot be ignored in a between-country comparison. Abstracted from the common time-variation problem, the cross-section difference-in-difference OLS approach also ignores country-specific fixed-factors affecting both inflation dynamics and IT adoption, and may erroneously suggest a causal relationship between IT and inflation. Additionally, in BS’s application, there is not even the concern for controlling for other observable covariates to minimize self-selection bias.

LY have perceptively advocated the use of the PSM by arguing that the literature in general and BS in particular have not addressed the self-selectivity bias of policy adoption. Setting aside for the moment that PSM methods do not address the above problems of common-time and country-fixed effects, being essentially a cross-section technique, LY applies the PSM to a panel without previously summarizing time series observations into one observation per country. Instead of one unit of treatment per IT country, 7 countries in their sample, LY consider each year-country under IT as a different unit going under treatment, 45 events in total – the sum of the number of years of treatment of each ITer country until 1999. Moreover, LY match those 45 IT-year-countries with non-IT-year-countries by their contemporaneous year observable characteristics, disrespecting the exogeneity of the control variables assumption (in least 38=(45-7) after first year cases).

These modeling choices have harmful consequences for LY’s PSM results and undermine their conclusions. LY are not asking the data if the countries that adopted IT had a different post-IT performance from countries that were similar to them before IT adoption. Without assuring the conditional independence assumption, LY ignore that those observable characteristics – originally controlled for because they were believed to simultaneously affect inflation and IT adoption – might have been influenced by previous years IT treatment through monetary transmission mechanisms, thus feeding the selection bias the PSM was intended to minimize.

Apart from LY's PSM implementation problems, a general warning about the PSM is that it solves the self-selection bias when all relevant characteristics influencing the treatment assignment are fully captured by observables. The assumption that the political macroeconomic decision of IT adoption is not affected by non-observables, like common-time and country-fixed effects, seems too strong, given policy fads and that inflation – the outcome variable IT wants to control – is affected by those non-observables (shown below in Tables 2 and 3, columns (1)-(3)). Moreover, it should be noted that PSM addresses the selection bias through the same conditional independence assumption that supports regression-based causality tests. Given that regression approximates the conditional expectation function, differences between PSM and regression estimates that control for the same covariates are unlikely to be of major empirical importance. These make clear that PSM has drawbacks in the IT context, and is thus best advised as a complementary exam rather than a superior technique.

Instead of working with a cross-section that makes it difficult to control for common time variation and countries' specificities, I suggest a dynamic panel approach that exploits the time and country dimensions to isolate the improvement in performance exclusive due to the IT regime from other sources that might overlap in a cross-section. In the evaluation of the non-simultaneous IT treatment, to control for the worldwide

1980-90s trend of fall in inflation and in macroeconomic volatility is particularly important. By investigating the within-country variation, the control for country fixed-effects and for an appropriate set of observable covariates addresses some of the omitted variable bias and improves the inference on the causal effect of IT on the economic performance. I choose to model data at annual frequency and to identify the IT effects with a one-year lag, together with other observable controls. The annual frequency is a simple solution to minimize the autocorrelation of the residuals, while parsimoniously keeping the estimated equations in the AR(1) family. The one-year lag of the explanatory variables gives some time for the sluggish responses to monetary shocks of macro variables, as documented by Batini and Nelson (2002) for monetary policy in general and by Johnson (2002) for IT practice, and also weakens some endogeneity suspects of the IT policy.[4] The small number of countries (N) relative to number of years (T), and the likely weak exogeneity of the IT variable drive me to use the pooled Ordinary Least Square (OLS) estimation with common time and country dummies (also known as Least Square Dummy Variable – LSDV), clustering standard errors by country for accurate inference purposes, choices I defend in more details below (in the Methodology, section 3).

Besides analyzing LY's 1984-1999 annual balanced panel of 22 industrial economies, compound with (i) BS's or (ii) Mishkin and Schmidt-Hebbel's (2007) [denominated MSH hereafter] calendar of IT adoptions, I study: (iii) LY's 1984-1999 reduced to the 18 most similar countries, (iv) an annual balanced panel that extends LY's data back to 1971, (v) an annual unbalanced panel that covers BS's 20 industrial countries between 1985-2001, and (vi) another unbalanced panel with BS's 20 economies

[4] I have also examined versions with a contemporaneous IT dummy as an explanatory variable, instead of a one-year lagged IT dummy - estimated both by OLS and instrumental variable - and have got the same significant results. Those can be provided upon request.

between 1980-2009. In all samples, I have been able to show that the IT policy improved realized inflation and output growth.

In addition to the several robustness exercises just listed, I address the differences between "converging" and "constant" ITers. After explaining that the existing literature tests of effectiveness of constant IT have been misspecified (in BS, LY and MSH for examples), I find that this stage also significantly mattered, although less strongly than the converging stage. I also show that the impacts of IT on the volatilities of inflation and output were negative, but mostly insignificant. Even so, given that inflation and output growth rates had to go through unavoidable volatility for them to gradually converge to the new long-run goals during the period studied, the fact that IT did not cause increases in volatilities is actually another result of IT effectiveness in improving macroeconomic performance at its inception.

Having overturned BS's and LY's conclusions of IT irrelevance, I then revisited BS's cross-section data and detected that Portugal and New Zealand should have been treated as outliers because their pre-IT inflations were much higher than others.[5] By re-estimating BS's cross-section regressions using a robust technique (the MM estimator by Yohai (1987)), or including a dummy for these countries pre-IT period high inflation, or just dropping them out of the sample, I am able to show that IT significantly lowered inflation among industrial countries in BS's sample.

Finally, I reformulate LY's PSM to fulfill the requirements of: (i) one unit of treatment per ITer country and (ii) match on observables before treatment; and reevaluate the differences between ITers' and non-ITers' performances in LY's sample through the

[5] BS sample does not include Greece and Iceland (included in LY), or these two countries would be categorized as outliers as well.

following years post-IT adoption. One more time, I get to the conclusion that the IT framework resulted in significant improvements in the realizations of inflation and output growth among industrial countries.

In sum, this paper overturns the previous literature and reveals that the initial track record of IT in terms of realized inflation and output growth was significantly positive to support policy-makers optimism and its spread around the world after the mid-nineties.

The article is organized as follows. The data sources and various samples used are described in section 2. Section 3 shortly describes the panel regression methodology used and compares it with the cross-section regression and the PSM techniques. Subsection 4.1 reports and discusses the main results. Ball and Sheridan (2007) and Lin and Ye (2007) are respectively revisited in subsections 4.2 and 4.3. The conclusions are in section 5.

2. Data

I examine samples of industrial countries similar to BS's and LY's, in which evidences of IT effectiveness in improving inflation and output growth were not found. The BS's sample is composed of 20 major OECD economies, divided into a treatment group of 7 IT countries: Australia, Canada, Finland, New Zealand, Spain, Sweden and United Kingdom; and a control group of 13 non-IT countries: Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Switzerland and the United States. The LY's sample has the same 20 economies plus Greece and Iceland added to the control group, totaling 22 countries.

These samples are particularly interesting because they cover the IT introduction. Given monetary policy is assumed to be neutral in the long-run, it is at IT inception that we are better able to evaluate its short-run real effects and tradeoffs. Moreover, with the passing of time, many of the novel operating procedures introduced by the pioneering IT Central Banks got adopted by other non-explicit-IT Central Banks, which makes it more difficult to test for the effectiveness of the IT treatment during the 2000s.

< Insert **Table 1** around here >

Aiming to compare with those previous works and to provide robust evidence, I study five samples of data at annual frequency, as described in Table 1. First, and the focus of this paper, I analyze LY's balanced panel of 22 major OECD economies from 1984 to 1999, a sample I call B22-8499 (in Table 1, column (1)). Like in LY, my data comes from Ghosh et al. (2002), who derive their data from IMF sources as described in the Appendix A.

To weaken suspicions that the treatment and control groups are different populations, or of self-selection bias, I also study a sample of 18 countries, named B18-8499 (in Table 1, column (2)), that uses the same data as B22-8499, but that excludes the four countries that experienced inflations above 20% per year in at least one year between 1984 and 1999: Greece, Iceland, New Zealand and Portugal.

From Ghosh's et al. (2002) data, it is possible to study those same 22 major OECD economies from 1971 to 1999, a sample I call B22-7199 (in Table 1, column (3)), useful to address the concerns that the results were influenced by the short length of time period observed.

The fourth sample, U20-8501 (in Table 1, column (4)), is an unbalanced panel from 1985 to 2001 of the 20 countries studied by BS. Like them, my sample finishes in 1998 for Euro area countries because of the common European Central Bank policy since 1999, and in 2000 and in 1999, respectively for Norway and Switzerland because of difficulties in evaluating their relatively late IT adoptions in just two years. The fifth sample, U20-8009 (in Table 1, column (5)), is an extension of U20-8501 that starts in 1980 for all 20 countries. It is still covering the 9 Euro area countries until 1998, but covers the other 11 countries up to 2009. In this sample, Norway and Switzerland become ITers at the beginning of the 2000s. The data in U20-8501 U20-8009 are derived from various sources as described in the Appendix A.

The data are difficult to work with because of cross-country heteroskedasticity, exacerbated by the persistence of high inflation rates (above 20% per year) in some countries until the 1970s (Japan, Spain, United Kingdom), 1980s (Ireland, Italy, Portugal, New Zealand), and the early-1990s (Greece, Iceland, Italy, Portugal, New Zealand). Given that I will be working with time series, I include a threshold dummy for inflation rates higher than a certain ceiling as control variables. A dummy called $high20_{n,t}$ is one for country *n* from the beginning of the sample until the last year that country had the annual inflation above 20% and zero from then on. The variable $big40_{n,t}$ is a standard dummy that equals one in the year that the country's n annual inflation is above 40%, being zero otherwise. Among the samples studied, $big40_{n,t}$ is activated only for Iceland in sample B22-7099.

The existing literature diverges on when to date the adoption of IT, and distinguishes between converging and constant IT (for examples of different IT calendars, see Johnson (2002), Corbo et al. (2002), Roger and Stone (2005), BS, and MSH). In columns (6)-(7) of Table 1, MSH's IT adoption dates are the closest to Bernanke's et al. (1999) country-case studies and the IMF (2006). They also better represent the common central banks operating procedure of initializing an IT regime with explicit targets for the coming year on, and not for the current year, described in Johnson (2002). The time lag recognizes the stylized fact documented by Friedman (1961) and Batini and Nelson (2002) among others that it takes some time for monetary policy to transmit to the economy. This is an aspect I model below and, thus, the reason why I prefer to focus on MSH's IT adoption dates. However, given my intention to compare results with BS's and LY's, BS's IT adoption dates deserve equal attention in the following presentation, in columns (8)-(9) of Table 1.[6]

3. Methodology

First, I work with a partial adjustment model:

$$y_{n,t} = \alpha^{y} \cdot y_{n,t-1} + \beta^{y} \cdot d^{IT}_{n,t-1} + \gamma^{y} \cdot X_{n,t-1} + \delta^{y}_{t} + \eta^{y}_{n} + \upsilon^{y}_{n,t}, \qquad (1)$$

where: $y_{n,t}$ is some macroeconomic performance indicator of interest; subscript $n = 1, 2, \ldots, N$ is for country and $t = 1, 2, \ldots, T$ is for period in years. For concreteness, I will sometimes refer to $y_{n,t}$ as "inflation" of country n in year t, but a similar reasoning

[6] Although LY claim to use BS's adoption dates, they mistakenly consider Spain as a constant inflation targeter after 1995. However, BS catalogue Spain as a convergent inflation targeter that joined the European Central Bank common policy since 1999, before reaching the constant stage. I follow BS.

can be applied to other indicators of macroeconomic performance, such as real output growth, inflation volatility or output growth volatility. The lagged value $y_{n,t-1}$ on the right-hand side captures persistence and mean-reverting dynamics. Among the independent variables, my focus is going to be on the IT dummy variable $d_{n,t-1}^{IT}$, equal to *1* if country n is an inflation targeter in period *t-1* and *0* if it is not. Hence $d_{n,t}^{IT}$ is the treatment variable. The one year lag gives time for policy to transmit to the economy (for treatment to take effect). The vector $X_{n,t-1}$, accounts for other covariates. For example, when y refers to inflation, $X_{n,t-1}$ can include the per capita GDP growth, among others. The term δ_t^y allows for time-effects that capture common shocks to all countries, and η_n^y allows for cross-country fixed-effects. The vector $\theta^y = (\alpha^y, \beta^y, \gamma^y)$ of common coefficients has β^y as the main parameter of interest for evaluation of the IT policy effectiveness. Due to equation (1) AR(1) structure, IT policy implies a cumulative effect on y of $\beta^y / (1-\alpha^y)$.

BS's model can result from the time degeneration of equation (1), which sum up all the data available into t = *pre-IT, post-IT* periods, thus turning the *(T-1)-period* dynamic panel (1) into a cross-section:

$$y_{n,\,post} = \alpha^y \cdot y_{n,\,pre} + \beta^y \cdot d_{n,\,post}^{IT} + \gamma^y \cdot X_{n,\,pre} + \delta^y + e_{n,\,post}^y \qquad \forall n \ ,$$

where: $y_{n,\,post}$ is country's n post-targeting value of average inflation, $y_{n,\,pre}$ is its pre-targeting value, and $e_{n,\,post} = (\eta_n + \upsilon_{n,\,post})$. By subtracting $y_{n,\,pre}$ on both sides of the above equation, BS estimate:

$$y_{n,\,post} - y_{n,\,pre} = \alpha^{y}{}' \cdot y_{n,\,pre} + \beta^{y} \cdot d^{IT}_{n,\,post} + \gamma^{y} \cdot X_{n,\,pre} + \delta^{y} + e^{y}_{n,\,post} \qquad \forall n \ , \qquad (2)$$

with $\alpha^{y}{}' = (\alpha^{y} - 1)$.

In equation (2), it makes sense that treatment variable $d^{IT}_{n,\,post}$ is contemporaneous with the dependent variable $y_{n,\,post}$, given $d^{IT}_{n,\,pre}$ is always zero. The necessary time lag for treatment to transmit to the economy is not a concern in the cross-section setup because the outcome variable is summary statistics (for example, the mean or the standard deviation) over many post-treatment years. However, equation (2) parameters estimates may be biased because of the omission of the time-effect variation δ^{y}_{t} and the impossibility of identifying the country-effect variation η^{y}_{n} from $e^{y}_{n,\,post}$.

As Bertrand et al. (2004) point out, to ignore the time series information of the data would work well only if all targeters had adopted the regime at the same time. Then, the periods covered by *pre-IT* and *post-IT* windows would be the same for every country, incorporating exactly the same combination of time-effect variation, and cancelling out in a between-country comparison. But given IT adoption happened at different times for different economies, the pair of *pre-IT* and *post-IT* periods are not the same for all IT countries and have to be arbitrarily defined for non-ITers. This means that the time-effect variation δ_t cannot be ignored in the IT analysis.

Abstracted the common time-variation problem, BS's cross-section regression would be useful to investigate the between-country variation, which is to ask whether targeters have lower inflation. However, equation (2) also ignores unobserved country-specific factors affecting both inflation dynamics and IT adoption, and may erroneously suggest a causal relationship from IT to inflation. To investigate the "within-country" variation, which is to ask whether a country is more likely to have a lower inflation in case it adopts the IT framework, it is necessary to control for country-specific observed and non-observed factors affecting both inflation and IT adoption. In addition to the inclusion of appropriate covariates $X_{n,t-1}$ in the regressions, this control can partially be accomplished by the use of country-effects η_n^y, like in equation (1). Although η_n^y does not control for the time variation of those country-specific non-observable factors, it removes at least their time-invariant part, improving inference on the causal effect.

In above equations (1) or (2), the estimated β^y is intended to represent the average direct causal effect of IT on y over all years and all ITers. However, this is only exact if all confounding variables have been properly included in the model and if the linear parameterization chosen is enough to correct for differences between treated and control units. In other words, regression-based causality tests assume a simple conditional independence assumption to assure that treated and non-treaded subjects in the sample become comparable and there is no selection bias.

Because a regression-based conditional independence test relies on a model of the process determining inflation (or other macroeconomic variable), it implies auxiliary assumptions that are hard to assess and interpret. Hence, alternative tests able to relax those assumptions are worth considering. Assuming that assignment to treatment only depends on observable variables, but not on non-observables, and more focused on the estimation of the propensity score of policy adoption, $p(y_{n,pre}, X_{n,pre}) \equiv E[d^{IT}_{n,post} \mid y_{n,pre}, X_{n,pre}]$, where $p(y_{n,pre}, X_{n,pre})$ is the conditional probability of IT adoption, instead of on the model for outcomes, $E[y_{n,post} \mid d^{IT}_{n,post}, y_{n,pre}, X_{n,pre}]$, the PSM approach is an alternative. According to Angrist and Pischke (2009), this is attractive in applications where the former is easier to model or motivate, while by leaving the model for outcomes unspecified, PSM should increase robustness.

LY and Vega and Winkelried (2005) follow this approach, emphasizing they address the self-selection problem of IT policy adoption ignored by BS and others. Not explicit in their argument, though, is that it is the control for $X_{n,pre}$, and not the PSM framework itself, which minimizes the selection bias. Although PSM provides a robust measure of the conditional expectation function (CEF), given that regression also approximates the CEF, differences between the PSM average treatment effect on the treated (*ATT*) and the regression β^{y} estimates that control for the same covariates are unlikely to be of major empirical importance. As Angrist and Pischke (2009) show, in a cross-section context, both can be motivated as a weighted matching estimator:

$$E\left\{ g(y_{n,pre}, X_{n,pre}) \cdot \left[\frac{\left(d^{IT}_{n,post} - p(y_{n,pre}, X_{n,pre})\right) \cdot y_{n,post}}{p(y_{n,pre}, X_{n,pre}) \cdot \left(1 - p(y_{n,pre}, X_{n,pre})\right)} \right] \right\},$$

where $g(y_{n,\,pre}, X_{n,\,pre})$ is a known weighting function. For the PSM ATT, $g(y_{n,\,pre}, X_{n,\,pre}) = \frac{p(y_{n,\,pre}, X_{n,\,pre})}{p(d^{IT}_{n,\,post} = 1)}$, meaning that PSM weights are proportional to the probability of treatment at each value for the covariates. For the β^{y} regression, $g(y_{n,\,pre}, X_{n,\,pre}) = \frac{p(y_{n,\,pre}, X_{n,\,pre}) \cdot (1 - p(y_{n,\,pre}, X_{n,\,pre}))}{E[p(y_{n,\,pre}, X_{n,\,pre}) \cdot (1 - p(y_{n,\,pre}, X_{n,\,pre}))]} = \frac{\sigma^2_{d^{IT}}(y_{n,\,pre}, X_{n,\,pre})}{E[\sigma^2_{d^{IT}}(y_{n,\,pre}, X_{n,\,pre})]}$, meaning that regression weights are proportional to the variance of treatment at each value for the covariates.[7] Thus, it is BS's and others' lack of control for $X_{n,\,pre}$ in the estimation of equation (2) that may flaw previous literature results, rather than the linear regression approach.

Further, as noted by Dehejia and Wahba (2002): "An important issue is whether the assumption of selection on observable covariates is valid, or whether the selection process depends on variables that are unobserved ... Only when the researcher is comfortable with the former assumption do PSM methods come into play." In the IT literature, the propensity score of IT adoption is far from well understood and the assumption that the political macroeconomic decision of IT adoption is not affected by common-time and country-fixed non-observables seems too strong, given the cluster of IT adoption in the beginning of the 1990s, and that the inflation process IT wants to control is affected by common-time and country-fixed effects.[8] The above points make clear that PSM has drawbacks in the IT context, and is thus best advised as a complementary exam rather than a superior technique.

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[7] Note that for the average treatment effect (*ATE*), $g(y_{n,\,pre}, X_{n,\,pre}) = 1$.

[8] See LY, Vega and Winkelried (2005) and Mishkin and Schmidt-Hebbel (2002) for different models of propensity score of IT adoption.

Yet abstracted the plausible common-time variable and country-fixed effects, to better understand LY's results, it is enlightening to present how the PSM would work fine in a panel context, which is complicated by the fact that potential outcomes are determined not just by current policy actions but also by past policy actions and covariates. Given that IT country ni adopts the IT treatment from the year k_{ni} on, i.e. for $k_{ni} \leq t' \leq T$, $d^{IT}_{ni,t'} = 1 \; \forall \; t'$, to measure the average causal effect of the IT on the outcome variable $y_{n,\,h+k_{ni}}$ after h years of treatment, the researcher would have to compute:

$$ATT_h = E\left[y^1_{ni,\,h+k_{ni}} - y^0_{ni,\,h+k_{ni}} \mid d^{IT}_{ni,\,h+k_{ni}} = 1\right] ,$$

where $y^1_{ni,\,h+k_{ni}}$ is the outcome in period $(h + k_{ni})$ if country ni has adopted IT and $y^0_{ni,\,h+k_{ni}}$ if not.

However, because the outcome that would have been observed if IT country ni had not adopted IT policy $y^0_{ni,\,h+k_{ni}} \mid d^{IT}_{ni,\,h+k_{ni}} = 1$ is not observable, the researcher assumes that potential outcomes are independent of treatment status conditional on a scalar function of covariates, i.e. the conditional independence assumption $\left\{y^1_{n,\,h+k_n}, \, y^0_{n,\,h+k_n}\right\} \perp d^{IT}_{n,\,h+k_n} \mid p\left(y_{n,\,k_n-1}, \, X_{n,\,k_n-1}\right)$, to replace $E\left[y^0_{ni,\,h+k_{ni}} \mid d^{IT}_{ni,\,h+k_{ni}} = 1, \, p\left(y_{ni,\,k_{ni}-1}, \, X_{ni,\,k_{ni}-1}\right)\right]$ by observable $E\left[y^0_{n,\,h+k_{ni}} \mid d^{IT}_{n,\,h+k_{ni}} = 0, \, p\left(y_{n,\,k_{ni}-1}, \, X_{n,\,k_{ni}-1}\right) = p\left(y_{ni,\,k_{ni}-1}, \, X_{ni,\,k_{ni}-1}\right)\right]$, and equivalently measure:

$$ATT_h = E\left[y^1_{ni,h+k_{ni}} \mid \begin{matrix} d^{IT}_{ni,h+k_{ni}} = 1, \\ p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \end{matrix} \right] - E\left[y^0_{n,h+k_{ni}} \mid \begin{matrix} d^{IT}_{n,h+k_{ni}} = 0, \\ p\left(y_{n,k_{ni}-1}, X_{n,k_{ni}-1}\right) = p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \end{matrix} \right],$$

or its sample analogue:

$$ATT_h = \frac{1}{N_{IT}} \sum_{ni=1}^{N_{IT}} \left\{ y^1_{ni,h+k_{ni}} - E\left[y^0_{n,h+k_{ni}} \mid d^{IT}_{n,h+k_{ni}} = 0,\ p\left(y_{n,k_{ni}-1}, X_{n,k_{ni}-1}\right) = p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \right] \right\},$$

where N_{IT} is the number of ITer.

If additionally one assumes that common-time varying effects do not affect the outcomes, or the common-time varying effects have been extracted from the outcome series, there is no need that the treatment and controls outcomes occur at the same time:

$$ATT_h = E\left[y^1_{ni,h+k_{ni}} \mid \begin{matrix} d^{IT}_{ni,h+k_{ni}} = 1, \\ p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \end{matrix} \right] - E\left[y^0_{n,h+\tau} \mid \begin{matrix} d^{IT}_{n,h+\tau} = 0, \\ p\left(y_{n,\tau-1}, X_{n,\tau-1}\right) = p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \end{matrix} \right],$$

and

$$ATT_h = \frac{1}{N_{IT}} \sum_{ni=1}^{N_{IT}} \left\{ y^1_{ni,h+k_{ni}} - E\left[y^0_{n,h+\tau} \mid d^{IT}_{n,h+\tau} = 0,\ p\left(y_{n,\tau-1}, X_{n,\tau-1}\right) = p\left(y_{ni,k_{ni}-1}, X_{ni,k_{ni}-1}\right) \right] \right\}. \qquad (3)$$

However, to measure the cumulative ATT over $(H_a + 1)$ years in a panel context, one has to average the ATT_h s :

$$\overline{ATT}_{H_a} = \frac{1}{(H_a+1)}\sum_{h=0}^{H_a} ATT_h$$

$$= \frac{1}{(H_a+1)}\sum_{h=0}^{H_a}\frac{1}{N_{IT}}\sum_{ni=1}^{N_{IT}}\left\{y^1_{ni,\,h+k_{ni}} - E\left[y^0_{n,\,h+\tau} \mid d^{IT}_{n,\,h+\tau}=0,\ p\left(y_{n,\,\tau-1}, X_{n,\,\tau-1}\right)=p_{ni}\right]\right\}. \tag{4}$$

which, for the biggest H_a possible, approximates the cross-section ATT measure over all post-treatment years, usually calculated in the PSM literature:[9]

$$ATT_{post} = E\left[y^1_{ni,\,post} \mid \begin{matrix} d^{IT}_{ni,\,post}=1, \\ p\left(y_{ni,\,pre}, X_{ni,\,pre}\right)\end{matrix}\right] - E\left[y^0_{n,\,post} \mid \begin{matrix} d^{IT}_{n,\,post}=0, \\ p\left(y_{n,\,pre}, X_{n,\,pre}\right)=p\left(y_{ni,\,pre}, X_{ni,\,pre}\right)\end{matrix}\right], \tag{5}$$

this last one being the PSM analogue of the cross-section difference-in-difference β^y .

LY's PSM results are flawed because, when attempting to compute (4), or (5), they ignore that the year of IT adoption k_{ni} is a definite and fixed one for each country for exogeneity to be preserved. Instead of conditioning on $p\left(y_{ni,\,k_{ni}-1}, X_{ni,\,k_{ni}-1}\right)=p_{ni}$ for every horizon $h \geq 0$, LY condition on $p\left(y_{ni,\,h+k_{ni}-1}, X_{ni,\,h+k_{ni}}\right)$, and compute the measure:

$$\overline{LYATT} = \frac{1}{N_{IT}}\sum_{ni=1}^{N_{IT}}\frac{1}{(T-k_{ni}+1)}\sum_{h=0}^{T-k_{ni}}\left\{y^1_{ni,\,h+k_{ni}} - E\left[y^0_{n,\,h+\tau} \mid \begin{matrix} d^{IT}_{n,\,h+\tau}=0, \\ p\left(y_{n,\,h+\tau-1}, X_{n,\,h+\tau}\right)=p\left(y_{ni,\,h+k_{ni}-1}, X_{ni,\,h+k_{ni}}\right)\end{matrix}\right]\right\},$$

[9] Vega and Winkelried (2005) use equation (5) to evaluate the IT policy. However, although they analyze industrial ITers performance separately, they do it against a control group that includes both industrial and emerging economies, thus not restricting the comparison group to units that are similar to the exposed units, what makes their results not comparable to LY's and mine.

thus matching on observables that have already been influenced by previous h years of treatment. Without loss of generality, take just the conditioning observable lagged outcomes $y_{ni,\,h+k_{ni}-1}$ and $y_{n,\,h+\tau-1}$ into account. LY are matching the treated outcome after h years of treatment $y^{1}_{ni,\,h+k_{ni}}$ with non-treated $y^{0}_{n,\,h+\tau}$'s not because they were similar before treatment started *(h+1)* years ago, $y_{ni,\,k_{ni}-1} \approx y_{n,\,\tau-1}$, but because they were similar last year, $y_{ni,\,h+k_{ni}-1} \approx y_{n,\,h+\tau-1}$, what clearly results in underestimation of the treatment effect through time.

Having explained that the linear dynamic panel procedure controlled for the suitable set of covariates is not dominated by the PSM approach, I still have to choose a regression estimation method. Regarding the estimation method of equation (1), the LSDV estimator may have a bias when *T* is small, as demonstrated by Nickell (1981). With big *N* and small *T*, Difference GMM by Arellano and Bond (1991), or System GMM by Blundell and Bond (1998) would be advised. However, Judson and Owen (1999) and Roodman (2008), among others, indicate the weak instruments and over-fit risks of applying these techniques to panels with small *N* relative to *T*. Judson and Owen (1998) additionally show that LSDV performs well when *N* is small and *T* increases. Bruno (2005) develops a correction of the LSDV estimator for panels, but that is only valid for strictly exogenous regressors. Thus, in the absence of a best technique to handle this small *N* panel with weakly exogenous regressors and given the possibility to perform robustness checks by increasing *T*, I choose to use the LSDV estimator.

The LSDV, or fixed-effect OLS estimator, is biased if the transformed independent variables $y^{*}_{n,\,t-1} = y_{n,\,t-1} - (T-1)^{-1}\left(y_{n,1} + \ldots + y_{n,T-1}\right)$ and

$Z^*_{n,t-1} = Z_{n,t-1} - (T-1)^{-1}(Z_{n,1} + ... + Z_{n,T-1})$, for $Z_{n,t-1} = (d^{IT}_{n,t-1}, X_{n,t-1})$, are correlated with the transformed error $\upsilon^{y*}_{n,t} = \upsilon^{y}_{n,t} - (T-1)^{-1}(\upsilon^{y}_{n,2} + ... + \upsilon^{y}_{n,T})$. The correlation between $y^*_{n,t-1}$ and $\upsilon^{y*}_{n,t}$ has been shown by Nickell (1981) to be negative, because the terms $y_{n,t-1}$ and $-(T-1)^{-1} y_{n,t}$ in $y^*_{n,t-1}$, respectively, correlate with the terms $-(T-1)^{-1}\upsilon^{y}_{n,t-1}$ and $\upsilon^{y}_{n,t}$ in $\upsilon^{y*}_{n,t}$, resulting in a downward biased estimator of α^y , known as the dynamic panel bias problem, which shrinks as T gets larger.

The biases in the (β^y, γ^y) fixed-effect OLS estimators of (1) depends on the covariances $\text{cov}(Z_{n,t-1}, \upsilon^{y}_{n,t-j})$ for all $t \geq 2$ and *j = 1, ..., (t - 2)*. Specifically for the inflation-IT relation, when $y = \pi$ represents the inflation rate, once switches to the IT regime are more probable when inflation is already low (grounded on the estimated Probit models of Table 7 below), it is not unreasonable to suspect that $\text{cov}(d^{IT}_{n,t-1}, \upsilon^{\pi}_{n,t-j}) \leq 0$ for $j > 1$. This being the case, the $\text{cov}(d^{IT}_{n,t-1}, \upsilon^{\pi}_{n,t-j}) \leq 0$ for $t \geq 2$ and *j = 2, ..., (t - 2)* would bias the β^{π} fixed-effect OLS estimates of (1) upwards, thus making it more difficult to find that IT decreases inflation. In the output growth-IT relation, when $y = GDPPCG$ represents per capita output growth, the $\text{cov}(d^{IT}_{n,t-1}, \upsilon^{GDPPCG}_{n,t-j}) \leq 0$ for $t \geq 2$ and *j = 2, ..., (t - 2)* (grounded on the estimated Probit models shown in Table 7) would bias the β^{GDPPCG} fixed-effect OLS estimates of (1) upwards, making it easier to find a positive IT effect on output growth that should be interpreted with care.

Because serially correlated residuals might cause the standard error for estimated β^y to severely understate the standard deviation of population β^y, as Bertrand et al. (2004) have shown, I report tests of whether the error term $\upsilon^y_{n,t}$ is serially correlated. The assumption that the error terms are not serially correlated is equivalent to their first-differences $\Delta\upsilon^y_{n,t}$ being first-order correlated but not second-order correlated.

4. Results

Next, in subsection 4.1, I present the linear regression results. As explained in section 3, a dynamic panel that controls for observable covariates is as able to address the IT causal effect on inflation as the PSM, with the additional advantage of taking relevant non-observables into account and making explicit the roles of past policy actions and observable covariates in the inflation determination.

To make sure that the main results I have gotten in subsection 4.1 are not technique sensitive, in subsections 4.2 and 4.3, I show that similar qualitative results could have been obtained by cross-section difference-in-difference and PSM, if BS and LY had respectively addressed the outliers problem and assured exogeneity of covariates.

4.1. Main results

In Tables 2 to 5, I show estimates of variations of equation (1), presented in section 3. Table 2 presents estimates of the following adaptive Phillips curve with a set of controls:

$$\pi_{n,t} = \alpha^{\pi} \cdot \pi_{n,t-1} + \beta^{\pi,IT} \cdot d^{IT}_{n,t-1} + \gamma^{\pi}_{open} \cdot OPEN_{n,t-1} + \gamma^{\pi}_{bmg} \cdot BMG_{n,t-1} + \gamma^{\pi}_{cbturn} \cdot CBTURN5_{n,t-1}$$
$$+ \gamma^{\pi}_{cggdp} \cdot CGGDP_{n,t-1} + \gamma^{\pi}_{fix} \cdot FIX_{n,t-1}$$
$$+ \gamma^{\pi}_{gdppcg} \cdot GDPPCG_{n,t-1} + \gamma^{\pi}_{ttg} \cdot TTG_{n,t-1} \quad (6)$$
$$+ \delta^{\pi}_{t} + \eta^{\pi}_{n} + \upsilon^{\pi}_{n,t}.$$

Beyond the current and lagged inflations ($\pi_{n,t}$ and $\pi_{n,t-1}$), the per capita GDP growth ($GDPPCG_{n,t-1}$), and the terms of trade growth ($TTG_{n,t-1}$), usual variables in an open economy adaptive Phillips curve,[10] following Ghosh et al. (2002) and LY, I include other potential determinants of inflation suggested in the literature, to certify that the IT effect shows up in a very controlled model.

According to Romer (1993), greater trade openness ($OPEN_{n,t-1}$) should imply lower inflation, given the higher costs of a monetary expansion. Faster growth of the money supply ($BMG_{n,t-1}$) should trivially be associated with higher inflation. Cukierman (1992) argues that a higher turnover rate of the central bank governor ($CBTURN5_{n,t-1}$) should be associated with higher inflation. By its importance in aggregate demand, higher government surplus ($CGGDP_{n,t-1}$) should alleviate pressures to raise prices. Ghosh et al. (2002) also introduce a dummy for pegged exchange rate regimes ($FIX_{n,t-1}$) and find that these regimes presented lower average inflation worldwide.[11]

<Insert **Table 2** around here>

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[10] See Mehra (2004), Andersen and Walsher (1999), or Hutchison and Walsh (1998) for similar adaptive Phillips curves applied to one-country time series data.

[11] Another way to get an equation similar to (6), actually the one used by Ghosh et al. (2002), is to start with an aggregate demand in term of growth rates $\pi_t = BMG_{t-1} - GDPPCG_{t-1}$, and then to add other variables suggested by the literature. Compared to equation (6), Ghosh et al. (2002) do not include lagged inflation. I have also estimated versions of equation (6) without the lagged inflation and have found similar results in terms of IT effectiveness. Results can be provided upon request.

The first 4 columns of Table 2, show estimates for sample B22-8499 (described in Table 1, column (1)). This is the sample that gets more attention in this paper, not only because it was the sample used by LY, to whom I want to compare, but also because it is a balanced panel that covers the inception of the IT system. After 1999, 10 of these 22 industrial countries unified their monetary policy under the European Central Bank, what makes it debatable if they should be analyzed as independent individuals from then on.

Using MSH's IT adoption dates, I present estimates by pooled cross-sectional OLS in columns (1), by including the common time-variable effect in column (2), and the common-time-variable and country-fixed-effects in column (3). From the changes in coefficient estimates among these 3 columns, we realize that the common-time-variable and country-fixed effects do affect inflation. The IT impact is already significant in the simplest model in column (1). Although IT becomes less important with the inclusion of the common-time effect, it reaches its highest size and significance when the country-fixed effects are included, showing that non-observables play a role in the inflation process. Both the direct impact of IT on inflation, β^{π} , and its cumulative effect, $\beta^{\pi}/(1-\alpha^{\pi})$, which accounts for subsequent decreasing over-time lagged inflation on the right-hand side, are significant.

Columns (3) to (9) of Table 2 present panel estimates with time and fixed-effects for the various samples and two authors' calendars of IT adoption described in Table 1. To check how important it is to vary the dates of IT adoption, from column (3) to (4), I keep sample B22-8999 and change from MSH's to BS's IT calendar, the one used by LY. BS's IT adoption dates differ from MSH's in relation to Australia (1994 instead of 1995), Canada (1992 instead of 1991), Finland (1994 instead of 1993) and United Kingdom

(1993 instead of 1992). The different calendars of IT adoption make a quantitative difference, but not important enough to change the qualitative result that IT mattered. BS's IT adoption dates reduces the IT direct impact on the average inflation level from -1.37 to -1.01 percentage points, or its implied cumulative effect from -2.28 to -1.68 percentage points. Anyway, the IT effect is still significantly important, different from LY's irrelevance conclusion (in their Table 3, Panel A), and differences between columns (3) and (4) mainly reflect that my identification strategy of one-year lag in policy effects matches better with MSH's calendar.

The balanced panel B18-8499, used in column (5), excludes Greece, Iceland, New Zealand and Portugal, to confirm that the significant IT impact is not the deceiving consequence of relatively high-inflation countries in the control group. In column (6), the balanced panel B22-7199 extends B22-8499 back to 1971 to reduce the dynamic panel bias problem suspicion and to check if the IT effect is still important when observed in a longer retrospect. In both columns (5) and (6), the direct effect of IT is around -0.8 percentage points, lower than the -1.4 percentage points from column (3), but is still significant at respectively 1% and 6% significance levels.

Columns (7) to (9) estimates are based on unbalanced panels of 20 countries, excluding Greece and Iceland. The remaining nine Euro members are also excluded after 1998, as they have not had independent national monetary policies since then. The data used in columns (7) and (8) resemble BS's sample, excluding Norway after 2000 and Switzerland after 1999. The results in column (8) are even more comparable to BS's findings, given BS's IT adoption dates are used in it. From the comparison of columns

(7) and (8), again it becomes clear that the combination of the one-year lag in policy effects with BS's calendar captures less of the IT effect than MSH's calendar.

In column (9), the non-Euro countries are observed until 2009, with Norway and Switzerland joining the ITers group in 2001 and 2000 respectively. Although smaller than the estimated value for data until 2001, in column (7), the IT coefficient presents significant enhancing effects on inflation until 2009, meaning that the average disinflation gain of the IT policy is still noticeable many years after its introduction. Among many, one possible explanation for the reduction in size of the IT effect is that non-explicit IT Central Banks have incorporated IT best practices during the 2000s.

Regarding the statistics of goodness of fit, in accordance with the assumptions, there is no evidence of first-order autocorrelation of the residuals in the equations tested in levels (OLS and TE-OLS, in columns (1) and (2)), or of second-order autocorrelation in the equations tested in differences (CTE-OLS, in columns (3)-(9)), respectively represented by the p-values of the AR(1) and AR(2) test statistics. Besides weakening suspects that the significances found are the spurious result of auto-correlated residuals, these tests show that the one-year period frequency suits the AR(1) model parameterization well.

In case one agrees that the set of variables controlled for by LY is just the right set of covariates, their inclusion in regression (3) is enough to interpret the IT-inflation regression relation as a causal one as well. In the inflation-output tradeoff context of the Phillips curve, this IT effect is what monetary economists call the "credibility bonus" of IT, or the "confidence effect" of IT. The Central Bank governor turnover rate is only significant in the 1985-2001 unbalanced panels, but even there it does not obviate the IT effect.[12] If $CBTURN5_{n,t-1}$ is a good proxy for Central Bank independence, the results in Table 2 allow the conclusion that IT was effective in lowering inflation, and its impact was in addition to Central Bank independence.

Yet in columns (3) to (9) of Table 2, it is noticeable that besides lagged inflation, per capita output growth and terms of trade growth, usual characters in an adaptive open economy Phillips curve, the IT dummy is the only variable that is significant in all seven samples. The variables for exchange rate regimes, trade openness and broad money growth were rarely significant. The government balance surplus ($CGGDP_{n,t-1}$) is significant for the samples that cover the period 1984-1999, but with a positive sign that is opposite to expected; and it becomes insignificant in column (6), which just extends the same vintage of data back to 1971.

In Table 3, I exclude the variables that were not useful in predicting inflation in Table 2, and step back to a more parsimonious adaptive Phillips curve, where:

$$\begin{aligned}\pi_{n,t} = \alpha^{\pi} \cdot \pi_{n,t-1} + \beta^{\pi,IT} \cdot d^{IT}_{n,t-1} + \gamma^{\pi}_{cbturn} \cdot CBTURN5_{n,t-1} \\ + \gamma^{\pi}_{gdppcg} \cdot GDPPCG_{n,t-1} + \gamma^{\pi}_{ttg} \cdot TTG_{n,t-1} \\ + \delta^{\pi}_{t} + \eta^{\pi}_{n} + \upsilon^{\pi}_{n,t};\end{aligned} \quad (7)$$

with the $CBTURN5_{n,t-1}$ kept in the equation to isolate the IT regime from other observable differences at the Central Bank.[13]

[12] The significance of CBTURN5 in sample U20-8501 might be the consequence of two facts. First, as described in the data section, the variable CBTURN5 used in this sample is not a simple extension of Ghosh et al. (2002), but a revised series. Second, Iceland, which presented a relatively high inflation and low governor turnover, is not included.

[13] I am aware that, in the sensible case that successful IT adoption increases Central Bank independence and Central Bank independence decreases inflation, the inclusion of CBTURN5 in an equation to measure

<Insert **Table 3** around here>

Keeping the pattern of Table 2, the IT effects change slightly in Table 3, usually becoming stronger and more significant. In columns (1) to (3), I again show how non-observables affect the inflation process. Over again, different from LY's irrelevance conclusion, column (4) presents significant IT accomplishments. The IT effects strengthen considerably in columns (6), (8) and (9), emphasizing that, in column (8), the significant IT action contrasts with BS's irrelevance conclusion (in their Table 6.3). As in Table 2, the $CBTURN5_{n,t-1}$ is significant only in sample U20-8501, although its effect is weaker here.

BS and LY have both made a distinction between "non-constant" ITers, defined as every country that explicitly adopts the IT regime, independent of their target being variable or constant; and "constant" ITers, defined as countries that have an explicit constant target for inflation. While their measure of the "non-constant" IT policy follows the same logic as that of my general IT policy $d_{n,t}^{IT}$, the way they measure the "constant" IT effect seems unfair, given the convergent stage of all ITers (i.e. the years ITers were lowering inflation targets to their long-term goals) is mixed in the control group. Hence, BS and LY mistakenly mingle the successful convergent stage of IT in the control group average, biasing the effect of the constant IT stage to zero. Algebraically speaking, with $d_{n,t}^{IT}$ representing the "non-constant" IT, and defining $d_{n,t}^{CIT}$ to represent the "constant"

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the effect of IT on inflation will generate a selection bias. However, because Central Banks that reached high levels of independence without inflation targeting are probably more reliable, they should generate lower than average inflation. Hence, this potential selection bias would be positive and make it more difficult to find a dampening down effect of IT on inflation.

IT, we should isolate the effect of convergent IT, $d_{n,t}^{CvgIT} = \left(d_{n,t}^{IT} - d_{n,t}^{CIT}\right)$, from the control group, before we measure the right effect of "constant" IT:

$$y_{n,t} = \alpha^{y} \cdot y_{n,t-1} + \beta^{y,CvgIT} \cdot d_{n,t-1}^{CvgIT} + \beta^{y,CIT} \cdot d_{n,t-1}^{CIT} + \gamma_{cbturn}^{y} \cdot CBTURN5_{n,t-1} + \delta_{t}^{y} + \eta_{n}^{y} + \upsilon_{n,t}^{y} \,. \tag{8}$$

In Tables 4 and 5, instead of computing the IT result on the inflation-output tradeoff, I estimate the IT achievements on inflation and output growth separately, both on levels and on volatilities, and differentiate between convergent and constant IT effects, as in equation (8), obtaining some results that are comparable to BS's and LY's tests.

<Insert **Tables 4** and **5** around here>

Table 4 shows estimates for the dynamics of $y_{n,t} = \{\pi_{n,t}, \sigma\pi_{n,t}\}$, with both MSH's and BS's IT calendars. Columns odds and evens make a distinction between the general IT effect $\beta^{y,IT}$ and its two compounding stages achievements $\left(\beta^{y,CvgIT}, \beta^{y,CIT}\right)$. For example, comparison of columns (1) to (2) show that $\beta^{\pi,IT}$ is an average of $\left(\beta^{\pi,CvgIT}, \beta^{\pi,CIT}\right)$, and by isolating the very significant IT action in the convergent stage $\beta^{\pi,CvgIT}$ from the control group, the IT accomplishment in the constant stage $\beta^{\pi,CIT}$ is also significant with MSH's IT calendars. The inflation standard deviations in columns (3) and (4) were not as affected by IT as the levels. However, the fact that IT did not cause an increase in volatility is actually an evidence of IT improvement in macroeconomic performance, if we take into account that inflations were going down during the period studied, and thus going through unavoidable volatility due to the change in mean. The fact that the dampening down effects of IT on inflation volatilities were stronger during the constant IT stage in column (4) than during the converging stage in column (3), also strengthens the results with economic sense. In columns (5) to (8) of Table 4, with BS's calendar, I get closer to LY's exercises once more. But different from them (in their Tables 3 and 4), I find significant IT effects on inflation levels for the general "non-constant" IT $\beta^{\pi,IT}$ (in my column (5)), concentrated in the convergent stage $\beta^{\pi,CvgIT}$ (in my column (6)), and on inflation volatility for the "constant" IT $\beta^{\sigma\pi,CIT}$ (in my column (8)).

Except for the inclusion of $CBTURN5_{n,t-1}$, Mishkin and Schmidt-Hebbel (2007) adjust a common-time and country-fixed-effect AR(1) to a 1989-2004 inflation panel of industrial countries similar to mine. However, they use quarterly frequency data and give no lag for IT to take effect. The facts that, at quarterly frequency, it seems necessary to adjust an AR(4) model to fade the residuals autocorrelation away, and that one quarter is a too short time interval for inflation to react to monetary policy, might explain why they get insignificant results for the IT regime among industrial economies (in column 1 of their Table 7).

Because the inflation-output tradeoff is inexorable on making sense of the short-run effects of monetary policy, columns (1) to (8) of Table 5 follow the same samples order of Table 4 to present estimates of equation (8) for output growth levels and volatilities $y_{n,t} = \{GDPPCG_{n,t}, \sigma GDPPCG_{n,t}\}$, with the warning of section 3 that the effects of IT on output growth levels might be biased upwards. In addition to the enhancing performance on inflation just shown, it seems that IT had a positive impact on output growth levels in columns (1)-(2) and (5)-(6), without much difference between the convergent and constant stages. However, because positive CTE-OLS estimates of the IT effect on output might be biased, due to $\text{cov}\left(d_{n,t-1}^{IT}, \upsilon_{n,t-j}^{GDPPCG}\right) \leq 0$ (to be shown in Table 7 below), columns (9) and (10) present TE-OLS estimates of $\beta^{GDPPC,IT}$ for MSH's and BS's adoption dates respectively. Although the IT consequence becomes insignificant, it is still positive for the output growth levels, and the output growth standard deviations in columns (3)-(4) and (7)-(8) did not increase either.

In sum, taking into account that ITers inflations were going down during this period, the facts that IT did not cause a decrease in output growth or increases in the volatilities of inflation and output growth confirm the IT effectiveness in improving macroeconomic performance. There was a confidence bonus for IT adoption, paid both in inflation reduction and in higher output growth, which makes sense, given most of ITers started their regimes already on target or reached it fast, leaving some slack for output growth.[14]

4.2. Ball and Sheridan (2005) revisited

In Tables 6.A and 6.B, I revisit BS's 20 countries cross-section difference-in-difference regressions respectively for annual inflation and GDP growth rates, using their

[14] It should be noted that these findings are not contradictory with Brito (2010) that, in a debate with Gonçalves and Carvalho (2010), shows that IT disinflations were not less costly in the OECD. They discuss about Ball's (1994) disinflations, i.e. events in which the nine-quarter moving average of inflation went down by more than 2%, when it is better to minimize the output sacrifice-ratio $\left(1/\gamma_{gdppcg}^{\pi}\right)$, in equations (6) or (7). However, to lower the output sacrifice ratio is not a state-independent goal. For example in expansions, the ideal is to have the highest output sacrifice-ratio. Here, because I am interested in the continuing inflation-output tradeoffs and to drawn the conclusion of superior policy performance, it is necessary to evaluate the average effect of the IT regime during the whole treatment (both in contractions and in expansions).

data. Columns (1) and (5) of my Tables 6.A and 6.B, respectively, reproduce columns (6) and (8) of their Tables 6.3.B and 6.6.B, according to my equation (2).

After summarizing quarterly inflation and annual GDP growth rates data into pre- and post-treatment averages, BS's cross-section set-up ends up making inferences from a sample of just 20 observations, which raises small sample robustness doubts. In such small sample, results can change from insignificant to significant depending on how one computes the standard errors. As I show in my Tables 6, the robust standard errors in brackets are smaller than the conventional ones in parentheses, which is a sign of finite-sample bias in the calculations.

<Insert **Table 6** around here>

Also because of the small sample, results are highly influenced by a few outliers. Using the graphical tool proposed by Rousseeuw and Van Zomeren (1990), I detect that Portugal and New Zealand should have been treated as outliers because their pre-IT inflations were much higher than others.[15] On re-estimating BS's cross-section regressions with a robust technique (the MM estimator by Yohai (1987)) in column (2), or including a dummy for these countries past-period high inflation in column (3), or just dropping them out of the sample in column (4), I am able to show that IT significantly lowered inflation among industrial countries in BS's sample, without compromising output growth, which also showed positive result, although not significant.

Regarding the constant IT stage evaluation, presented in columns (5) to (8), the above pointed problem of biasing the constant IT stage coefficient $\beta^{\pi,CIT}$ to zero, by mingling the convergent IT stage accomplishments in the control group, is insoluble in

[15] Note that Greece and Iceland are not part of BS sample, or they would be categorized as leverage points too.

the cross-section set up. But even there, IT brings significant enhancing effects for inflation-output tradeoff when looked at simultaneously in Tables 6.A and 6.B. While columns (6) show a significant increase in GDP growth together with an insignificant decrease in inflation, columns (7) and (8) present a significant decrease in inflation together with an insignificant increase in GDP growth.

4.3. Lin and Ye (2007) revisited

As explained in section 3, and demonstrated by replication in the Appendix B, LY's PSM application to a panel is not standard. Instead of one unit of treatment per IT country (7 countries in their case), they consider every year-country under IT as a independent unit going under treatment (totaling 45 events), match those units by their contemporaneous year observable characteristics, instead of by their pre-treatment characteristics, and base their evaluation on that same year-inflation performance, $E\left[y^1_{ni,t'} \mid d^{IT}_{ni,t'}=1,\ p\left(y_{ni,t'-1}, X_{ni,t'}\right)\right] - E\left[y^0_{n,\tau} \mid d^{IT}_{n,\tau}=0,\ p\left(y_{n,\tau-1}, X_{n,\tau}\right)=p\left(y_{ni,t'-1}, X_{ni,t'}\right)\right]$ for all t' and τ, including $t'>k$. The fact that current ITers have (or have not) current lower inflations does not imply that a country is more likely (or is not likely) to lower inflation as it becomes an ITer. By violating the CIA, LY's results are subject to the selection bias they propose to solve. Furthermore, the IT policy evaluation also has to account for the short-run inflation-output tradeoffs involved, which LY do not examine either.

<Insert **Table 7** around here>

In Table 7, I reformulate LY's Probit model of IT adoption, fulfilling the requirements of: (i) one unit of treatment per ITer country and (ii) match on observables before treatment, but keeping their panel structure and the set of covariates. To calculate the probability of IT adoption, instead of the probability of being an IT (calculated by LY in their Table 2 and replicated in my Table B.1), ITers observations are included in the Probit estimation only if the country had not adopted IT previously, $\tau \le k_{ni}$, i.e. ITers observations are not used in the Probit estimation after the first year of treatment, $t' > k_{ni}$. To assure exogeneity, all seven observable variables controlled for by LY are lagged in one year, $p\left(y_{n,\tau-1}, X_{n,\tau-1}\right)$ instead of $p\left(y_{n,\tau-1}, X_{n,\tau}\right)$. Finally, because when matching at horizon h , I need the outcome variable to exist at date $(\tau + h)$ and have to avoid that ITers outcomes already under treatment become controls for themselves, I drop the observations of all countries dated $\tau > (T - h)$ and of pre-treatment ITers' years from which the term h ends up in their treatment periods. These two latter cautions generate a different subsample and its respective Probit for each horizon h.[16]

From sample B22-8499 combined with BS's IT calendar (my equivalent of LY's sample), where the last IT adoptions happened in 1995, it is possible to compute the ATT_h for $h=0, 1, 2, 3, 4$. In Table 7, I present the general IT adoption Probit estimates, $E\left[d_{n,\tau}^{IT} \mid y_{n,\tau-1}, X_{n,\tau-1}\right] = p\left(y_{n,\tau-1}, X_{n,\tau-1}\right)$, for each of these five subsamples. In the Probits, the explanatory variable last-year inflation has a negative and significant impact on IT adoption, meaning that countries with lower inflation are more probable of adopting IT in all subsamples. Among the seven observables studied by LY, this was the only one they lagged and is the only one that has preserved its sign and significance (see Table B.1 in

[16] For illustrative purposes, take as examples the sample B22-8499, the inflation difference two years after IT adoption $ATT_2 = E\left[y_{n,k+2}^{1} \mid d_{n,k}^{IT} = 1,\ p\left(y_{n,k-1}, X_{n,k-1}\right)\right] - E\left[y_{n,\tau+2}^{0} \mid d_{n,\tau}^{IT} = 0,\ p\left(y_{n,\tau-1}, X_{n,\tau-1}\right)\right]$, and Sweden's IT adoption in year $k_{ni}=1995$. Because $y_{n,\tau+2} \mid p\left(y_{n,\tau-1}, X_{n,\tau-1}\right)$ does not exist in the sample for τ=*1998 and 1999* , all countries observations for those years are dropped. Given Sweden's just-before-treatment observables in $(k_{ni}\text{-}1)=1994$ are similar to Sweden's observables in $(\tau\text{-}1)=1993$ *and 1992* , the latter two Sweden's years are good control matches for the former. However, $(\tau+2)=1996$ *and 1995* are already treatment years for Sweden, and should not be used as controls.

the Appendix B, or LY's Table 2). Compared to LY's Table 2, broad money growth, central bank turnover and pegged exchange rate regimes are now insignificant. Government balance surplus becomes negative and significant, with the non-intuitive implication that IT adopters have previous-year looser fiscal policies.[17]. More different, and with an important implication, is that per capita GDP growth changes its sign to negative and significant in most of the subsamples, meaning that countries with weaker output growth are also more probable to adopt IT. Although PSM protocols would suggest the exclusion of observables that do not influence IT adoption or inflation, given my objective in this subsection is just to review LY's results, I keep all of them to the end.

<Insert **Table 8** around here>

To check for the balancing of the covariates in the 5 subsamples, in Table 8, I present t-tests of differences between treated and control groups, matched by the same seven methods used in LY. From the unmatched, column (1), one sees that the concern that lower post-IT inflation may be caused by lower pre-IT inflation does make sense. A similar concern applies to per-capita GDP growth, given lower post-IT inflation may be justified by the lagged effects of lower pre-IT growth. As shown in Table 8, after match, treated and control groups are similar with respect to the seven observables by any of the six matching methods (except for the Local linear regression), and thus comparable. [18]

[17] The negative coefficient for L.CGGDP is basically driven by Finland and Sweden, which ran central government deficits above 10% of the GDP in the year before their IT adoptions.

[18] *pstest* does not make sense in the Local linear regression matching (in column 7) because it first smooths the outcome and then performs Nearest neighbor matching, using more controls to calculate the counterfactual outcome than the nearest neighbor only. They are shown in Table 8 just for completeness in results presentation.

As stated before, another criticism to LY is that on making sense of the short-run effects of monetary policy, there is an inflation-output tradeoff to be chosen by the Central Banker. For ITers already on their inflation targets, or below targets – and those cases happened according to Johnson (2002) – the IT confidence bonus can translate into loose demand policies and higher short-term output growth. With this perspective of the monetary policy effects, I present estimates of the general IT policy $d_{n,t}^{IT}$ for inflation and output growth differences in Tables 9 and 10. Panels A to E show the average treatment effect, ATE_h, in column (1), and the average treatment effect on treated, ATT_h for $h=0, 1, 2, 3, 4$, in columns (2)-(8), calculated according to equation (3). Panel F presents the 5-year averages $\overline{ATE}_5$ and $\overline{ATT}_5$, calculated according to equation (4).

<Insert **Tables 9** and **10** around here>

From Tables 9 and 10, in panels A to E, columns (2) to (8), the fact that the point estimates of inflation and output growth differences are almost always favorable to ITers against non-ITers is convincing. Overall, the ITers performed somewhat better than the non-ITers in most of the cases and significantly better in some cases. The *ATT*s for inflation are negative in 33 out of 35 matches in Table 9, and the *ATT*s for output growth are positive in 30 out of 35 matches in Table 10. If one looks at the 5-year average $\overline{ATT}_5$, calculated from equation (4) in panels F, to infer about the effects of monetary policy on the inflation-output tradeoff, all pairs of inflation-output growth differences have a positive-negative sign, respectively meaning that lower inflation was achieved with higher growth, an impressive result from the Phillips curve perspective of short-run positive inflation-output growth tradeoff. Except for the "Local linear regression", ITers presented an average inflation around 1 percent point lower and/or an average output growth one percent point higher in the first five-years of IT treatment, with one, if not both, of the outcome differences significant at 99% confidence level. [19]

5. Conclusion

In a comprehensive survey on the empirical evidence on IT, Walsh (2009) concludes that "… macroeconomic experiences among both inflation targeting and non-targeting developed economies have been similar", a puzzlingly contrast with policy makers enthusiasm about the system. If it is possible that recent IT adoptions by emerging economies have been pushed by fashion in the profession, at least at its inception, IT should have improved monetary policy to justify pioneering IT central bankers satisfaction and international agencies support.

With the goal of answering the question "Did pioneering IT regimes make a difference in industrial countries' short-run macroeconomic outcomes?", I have revisited industrial countries data studied by BS, LY, and some sample extensions. Through a dynamic panel that controls for observables and non-observables covariates, estimated at annual frequency, I find significant evidence that IT positively mattered for pioneering industrial ITers, meaning that IT conquered lower inflation without compromising output. These IT achievements survive to several kinds of controls, to samples variations and to the different IT dating calendars attempted.

[19] Like in the cross-section difference-in-difference context, measures of the constant-IT stage are biased to zero in the PSM because the convergent-IT stage accomplishments are mingled in the control group, which is the reason why I only analyze the general IT policy. Also not to make this paper even lengthier, I do not present the results for sample B22-8499 combined with MSH's IT calendar. As in the dynamic panel case, these latter results are even more favorable to IT, and can be provided upon request.

I also clarify that the solution to the criticism of selection bias in policy adoption is not in the choice of PSM over linear regression, but in the control for the right set of covariates. The assumption that validates causality tests in PSM methods and in linear regression is the same. Once the conditional independence assumption (CIA) holds, given linear regression also approximates the conditional expectation function (CEF), differences in the estimated treatment effects by the two approaches should be minor. Because the PSM does not achieve identification if there are non-observables that explain treatment reception and the outcome variable, and this seems to be the case of IT adoption and inflation, the dynamic panel regression with common-time and country-fixed effects seems more suitable to the context.

I finally demonstrate that BS's results are sensitive to outliers, and that LY have violated the exogeneity assumption. If one uses a robust to outliers technique in BS's sample, or reformulates LY's PSM to assure exogeneity, their IT irrelevance results are overturned, concluding that IT did make an enhancing difference in industrial countries' inflation and output growth at IT inception.

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Table 1 - Countries studied and dates of inflation targeting adoption

Country:	Sample name and period:					Year of inflation targeting adoption according to:			
						Mishkin and Schmidt-Hebbel (2007)		Ball and Sheridan (2005)	
	B22-8499	B18-8499	B22-7199	U20-8501	U20-8009	Converging-IT adoption date:	Constant-IT adoption date:	Converging-IT adoption date:	Constant-IT adoption date:
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Australia	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009	1995	1995	1994	1994
Austria	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Belgium	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Canada	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009	1991	1995	1992	1994
Denmark	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009				
France	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Finland	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998	1993	1993	1994	1994
Germany	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Greece	1984-1999		1971-1999						
Iceland	1984-1999		1971-1999			2001	2003		
Ireland	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Italy	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998				
Japan	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009				
Netherlands	1984-1999	1984-1999	1971-1999	1985-1998	1980-2009				
New Zealand	1984-1999		1971-1999	1985-2001	1980-2009	1990	1993	1990	1993
Norway	1984-1999	1984-1999	1971-1999	1985-2000	1980-2009	2001	2001		
Portugal	1984-1999		1971-1999	1985-1998	1980-1998				
Spain	1984-1999	1984-1999	1971-1999	1985-1998	1980-1998	1995		1995	
Switzerland	1984-1999	1984-1999	1971-1999	1985-1999	1980-2009	2000	2000		
Sweden	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009	1995	1995	1995	1995
United Kingdom	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009	1992	1992	1993	1993
United States	1984-1999	1984-1999	1971-1999	1985-2001	1980-2009				

Note: See section 2 (Data) for more details.

Table 2 - Estimates of the controlled adaptive Phillips curve equation (3) - rates in %

Sample and authors' dates of IT	B22-8499 MSH			B22-8499 BS	B18-8499 MSH	B22-7199 MSH	U20-8501 MSH	U20-8501 BS	U20-8009 MSH
Estimator:	OLS	TE-OLS	CTE-OLS	CTE-OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.Inflation targeting	-0.78***	-0.27	-1.37***	-1.01**	-0.78***	-0.77*	-0.98***	-0.58*	-0.68***
	(0.24)	(0.24)	(0.35)	(0.38)	(0.19)	(0.40)	(0.23)	(0.30)	(0.14)
L.Central bank turnover	0.81	1.03	2.00	1.94	0.23	-0.05	1.56**	1.46**	0.52
rate	(1.47)	(1.38)	(1.66)	(1.67)	(0.42)	(1.19)	(0.66)	(0.64)	(0.49)
L.Pegged regimes	0.53	0.51	0.51	0.52	0.80	0.80	0.58	0.59	0.37
	(0.36)	(0.35)	(1.31)	(1.34)	(0.56)	(0.53)	(0.64)	(0.79)	(0.29)
L.Trade openness	-0.01***	-0.01**	0.03	0.03	-0.01	-0.03	0.02	0.01	0.01
	(0.00)	(0.00)	(0.03)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
L.Money growth	0.00	-0.01	-0.03	-0.03	0.04**	0.01	0.01	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
L.Government balance	0.01	0.01	0.10**	0.11**	0.07*	-0.01	0.02	0.02	0.01
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)	(0.03)	(0.03)	(0.03)
L.Terms of trade growth	-0.04	-0.04	-0.06*	-0.06*	-0.06***	-0.04*	-0.06***	-0.06***	-0.03**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
L.GDP per head growth	0.19**	0.18**	0.14**	0.14**	0.09***	0.15***	0.19***	0.20***	0.16**
	(0.07)	(0.08)	(0.07)	(0.07)	(0.02)	(0.04)	(0.06)	(0.06)	(0.06)
L.Inflation	0.54***	0.53***	0.40***	0.40***	0.52***	0.51***	0.56***	0.57***	0.57***
	(0.09)	(0.09)	(0.11)	(0.11)	(0.06)	(0.11)	(0.06)	(0.06)	(0.05)
L.High inflation regime	7.71***	7.63***	8.66***	8.60***		3.35***			5.65***
	(1.95)	(1.94)	(2.03)	(2.07)		(0.86)			(1.55)
AR(1) test	0.71	0.71	0.11	0.11	0.00	0.06	0.00	0.01	0.00
AR(2) test	0.50	0.52	0.81	0.82	0.33	0.23	0.81	0.85	0.21
ICE of IT	-1.69***	-0.57	-2.28***	-1.68***	-1.63***	-1.56*	-2.21***	-1.33*	-1.58***
Observations	329	329	329	329	269	595	275	275	442
Countries	22	22	22	22	18	22	20	20	20
Adj. R2	0.81	0.83	0.76	0.76	0.74	0.82	0.68	0.67	0.86

Notes: Samples and authors' dates of IT adoption are described in Table 1 and in section 2 (Data). MSH columns use Mishkin and Schmidt-Hebbel's (2007) IT dating, while BS columns use Ball and Sheridan's (2005) IT dating. The data frequency is annual and $L.Z=Z_{t-1}$ means 1-year lag in relation to the dependent variable ($Inflation_t$). Pooled cross-sectional OLS (OLS) in column (1), including time-variable effect (TE-OLS) in (2), and time and country effects (CTE-OLS) in columns (3)-(9), with robust standard errors clustered by country in parentheses. *, ** and *** indicate the significance level of 10%, 5%, and 1%, respectively. AR(1) and AR(2) respectively report the p-values of tests for 1st- and 2nd-order serial correlation. These test the levels residuals for columns (1)-(2) and first-differenced residuals in all other columns. ICE means Implied Cumulative Effect equal to IT $dummy_{t-1}/(1\text{-}Inflation_{t-1})$, with significance tested by a nonlinear test. Column (6) additionally includes a not reported dummy variable for annual inflation rate above 40% (i.e., only Iceland before 1983).

Table 3 - Estimates of the parsimonious adaptive Phillips curve equation (4) - rates in %

Sample and authors' dates of IT	B22-8499 MSH			B22-8499 BS	B18-8499 MSH	B22-7199 MSH	U20-8501 MSH	U20-8501 BS	U20-8009 MSH
Estimator:	OLS	TE-OLS	CTE-OLS	CTE-OLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.Inflation targeting	-0.81***	-0.31*	-1.26***	-0.95**	-1.17***	-1.09***	-1.05***	-0.72**	-0.99***
	(0.23)	(0.15)	(0.30)	(0.35)	(0.19)	(0.25)	(0.18)	(0.26)	(0.24)
L.Central bank turnover rate	0.87	1.02	1.56	1.53	0.23	-0.01	1.27*	1.21*	0.92
	(1.41)	(1.30)	(1.63)	(1.65)	(0.48)	(1.22)	(0.63)	(0.61)	(0.58)
L.Terms of trade growth	-0.03	-0.04	-0.06*	-0.06*	-0.05**	-0.03	-0.05***	-0.05***	-0.03**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)
L.GDP per head growth	0.17**	0.15**	0.16**	0.16**	0.11***	0.12***	0.22***	0.22***	0.16***
	(0.06)	(0.07)	(0.07)	(0.07)	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)
L.Inflation	0.55***	0.53***	0.39***	0.39***	0.57***	0.51***	0.62***	0.63***	0.52***
	(0.10)	(0.10)	(0.12)	(0.12)	(0.04)	(0.11)	(0.04)	(0.04)	(0.05)
L.High inflation regime	7.47***	7.37***	8.39***	8.32***		3.18***			6.72***
	(1.82)	(1.82)	(2.00)	(2.04)		(0.89)			(1.86)
AR(1) test	0.72	0.73	0.09	0.09	0.00	0.06	0.01	0.01	0.00
AR(2) test	0.46	0.47	0.93	0.96	0.43	0.23	0.59	0.63	0.12
ICE of IT	-1.79***	-0.66*	-2.06***	-1.56***	-2.72***	-2.25***	-2.74***	-1.94**	-2.07***
Observations	330	330	330	330	270	616	287	287	470
Countries	22	22	22	22	18	22	20	20	20
Adj. R2	0.81	0.82	0.75	0.75	0.72	0.82	0.70	0.69	0.84

Notes: Samples and authors' dates of IT adoption are described in Table 1 and in section 2 (Data). MSH columns use Mishkin and Schmidt-Hebbel's (2007) IT dating, while BS columns use Ball and Sheridan's (2005) IT dating. The data frequency is annual and $L.Z=Z_{t-1}$ means 1-year lag in relation to the dependent variable ($Inflation_t$). Pooled cross-sectional OLS (OLS) in column (1), including time-variable effect (TE-OLS) in (2), and time and country effects (CTE-OLS) in columns (3)-(9), with robust standard errors clustered by country in parentheses. *, ** and *** indicate the significance level of 10%, 5%, and 1%, respectively. AR(1) and AR(2) respectively report the p-values of tests for 1st- and 2nd-order serial correlation. These test the levels residuals for columns (1)-(2) and first-differenced residuals in all other columns. ICE means Implied Cumulative Effect equal to IT $dummy_{t-1}/(1-Inflation_{t-1})$, with significance tested by a nonlinear test. Column (6) additionally includes a not reported dummy variable for annual inflation rate above 40% (i.e., only Iceland before 1983).

Table 4 - Common-time and country-fixed effects OLS (CTE-OLS) estimates of the annual inflation rate (in %) equation - sample B22-8499

	Authors' dates of IT adoption:							
	MSH				BS			
Dependent variable:	Levels		Standard deviations		Levels		Standard deviations	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
L.Inflation targeting	-1.22***		-0.31		-0.86**		-0.41	
	(0.33)		(0.28)		(0.39)		(0.25)	
L.Convergent IT stage		-1.61***		-0.02		-1.40***		-0.21
		(0.40)		(0.38)		(0.42)		(0.37)
L.Constant IT stage		-1.06***		-0.45*		-0.67		-0.48*
		(0.36)		(0.25)		(0.40)		(0.24)
L.Central bank turnover rate	1.59	1.56	0.31	0.35	1.56	1.53	0.28	0.30
	(1.61)	(1.60)	(0.23)	(0.25)	(1.62)	(1.61)	(0.23)	(0.23)
L.Dependent variable	0.37***	0.37***	0.50***	0.49***	0.37***	0.37***	0.50***	0.49***
	(0.12)	(0.12)	(0.10)	(0.10)	(0.12)	(0.12)	(0.10)	(0.10)
L.High inflation regime	8.47***	8.47***	0.78**	0.81**	8.40***	8.39***	0.79**	0.81**
	(2.12)	(2.13)	(0.37)	(0.36)	(2.17)	(2.17)	(0.37)	(0.36)
AR(1) test	0.10	0.10	0.06	0.06	0.10	0.09	0.05	0.05
AR(2) test	0.82	0.82	0.75	0.69	0.85	0.84	0.79	0.75
ICE of IT	-1.95***				-1.38**			
ICE of constant IT		-1.69***				-1.07*		
Observations	330	330	286	286	330	330	286	286
Adj. R2	0.73	0.73	0.52	0.53	0.73	0.73	0.53	0.53

Notes: The sample is a balanced panel of 22 industrial countries between 1984-1999, B22-8499, described in Table 1 and in section 2 (Data). MSH columns use Mishkin and Schmidt-Hebbel's (2007) IT dating, while BS columns use Ball and Sheridan's (2005) IT dating. The data frequency is annual and $L.Z=Z_{t-1}$ means 1-year lag in relation to the dependent variable (y_t = $Inflation_t$ or $Standard\ deviation\ of\ inflation_t$). Pooled cross-sectional OLS including time and country effects (CTE-OLS) in all colums, with robust standard errors clustered by country in parentheses. *, ** and *** indicate the significance level of 10%, 5%, and 1%, respectively. AR(1) and AR(2) respectively report the p-values of tests for 1st- and 2nd-order serial correlation of first-differenced residuals. ICE means Implied Cumulative Effect equal to IT $dummy_{t-1}/(1-Inflation_{t-1})$, with significance tested by a nonlinear test.

Table 5 - Estimates of the per capita GDP annual growth rate (in %) equation - sample B22-8499

Estimator:	CTE-OLS								TE-LS	
Authors' dates of IT:	MSH				BS				MSH	BS
Dependent variable:	Levels		Standard deviations		Levels		Standard deviations		Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
L.Inflation targeting	1.26**		-0.17		1.09**		-0.24		0.42	0.31
	(0.47)		(0.21)		(0.48)		(0.26)		(0.34)	(0.35)
L.Convergent IT stage		1.27***		-0.33***		1.24**		-0.26		
		(0.45)		(0.12)		(0.49)		(0.19)		
L.Constant IT stage		1.25**		-0.10		1.04*		-0.24		
		(0.55)		(0.27)		(0.56)		(0.31)		
L.Central bank turnover rate	0.56	0.56	1.26	1.24	0.62	0.63	1.24	1.23	0.19	0.16
	(1.13)	(1.12)	(1.19)	(1.18)	(1.15)	(1.14)	(1.20)	(1.20)	(0.77)	(0.79)
L.Dependent variable	0.21**	0.21**	0.47***	0.47***	0.21**	0.21**	0.47***	0.47***	0.33**	0.32**
	(0.09)	(0.09)	(0.04)	(0.04)	(0.09)	(0.09)	(0.04)	(0.04)	(0.13)	(0.13)
L.High inflation regime	-0.17	-0.17	1.12***	1.11***	-0.13	-0.13	1.13***	1.13***	-0.53	-0.53
	(0.27)	(0.27)	(0.34)	(0.34)	(0.28)	(0.28)	(0.33)	(0.33)	(0.32)	(0.32)
AR(1) test	0.10	0.10	0.20	0.20	0.10	0.10	0.20	0.20	0.69	0.68
AR(2) test	0.08	0.08	0.64	0.64	0.08	0.08	0.62	0.62	0.58	0.56
ICE of IT	1.59***				1.38**				0.62	0.46
ICE of constant IT		1.58**				1.31**				
Observations	330	330	286	286	330	330	286	286	330	330
Adj. R2	0.15	0.15	0.31	0.31	0.15	0.14	0.31	0.31	0.17	0.17

Notes: The sample is a balanced panel of 22 industrial countries between 1984-1999, B22-8499, described in Table 1 and in section 2 (Data). MSH columns use Mishkin and Schmidt-Hebbel's (2007) IT dating, while BS columns use Ball and Sheridan's (2005) IT dating. The data frequency is annual and $L.Z=Z_{t-1}$ means 1-year lag in relation to the dependent variable ($y_t = GDPPCG_t$ or $Standard\ deviation\ of\ GDPPCG_t$). Pooled cross-sectional OLS including time and country effects (CTE-OLS) in colums (1)-(8), and pooled cross-sectional OLS including time-variable effect (TE-OLS) in columns (9)-(10), with robust standard errors clustered by country in parentheses. *, ** and *** indicate the significance level of 10%, 5%, and 1%, respectively. AR(1) and AR(2) respectively report the p-values of tests for 1st- and 2nd-order serial correlation. These test first-differenced residuals for columns (1)-(8) and levels residuals for columns (9)-(10). ICE means Implied Cumulative Effect equal to IT $dummy_{t-1}/(1-GDPPCG_{t-1})$, with significance tested by a nonlinear test.

Table 6.A - Ball and Sheridan's (2005) cross-section regressions of the mean annual inflation rate (in %)

	Inflation targeting				Constant inflation targeting			
Sample:	U20-8501			U18-8501	U20-8501			U18-8501
	OLS	MM	OLS	OLS	OLS	MM	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inflation targeting dummy	-0.55	-0.87##	-0.97**, ###	-0.86*, ##				
	(0.35)		(0.36)	(0.42)				
	[0.32]	[0.37]	[0.32]	[0.38]				
Constant IT stage dummy					-0.51	-0.95	-0.58*	-0.80**, ##
					(0.34)		(0.32)	(0.37)
					[0.45]	[0.96]	[0.42]	[0.33]
L.Inflation	-0.78***, ###	-0.51###	-0.51***, ###	-0.54***, ###	-0.76***, ###	-0.69###	-0.62***, ###	-0.55***, ###
	(0.07)		(0.13)	(0.14)	(0.07)		(0.10)	(0.12)
	[0.08]	[0.17]	[0.17]	[0.18]	[0.15]	[0.11]	[0.15]	[0.13]
L.High inflation regime			-2.20**, ##				-1.24*	
			(0.95)				(0.70)	
			[1.10]				[1.14]	
Constant	-1.12***, ##	0.44	0.29	0.36	-1.01***, #	1.05##	0.56	0.36
	(0.32)		(0.46)	(0.49)	(0.33)		(0.40)	(0.44)
	[0.40]	[0.61]	[0.66]	[0.69]	[0.55]	[0.47]	[0.58]	[0.57]
Countries	20	20	20	18	20	20	20	18
Adj. R2	0.90		0.92	0.74	0.87		0.88	0.72

Notes: Cross-sectional OLS estimates (OLS) in columns (1), (3)-(5) and (7)-(8) and MM estimates (MM) in columns (2) and (6). *, **, and *** indicate the significance level of 10%, 5%, and 1% respectively infered from OLS standard errors in parentheses. #, ##, and ### indicate the significance level of 10%, 5%, and 1% respectively, infered from heteroskedasticity robust standad errors reported in brackets. Heteroskedastiticy robust standard errors are Davidson and MacKinnon's (1993) for OLS estimators and Croux, Dhaene and Horelbeke (2008) for MM-estimators. The data used in this table is exactly Ball and Sheridan's (2005).

Table 6.B - Ball and Sheridan's (2005) cross-section regressions of the mean GDP annual growth rate (in %)

	Inflation targeting				Constant inflation targeting			
Sample:	U20-8501			U18-8501	U20-8501			U18-8501
	OLS	MM	OLS	OLS	OLS	MM	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inflation targeting dummy	0.88#	0.62	0.99#	1.04#				
	(0.81)		(0.85)	(0.90)				
	[0.46]	[0.52]	[0.47]	[0.51]				
Constant IT stage dummy					1.29##	0.98##	1.34##	1.32##
					(0.88)		(0.93)	(0.97)
					[0.52]	[0.67]	[0.60]	[0.59]
L.GDP per head growth	-0.60	-1.30	-0.51	-0.53	-0.52	-1.06##	-0.50	-0.48
	(0.41)		(0.44)	(0.46)	(0.41)		(0.43)	(0.46)
	[0.65]	[1.01]	[0.79]	[0.85]	[0.64]	[0.50]	[0.69]	[0.83]
L.High inflation regime			-0.74				-0.30	
			(1.30)				(1.24)	
			[1.16]				[0.78]	
Constant	1.65	3.23	1.45	1.48	1.41	2.68##	1.37	1.30
	(1.31)		(1.38)	(1.43)	(1.31)		(1.36)	(1.45)
	[1.53]	[2.51]	[1.83]	[1.95]	[1.54]	[1.33]	[1.64]	[1.96]
Countries	20	20	20	18	20	20	20	18
Adj. R2	0.15		0.11	0.11	0.23		0.18	0.15

Notes: Cross-sectional OLS estimates (OLS) in columns (1), (3)-(5) and (7)-(8) and MM estimates (MM) in columns (2) and (6). *, **, and *** indicate the significance level of 10%, 5%, and 1% respectively infered from OLS standard errors in parentheses. #, ##, and ### indicate the significance level of 10%, 5%, and 1% respectively, infered from heteroskedasticity robust standad errors reported in brackets. Heteroskedastiticy robust standard errors are Davidson and MacKinnon's (1993) for OLS estimators and Croux, Dhaene and Horelbeke (2008) for MM-estimators. The data used in this table is exactly Ball and Sheridan's (2005).

Table 7 - Probit estimates of propensity scores of non constant IT - sample B22-8499 and BS' calendar - rates in %

Outcome variable:	Contemporaneous	One year ahead	Two years ahead	Three years ahead	Four years ahead
	(1)	(2)	(3)	(4)	(5)
L.Inflation	-0.1584*, #	-0.1916**, ##	-0.2265**, ##	-0.2287**, ##	-0.2444**, ##
	(0.0880)	(0.0883)	(0.0886)	(0.0914)	(0.0995)
	[0.0959]	[0.0956]	[0.1020]	[0.1044]	[0.1089]
L.Trade openness	-0.0073	-0.0064	-0.0079	-0.0085	-0.0099
	(0.0056)	(0.0061)	(0.0064)	(0.0065)	(0.0064)
	[0.0070]	[0.0086]	[0.0090]	[0.0091]	[0.0090]
L.Money growth	-0.0097	-0.0093	-0.0079	-0.0078	-0.0079
	(0.0294)	(0.0289)	(0.0295)	(0.0296)	(0.0296)
	[0.0292]	[0.0296]	[0.0295]	[0.0295]	[0.0293]
L.Central bank turnover rate	0.6676	0.5952	0.2621	0.1060	0.0111
	(0.6491)	(0.6787)	(0.7805)	(0.8072)	(0.8905)
	[0.7018]	[0.7418]	[0.8965]	[0.9329]	[1.0412]
L.GDP per head growth	-0.1382	-0.2334**, ##	-0.3557**, ##	-0.3509**, ##	-0.3476**, ##
	(0.0873)	(0.1104)	(0.1641)	(0.1607)	(0.1613)
	[0.1013]	[0.1076]	[0.1659]	[0.1625]	[0.1628]
L.Pegged regimes	-0.2269	-0.6150	-0.6333	-0.5928	-0.5567
	(0.4815)	(0.6500)	(0.7345)	(0.7398)	(0.7506)
	[0.4205]	[0.7182]	[0.8177]	[0.8251]	[0.8393]
L.Government balance	-0.1424***, ###	-0.2133***, ###	-0.2457***, ##	-0.2393***, ##	-0.2369***, ##
	(0.0527)	(0.0733)	(0.0888)	(0.0883)	(0.0904)
	[0.0429]	[0.0656]	[0.0985]	[0.0981]	[0.1007]
No. of obs.	289	267	245	223	201
Pseudo-R^2	0.2326	0.3031	0.3517	0.3524	0.3582

Notes : Constant terms are included but not reported. *, **, and *** indicate the significance level of 10%, 5%, and 1% respectively for Huber/White/sandwich standard errors reported in parenthesis. #, ##, and ### indicate the significance level of 10%, 5%, and 1% respectively for standard errors clustered by country reported in brackets.

Table 8 -
***t*-Tests and p-values of differences between treated and untreated - sample B22-8499 - rates in %**

Matching estimator:	t-stat. (p-value)							
	Unmatched	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching			Local linear regression matching	Kernel matching
				r=0.03	r=0.01	r=0.005		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
8.A. Contemporaneous annual CPI growth								
L.Inflation	-1.13	-1.48	-0.03	-0.38	-0.19	0.22	-0.32	-0.36
	(0.26)	(0.17)	(0.98)	(0.71)	(0.86)	(0.83)	(0.76)	(0.73)
L.GDP per head growth	-1.79	1.17	0.25	0.39	0.56	0.41	0.44	0.10
	(0.08)	(0.26)	(0.81)	(0.71)	(0.59)	(0.69)	(0.67)	(0.92)
L.Trade openness	-1.07	0.84	0.71	0.09	0.17	0.03	0.26	0.03
	(0.29)	(0.42)	(0.49)	(0.93)	(0.87)	(0.98)	(0.80)	(0.98)
L.Central bank turnover rate	0.00	-1.38	-0.62	-0.05	-0.40	0.04	-0.15	-0.05
	(1.00)	(0.19)	(0.55)	(0.96)	(0.70)	(0.97)	(0.88)	(0.96)
L.Government balance	-2.04	-0.19	-0.73	-0.45	-0.41	-0.41	-0.26	-0.58
	(0.04)	(0.85)	(0.48)	(0.66)	(0.69)	(0.70)	(0.80)	(0.57)
L.Money growth	-1.24	-1.79	-1.02	-0.36	-0.25	-1.30	-0.35	-0.35
	(0.22)	(0.10)	(0.33)	(0.73)	(0.81)	(0.23)	(0.73)	(0.73)
L.Pegged regimes	-0.72	-1.15	0.17	-0.55	-0.60	-0.26	-0.55	-0.42
	(0.47)	(0.27)	(0.87)	(0.59)	(0.56)	(0.80)	(0.59)	(0.68)
No. of treated	7	7	7	7	7	5	7	7
No. of controls	282	7	20	278	147	65	280	280
8.B. One year ahead annual CPI growth								
L.Inflation	-1.22	-1.10	-0.18	-0.53	-0.80	-0.61	-0.32	-0.51
	(0.23)	(0.29)	(0.86)	(0.61)	(0.44)	(0.56)	(0.76)	(0.62)
L.GDP per head growth	-1.84	0.57	0.21	0.49	0.41	-1.08	-0.63	0.42
	(0.07)	(0.58)	(0.84)	(0.64)	(0.69)	(0.31)	(0.55)	(0.68)
L.Trade openness	-1.06	1.14	1.43	0.26	0.27	0.30	-0.09	0.13
	(0.29)	(0.28)	(0.18)	(0.80)	(0.79)	(0.78)	(0.93)	(0.90)
L.Central bank turnover rate	-0.01	-0.55	0.54	-0.19	0.20	0.34	-0.07	-0.09
	(0.99)	(0.59)	(0.60)	(0.85)	(0.85)	(0.75)	(0.95)	(0.93)
L.Government balance	-2.02	0.35	-0.38	-0.24	0.12	1.02	0.40	-0.23
	(0.05)	(0.73)	(0.71)	(0.82)	(0.91)	(0.34)	(0.70)	(0.82)
L.Money growth	-1.47	-0.08	0.10	-0.38	-0.40	0.19	-0.04	-0.41
	(0.14)	(0.94)	(0.93)	(0.71)	(0.70)	(0.85)	(0.97)	(0.69)
L.Pegged regimes	-0.73	-1.15	0.00	-0.43	-0.65	-0.11	0.08	-0.46
	(0.47)	(0.27)	(1.00)	(0.67)	(0.53)	(0.92)	(0.94)	(0.65)
No. of treated	7	7	7	7	6	5	5	7
No. of controls	260	7	18	258	210	78	258	260

Table 8 cont. -
t **-Tests and p-values of differences between treated and untreated - sample B22-8499 - rates in %**

Matching estimator:	t-stat. (p-value)							
	Unmatched	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching r=0.03	Radius matching r=0.01	Radius matching r=0.005	Local linear regression matching	Kernel matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
8.C. Two years ahead annual CPI growth								
L.Inflation	-1.28	-1.06	-0.37	-0.41	-0.40	-0.27	-0.28	-0.31
	(0.20)	(0.31)	(0.72)	(0.69)	(0.70)	(0.80)	(0.79)	(0.76)
L.GDP per head growth	-1.90	0.92	0.10	-0.27	0.19	0.14	-0.28	-0.29
	(0.06)	(0.37)	(0.92)	(0.79)	(0.85)	(0.90)	(0.78)	(0.78)
L.Trade openness	-1.06	0.19	0.88	-0.12	-0.86	-0.91	-0.47	-0.28
	(0.29)	(0.85)	(0.40)	(0.91)	(0.43)	(0.41)	(0.65)	(0.79)
L.Central bank turnover rate	-0.03	-0.82	-0.63	-0.60	-1.16	-0.92	-0.50	-0.54
	(0.98)	(0.43)	(0.54)	(0.56)	(0.29)	(0.41)	(0.63)	(0.60)
L.Government balance	-1.93	-0.84	-0.49	0.29	-0.33	-0.32	0.40	0.27
	(0.06)	(0.42)	(0.64)	(0.78)	(0.75)	(0.77)	(0.70)	(0.79)
L.Money growth	-1.53	0.49	0.54	0.15	-0.56	0.02	0.06	0.12
	(0.13)	(0.63)	(0.60)	(0.89)	(0.60)	(0.98)	(0.95)	(0.91)
L.Pegged regimes	-0.74	-0.52	0.00	-0.20	-0.17	-0.63	-0.35	-0.24
	(0.46)	(0.61)	(1.00)	(0.85)	(0.87)	(0.56)	(0.74)	(0.82)
No. of treated	7	7	7	5	4	3	5	5
No. of controls	238	5	16	233	176	164	236	236
8.D. Three years ahead annual CPI growth								
L.Inflation	-1.31	-0.22	-0.57	-0.43	-0.73	-1.03	.	-0.29
	(0.19)	(0.83)	(0.58)	(0.68)	(0.51)	(0.41)	.	(0.77)
L.GDP per head growth	-1.90	0.38	0.29	-0.10	-0.04	0.83	.	0.08
	(0.06)	(0.71)	(0.77)	(0.93)	(0.97)	(0.49)	.	(0.94)
L.Trade openness	-1.08	0.33	0.80	-0.16	-0.87	-0.61	0.50	0.32
	(0.28)	(0.75)	(0.44)	(0.88)	(0.44)	(0.61)	(0.63)	(0.76)
L.Central bank turnover rate	-0.06	-0.97	-0.79	-0.77	-0.58	-1.03	-0.22	-0.19
	(0.95)	(0.35)	(0.44)	(0.46)	(0.59)	(0.41)	(0.83)	(0.85)
L.Government balance	-1.83	-0.49	-0.31	0.12	0.68	0.84	-0.14	-0.26
	(0.07)	(0.63)	(0.76)	(0.91)	(0.53)	(0.49)	(0.89)	(0.80)
L.Money growth	-1.53	-0.99	-0.50	-0.01	-0.94	-1.07	-0.21	-0.10
	(0.13)	(0.34)	(0.62)	(0.99)	(0.40)	(0.40)	(0.84)	(0.93)
L.Pegged regimes	-0.75	0.00	-0.17	-0.17	-0.63	-0.63	-0.34	-0.25
	(0.46)	(1.00)	(0.87)	(0.87)	(0.56)	(0.60)	(0.74)	(0.81)
No. of treated	7	7	7	5	3	2	6	6
No. of controls	216	6	16	203	159	65	216	216

Table 8 cont. -
***t* -Tests and p-values of differences between treated and untreated - sample B22-8499 - rates in %**

Matching estimator:	t-stat. (p-value)							
	Unmatched	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching			Local linear regression matching	Kernel matching
				r=0.03	r=0.01	r=0.005		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	8.E. Four years ahead annual CPI growth							
L.Inflation	-1.35	-0.97	0.16	-0.67	-0.14	0.53	-3.63	-0.40
	(0.18)	(0.35)	(0.88)	(0.52)	(0.90)	(0.64)	(0.01)	(0.70)
L.GDP per head growth	-1.85	0.16	0.18	0.25	0.02	0.22	1.15	0.05
	(0.07)	(0.88)	(0.86)	(0.81)	(0.99)	(0.84)	(0.28)	(0.96)
L.Trade openness	-1.09	0.10	0.38	0.48	-0.91	-0.44	0.11	0.27
	(0.28)	(0.92)	(0.71)	(0.64)	(0.42)	(0.69)	(0.92)	(0.79)
L.Central bank turnover rate	-0.10	1.15	0.00	-0.26	-0.43	-0.49	1.51	-0.07
	(0.92)	(0.27)	(1.00)	(0.80)	(0.69)	(0.66)	(0.16)	(0.95)
L.Government balance	-1.72	-0.03	-0.80	-0.16	-0.14	-0.68	0.19	-0.10
	(0.09)	(0.98)	(0.44)	(0.88)	(0.90)	(0.55)	(0.85)	(0.92)
L.Money growth	-1.59	0.49	0.46	-0.11	-0.56	-0.08	-0.13	-0.11
	(0.11)	(0.63)	(0.65)	(0.92)	(0.61)	(0.94)	(0.90)	(0.91)
L.Pegged regimes	-0.76	0.00	-0.17	-0.23	-0.18	0.17	-0.30	-0.30
	(0.45)	(1.00)	(0.87)	(0.83)	(0.87)	(0.88)	(0.77)	(0.77)
No. of treated	7	7	7	6	3	3	6	6
No. of controls	194	6	17	177	144	46	194	194

Table 9 - Matching estimates of treatment effect of IT on inflation rates - sample B22-8499

Matching estimator:	*ATE* Unmatched	*ATT* Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching r=0.03	r=0.01	r=0.005	Local linear regression matching	Kernel matching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			9.A. Contemporaneous annual CPI growth					
Difference	-1.59	-0.85	0.36	-0.20	-0.17	-0.31	-0.51	-0.26
S.E.	(1.82)	(0.81)	(0.78)	(1.15)	(0.82)	(0.72)	(1.52)	(1.02)
No. of treated	7	7	7	7	7	5	7	7
No. of controls	282	7	20	278	147	65	280	280
			9.B. One year ahead annual CPI growth					
Difference	-1.81	-1.01*	-0.06	-0.47	-0.67	-0.05	-0.11	-0.51
S.E.	(1.60)	(0.72)	(0.70)	(1.10)	(1.13)	(0.93)	(0.76)	(1.08)
No. of treated	7	7	7	7	6	5	5	7
No. of controls	260	7	18	258	210	78	258	260
			9.C. Two years ahead annual CPI growth					
Difference	-2.50**	-0.77	-0.85*	-1.15	-1.10	-1.47	-1.21*	-1.06
S.E.	(1.49)	(0.76)	(0.63)	(0.99)	(1.50)	(1.90)	(0.94)	(0.87)
No. of treated	7	7	7	5	4	3	5	5
No. of controls	238	5	16	233	176	164	236	236
			9.D. Three years ahead annual CPI growth					
Difference	-2.53**	-0.80	-1.18**	-1.32	-2.62**	-3.29**	4.71	-1.25*
S.E.	(1.42)	(0.92)	(0.63)	(1.08)	(1.37)	(1.50)	(10.15)	(0.88)
No. of treated	7	7	7	5	3	2	6	6
No. of controls	216	6	16	203	159	65	216	216
			9.E. Four years ahead annual CPI growth					
Difference	-2.24**	-0.80*	-0.48	-1.02	-1.06	-0.46	-3.36	-0.89
S.E.	(1.33)	(0.58)	(0.57)	(0.97)	(1.54)	(1.07)	(3.79)	(0.79)
No. of treated	7	7	7	6	3	3	6	6
No. of controls	194	6	17	177	144	46	194	194
			9.F. Five-year average annual CPI growth					
Difference	-2.13***	-0.84***	-0.44*	-0.83***	-1.12***	-1.12**	-0.10	-0.79***
S.E.	(0.19)	(0.04)	(0.27)	(0.21)	(0.41)	(0.59)	(1.33)	(0.18)

Notes: A 0.06 fixed bandwidth and a biweight kernel are used for kernel and local linear regression matching. Panels A-E show the differences in the levels of the annual rates. Panel F shows the differences in the levels of the 5-year average rates. *, **, *** indicate the significance level of 10%, 5%, and 1% respectively in one-sided t-tests. For inflation, $H_0: ATT=0$ and $H_1: ATT<0$. For per capita GDP, $H_0: ATT=0$ and $H_1: ATT>0$.

Table 10 - Matching estimates of treatment effect of IT on per capita GDP growth rates - sample B22-8499

Matching estimator:	*ATE*	*ATT*						
	Unmatched	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching			Local linear regression matching	Kernel matching
				r=0.03	r=0.01	r=0.005		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	10.A. Contemporaneous annual GDP per-capita growth							
Difference	-0.27	2.46**	0.98	1.32*	1.64*	1.31*	1.66*	1.02
S.E.	(1.06)	(1.37)	(0.93)	(0.91)	(1.04)	(0.85)	(1.08)	(0.85)
No. of treated	7	7	7	7	7	5	7	7
No. of controls	282	7	20	278	147	65	280	280
	10.B. One year ahead annual GDP per-capita growth							
Difference	-0.42	0.06	0.11	-0.07	0.03	-0.46	-0.54	-0.05
S.E.	(1.10)	(1.08)	(0.99)	(1.06)	(1.17)	(1.58)	(1.21)	(1.05)
No. of treated	7	7	7	7	6	5	5	7
No. of controls	260	7	18	258	210	78	258	260
	10.C. Two years ahead annual GDP per-capita growth							
Difference	0.37	-0.17	0.56	0.42	1.64*	1.95*	0.32	0.43
S.E.	(1.14)	(1.49)	(0.75)	(0.94)	(1.15)	(1.48)	(0.91)	(0.87)
No. of treated	7	7	7	5	4	3	5	5
No. of controls	238	5	16	233	176	164	236	236
	10.D. Three years ahead annual GDP per-capita growth							
Difference	1.42	1.19*	1.85***	0.16	0.89	0.81	2.80	0.85
S.E.	(1.16)	(0.86)	(0.67)	(0.97)	(0.89)	(1.28)	(8.28)	(0.76)
No. of treated	7	7	7	5	3	2	6	6
No. of controls	216	6	16	203	159	65	216	216
	10.E. Four years ahead annual GDP per-capita growth							
Difference	1.68*	2.03**	1.97***	1.44**	1.95**	2.25***	0.23	1.51**
S.E.	(1.19)	(0.92)	(0.72)	(0.85)	(1.00)	(0.83)	(3.42)	(0.88)
No. of treated	7	7	7	6	3	3	6	6
No. of controls	194	6	17	177	144	46	194	194
	10.F. Five-year average annual GDP per-capita growth							
Difference	0.56*	1.11**	1.09***	0.65**	1.23***	1.17***	0.90*	0.75***
S.E.	(0.43)	(0.52)	(0.36)	(0.31)	(0.35)	(0.48)	(0.59)	(0.26)

Notes: A 0.06 fixed bandwidth and a biweight kernel are used for kernel and local linear regression matching. Panels A-E show the differences in the levels of the annual rates. Panel F shows the differences in the levels of the 5-year average rates. *, **, *** indicate the significance level of 10%, 5%, and 1% respectively in one-sided t-tests. For inflation, $H_0: ATT=0$ and $H_1: ATT<0$. For per capita GDP, $H_0: ATT=0$ and $H_1: ATT>0$.

Appendix A. Data sources:

Samples' B22-8499, B18-8499 and B22-7199 data comes from Ghosh et al. (2002), who derive their data from the following IMF sources: World Economic Outlook (WEO), International Financial Statistics (IFS), Information Notice System (INS), Annual Report on Exchange Arrangements and Exchange Restrictions (AREAR) and Direction of Trade Statistics (DOTS). In Ghosh's et al. (2002) notation, the variables that LY and I use are: CPIG – the Consumer Price Index growth (average of period); GDPPCG – the per capita real GDP growth; OPEN=(XGDP+MGDP) – the sum of exports and imports of goods and services (percent of GDP); BMG - broad money growth; CGGDP – Central Government balance (percent of GDP); and TTG – terms of trade growth. Ghosh et al. (2002) construct the CBTURN5 – Central Bank governor turnover rate (over 5 years) – according to Cukierman (1992). Following LY, I use the *de facto* exchange rate classification proposed by Reinhart and Rogoff (2004) to construct a dummy variable for fixed exchange rate regimes denoted FIX. And similarly to LY, volatilities of consumer price inflation and per capita real GDP growth are calculated over three-year periods.

In U20-8501 and U20-8009, the inflation rate series is the average of year from the WEO by the IMF. The series of annual growth of GDP per head (expenditure approach, in US $, constant prices, constant PPPs), trade (exports plus imports of goods and services) to GDP ratio, terms of trade growth, and General Government underlying balance as percentage of the potential GDP, all come from the OECD. The broad money growth variable merges Ghosh's et al. (2002) data with OECD broad money index growth. Again, I use the *de facto* exchange rate classification proposed by Reinhart and

Rogoff (2004), extended until 2009, to construct a dummy variable for fixed exchange rate regimes. Finally, I have revised and extended Ghosh's et al. (2002) CBTURN5 until 2009, using the information available in Dreher's et al. (2010) database (available online), that covers central bank governors' turnovers in 140 countries during 1970-2009. Like Ghosh et al. (2002), who follow Cukierman (1992), I have constructed the updates of the variable as the number of central bank governors changes within a 5 year period, divided by 5 to give the annual rate of turnover. The variable is constructed for fixed 5-year periods, and assigned to each year within that period. Along this update process, I have found some discrepancies in Ghosh et al. (2002), probably do to Dreher's et al. (2010) updates in the database or Ghosh's et al. (2002) coding errors, for the period before 1999. Thus, to keep comparability with LY, I have used CBTURN5 exactly as made available in Ghosh et al. (2002) for B22-8499, B18-8499 and B22-7099. And I have used my updated measure in samples U20-8501 and U20-8009.

Appendix B. Lin and Ye replication

To facilitate comparison with LY, the series used in this appendix are measured in decimals, i.e. they have not been multiplied by 100, like the series in the main text.

Table B.1 - Probit estimates of propensity scores as in Lin and Ye (2007)

	Dependent variable	
	NCIT	CIT
CPIG_1	-18.696***, ###	-25.833***, ###
	(5.699)	(6.721)
	[8.615]	[10.330]
OPEN	-0.041	0.355
	(0.312)	(0.313)
	[0.814]	[0.822]
BMG	-3.807**	-3.468*
	(1.810)	(2.041)
	[2.395]	[2.747]
CBTURN5	-2.472***	-2.085**
	(0.793)	(0.868)
	[1.528]	[1.624]
GDPPCG	2.982	4.973
	(2.959)	(3.654)
	[3.693]	[3.791]
CGGDP	0.468	1.323
	(2.967)	(3.705)
	[4.454]	[5.996]
FIX	-0.640***	-0.949***
	(0.256)	(0.279)
	[0.587]	[0.629]
No. of obs.	321	321
No. of treated	45	36
Pseudo-R^2	0.22	0.27

Notes: Constant terms are included but not reported. *, **, and *** indicate the significance level of 10%, 5%, and 1% respectively for Huber/White/sandwich standard errors reported in parenthesis. #, ##, and ### indicate the significance level of 10%, 5%, and 1% respectively for standard errors clustered by country reported in brackets.

In a 16-year panel of 22 countries with 7 ITers, LY match and analyze 45 events of treatment, where 45 is the sum of the number of years each of the 7 IT countries had been under treatment until 1999. Moreover, LY match those 45 events by their same year observable characteristics and fully base their performance evaluation on that same one-year contemporaneous realization of the outcome.

To make sure that my understanding of LY's estimation is correct, in Table B.1, column NCIT, I use Ghosh's et al. (2002) data to replicate LY's Probit estimates (shown in column NCIT of their Table 2). Quantitative differences between mine and theirs are negligible and due to the fact that we have independently completed some missing values in Ghosh's BMG and calculated our own FIX variable series.

In Table B.2, Panel A, I present negative, but small and insignificant, matching estimates of the treatment effect of non-constant IT on inflation levels, results that are similar to LY's Panel A in Table 3. The small quantitative differences are because LY discard all controls whose estimated propensity score are lower than the lowest score among treated, a procedure they recognize not to change their results.

Yet replicating LY's PSM for constant IT, respectively in column CIT of Table B.1 and in Panel B of Table B.2, my Probit and matching estimates differ a little bit from LY's because we differ in the classification of Spain. While LY mistakenly consider Spain as a constant ITer after 1995, I follow BS, and consider Spain as a non-treated country for constant IT purposes, given it joined the European Central Bank common policy in 1999 before reaching the constant stage. In spite of this classification difference, the qualitative result of IT irrelevance also holds for constant IT in my replication of LY.

Table B.2 - Matching estimates of treatment effect as in Lin and Ye (2007) on the level of inflation (CPIG)

	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching			Local linear regression matching	Kernel matching
			r=0.03	r=0.01	r=0.005		
Panel A: Treatment effect of non-constant inflation targeting on the level of inflation (rates in decimals)							
ATT	-0.0009	-0.0008	-0.0018	-0.0018	-0.0031	-0.0019	-0.0022
s.e.	(0.0036)	(0.0028)	(0.0055)	(0.0030)	(0.0035)	(0.0055)	(0.0054)
No. of treated	45	45	45	44	38	45	45
No. of controls	35	79	276	189	120	276	276
No. of obs. used	80	124	321	233	158	321	321
Panel B: Treatment effect of constant inflation targeting on the level of inflation (rates in decimals)							
ATT	0.0002	-0.0015	-0.0007	-0.0002	-0.0025	0.0103	-0.0016
s.e.	(0.0030)	(0.0027)	(0.0062)	(0.0028)	(0.0029)	(0.0210)	(0.0058)
No. of treated	36	36	33	29	22	35	35
No. of controls	27	64	279	131	84	283	283
No. of obs. used	63	100	312	160	106	318	318

Notes: A 0.06 fixed bandwidth and a biweight kernel are used for kernel and local linear regression matching. *, **, *** indicate the significance level of 10%, 5%, and 1% respectively in one-sided t-tests. For inflation, $H_0: ATT=0$ and $H_1: ATT<0$. For per capita GDP, $H_0: ATT=0$ and $H_1: ATT>0$.