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

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Abstract

I evaluate the treatment effect of inflation targeting (IT) among industrial economies in the early 1990s, controlling for common time and country specific effects, to show that IT had significant enhancing effects on realized inflation and GDP growth. By additionally showing that IT lowered the long-term nominal interest rates, I indicate that IT worked through the expectation channel, meaning that IT was received as an improvement in monetary policy. This credibility bonus of pioneering IT regimes was economically important, significant and robust to sample and technique variations, providing support for policymakers' optimism at the inception of the regime.

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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 as a fad, without an initial track record of good performance?

Although the pioneering IT central banks 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]

Ball and Sheridan (2005) [denominated BS hereafter], for instance, put forth the proposition that IT caused no significant improvement in realized rates of inflation and output growth in a sample of major industrial economies. Although IT had a lowering effect on inflation and a positive impact on GDP growth, they were not significant after controlling these processes for mean-reversion in cross-section difference-in-difference regressions. This view of IT irrelevance is shared by Lin and Ye (2007) [denominated LY hereafter], who have found that IT had no significant effects on lowering inflation after controlling for the self-selectivity bias of policy adoption in propensity score matches. [2, 3]

In this paper, I demonstrate that IT did matter for realized inflation and output growth. Through panel estimates of an adaptive Phillips curve that use data similar to LY and BS, I show that the credibility bonus of IT policy amounted to a significant short-run effect close to a 1 percentage point reduction in the annual mean inflation of inflation

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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 …' (p. 195).

[3] Studies like Geraats (1998), Roger and Stone (2005) and Crowe (2010) indicate that IT improved transparency and accountability, but not realized macroeconomic aggregates, as inflation and output growth.

targeting countries (ITers), or a cumulative effect close to a 2 percentage point reduction (in Tables 2 and 3). Although small compared to the worldwide inflation reduction observed during the 1980s, it is relatively large and significant in comparison with average inflation among industrial economies close to 2% per year in the late 1990s.

In these same samples, the impacts of IT on the volatilities of inflation and output were negative, but mostly insignificant (in Table 4). Even so, given that inflation and output growth rates had to undergo unavoidable volatility to converge to the new long-run goals during the period studied, the fact that IT did not cause increases in volatilities also shows IT effectiveness in improving macroeconomic performance at its inception.

To rule out the suspicion that this result spuriously reflects the role played by supply shocks across the pre- and post-IT periods, and is in reality the consequence of a more credible policy, I first show that similar results still hold when I allow for time and cross-section heterogeneity in the inflation sensitivity to output growth (in Table 3, columns (6) and (10)). Second, and more intuitively, I show that IT adoption significantly lowered the long-term nominal interest rates, widely recognized as a proxy for long-run inflation expectations (in Table 5).

What has caused this qualitative change in BS's and LY's IT irrelevance results, distancing their results from my finding of IT enhancing effects?

In face of the generalized and sizable inflation reduction of the 1980s and early 1990s, IT's absolute importance was second-order. However, once in the low inflation regime of the 1990s, IT did play a significant role, which is not captured by BS, LY and others, because they do not control for the economic conditions at the eve of IT adoption.

Instead of working with cross-section difference-in-differences or panel propensity score matches – respectively used by BS and LY – which make it difficult to model the short-run monetary tradeoffs and to control for common time variation and countries' non-observable specificities, I suggest a dynamic panel approach that exploits the time and country dimensions to isolate the improvement in performance exclusively due to the IT regime from other sources that might overlap in a cross-section. In evaluating non-simultaneous IT treatment, it is particularly important to control for the 1980-90s' worldwide trend of falling inflation and macroeconomic volatility.[4] 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 about the causal effect of IT on 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, in accordance with Batini and Nelson (2002), and also weakens some endogeneity suspicions of the IT policy.

Besides analyzing LY's 1984-1999 annual balanced panel of 22 industrial economies, combined with (i) BS's calendar of IT adoptions or (ii) Mishkin and Schmidt-Hebbel's (2007) [denominated MSH hereafter] calendar of IT adoptions, I study: (iii) an annual balanced panel that extends LY's data back to 1971, (iv) an annual unbalanced

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[4] See Cecchetti *et al.* (2006) for a cross-country study about the worldwide trend in the 1980-90s of falling inflation and output volatilities.

panel that covers BS's sample of 20 industrial countries between 1985-2001, and (v) another unbalanced panel with BS's economies between 1980-2009. In all samples, I have been able to show that the IT policy improved realized inflation and output growth.

Having overturned BS's and LY's conclusions of IT irrelevance, I then revisit both studies. First, by subtracting the common time and country fixed effects from the variables of interest, then running BS's cross-section difference-in-difference regressions with these mean-deviation transformed variables, I am able to show that IT significantly lowered inflation, increased output and lowered long-term interest rates, in an example of Ball's (1994a) credible disinflation. Second, by reformulating LY's propensity score matching to fulfill the conditional independence assumptions (CIA) – of (i) one unit of treatment per ITer and (ii) matching of observables before treatment, violated by LY – once again, I reach 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 reveals that the initial track record of IT was significantly positive, in support of policymakers' optimism, and thus justifying its spread around the world after the mid-nineties.

The article is organized as follows. Section 1 compares the panel regression methodology with the cross-section difference-in-difference regression and the PSM techniques, showing that the linear dynamic panel procedure controlled for the right set of covariates is a more suitable approach in contexts where short-run tradeoffs and non-observables matter. Subsection 2.1 reports and discusses the main results. Ball and Sheridan (2007) and Lin and Ye (2007) are respectively revisited in subsections 2.2 and

2.3. The conclusions are in section 3, and the data sources and various samples used are described in Appendix A.

1. Methodology

1.1. Linear Regression

I first work with a partial adjustment model:

$$y_{n,t} = \alpha^{y} \cdot y_{n,t-1} + \beta^{y} \cdot d_{n,t-1}^{IT} + \gamma^{y} \cdot X_{n,t-1} + \delta_{t}^{y} + \eta_{n}^{y} + \upsilon_{n,t}^{y}, \tag{1}$$

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 similar reasoning 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 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 GDP growth, among others. The term δ_{t}^{y} allows for time effects that capture common shocks to all countries, η_n^y allows for cross-country fixed effects, and $\upsilon_{n,t}^y$ is the disturbance. 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 IT policy effectiveness.

BS's model can result from the time degeneration of equation (1), which sums 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:

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

where: $y_{n,\,post}$ is country's n post-targeting average inflation value, $y_{n,\,pre}$ 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:

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

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

In equation (2), it makes sense that the treatment variable $d_{n,\,post}^{IT}$ is contemporaneous with the dependent variable $y_{n,\,post}$, given that $d_{n,\,pre}^{IT}$ 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 a summary statistic (for example, the mean or the standard deviation) over many post-treatment years. However, equation (2)'s parameter estimates may be biased because of the omission of the time-effect variation δ_t^y and the impossibility of identifying the country-effect variation η_n^y from $e_{n,post}^y$.

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 $pre\text{-}IT$ and $post\text{-}IT$ 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 since IT adoption happened at different times for different economies, the pair of $pre\text{-}IT$ and $post\text{-}IT$ periods are not the same for all ITers 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.

Apart from 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.

Regarding the estimation method of equation (1), the LSDV (least square dummy variable) estimator may have a bias when *T* is small, as demonstrated by Nickell (1981). With big *N* and small *T*, the Difference GMM of Arellano and Bond (1991) would be advisable. 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 approach 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, I choose to present estimates from both the LSDV estimator and Difference GMM.

The LSDV, or fixed-effect OLS estimator, is biased if the transformed independent variables $y^*_{n,t-1} = y_{n,t-1} - (T-1)^{-1}(y_{n,1} + \ldots + y_{n,T-1})$ and $Z^*_{n,t-1} = Z_{n,t-1} - (T-1)^{-1}(Z_{n,1} + \ldots + Z_{n,T-1})$, for $Z_{n,t-1} = (d^{IT}_{n,t-1}, X_{n,t-1})$, are correlated with the transformed error $\upsilon^{y*}_{n,t} = \upsilon^{y}_{n,t} - (T-1)^{-1}(\upsilon^{y}_{n,2} + \ldots + \upsilon^{y}_{n,T})$. The correlation between $y^*_{n,t-1}$ and $\upsilon^{y*}_{n,t}$ has been shown by Nickell (1981) to be negative, 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) LSDV estimators of (1) depend on the covariances $\operatorname{cov}(Z_{n,t-1}, \upsilon^{y}_{n,t-j})$ for all $t \geq 2$ and $j = 1, \ldots, (t - 2)$. Specifically for the inflation-IT relation, when $y = \pi$ represents the inflation rate, it is not unreasonable to assume that

$\operatorname{cov}\left(d_{n,t-1}^{IT}, \upsilon_{n,t-j}^{\pi}\right) \neq 0$, meaning $d_{n,t-1}^{IT}$ is predetermined. After all, IT adoption is generally motivated as a way of "improving" the inflation process behavior. Less clear at this point is the sign of $\operatorname{cov}\left(d_{n,t-1}^{IT}, \upsilon_{n,t-j}^{\pi}\right)$. While BS argue that countries with higher past inflation tend to adopt IT, LY show (and I confirm in Table 7 below) that switches to the IT regime are more probable when inflation is already low. This latter being the case, the $\operatorname{cov}\left(d_{n,t-1}^{IT}, \upsilon_{n,t-j}^{\pi}\right) \leq 0$ for $t \geq 2$ and $j = 2, \ldots, (t - 2)$ would bias the β^{π} LSDV estimates of (1) upwards, thus making it more difficult to find that IT decreases inflation. Even so, to confirm the sign and significance of the IT effects, I additionally present Difference GMM estimates that treat the $Z_{n,t-1}$ explanatory variables as strictly exogenous, and also as predetermined ones.

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

1.2. Propensity score matching

In the 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 CIA 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 may be 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 – and does not depend on non-observables – the propensity score matching (PSM) approach is an alternative that is more focused on the estimation of the propensity score of policy adoption, $E\left[d^{IT}_{n,\,post}\,|\,y_{n,\,pre}, X_{n,\,pre}\right] \equiv p\left(y_{n,\,pre}, X_{n,\,pre}\right)$, where $p\left(y_{n,\,pre}, X_{n,\,pre}\right)$ is the conditional probability of IT adoption, instead of on the model for outcomes, $E\left[y_{n,\,post}\,|\,d^{IT}_{n,\,post}, y_{n,\,pre}, X_{n,\,pre}\right]$. 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:

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

where $g\left(y_{n,\,pre},X_{n,\,pre}\right)$ is a known weighting function. For the PSM ATT, $g\left(y_{n,\,pre},X_{n,\,pre}\right)=\frac{p\left(y_{n,\,pre},X_{n,\,pre}\right)}{p\left(d^{IT}_{n,\,post}=1\right)}$, meaning that PSM weights are proportional to the probability of treatment at each value for the covariates. For the β^{y} regression, $g\left(y_{n,\,pre},X_{n,\,pre}\right)=\frac{p\left(y_{n,\,pre},X_{n,\,pre}\right)\cdot\left(1-p\left(y_{n,\,pre},X_{n,\,pre}\right)\right)}{E\left[p\left(y_{n,\,pre},X_{n,\,pre}\right)\cdot\left(1-p\left(y_{n,\,pre},X_{n,\,pre}\right)\right)\right]}=\frac{\sigma^{2}_{d^{IT}}\left(y_{n,\,pre},X_{n,\,pre}\right)}{E\left[\sigma^{2}_{d^{IT}}\left(y_{n,\,pre},X_{n,\,pre}\right)\right]}$, meaning that regression weights are proportional to the variance of treatment at each value for the covariates.[5] Thus, it is BS's and others' lack of control for $X_{n,\,pre}$ in the estimation of equation (2) that may impair the results of the previous literature, 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

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

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 that IT seeks to control is affected by common-time and country-fixed effects. 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.

Ignoring the plausible common time-variable and country fixed effects, to better understand LY's results, it is enlightening to describe how the PSM would work well in a panel context. However, this 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 ITer 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 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 $\left(h + k_{ni}\right)$ if country ni adopted IT and $y^0_{ni,h+k_{ni}}$ if not.

However, because the outcome that would have been observed if ITer 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 CIA $\left\{y^1_{n,h+k_n}, y^0_{n,h+k_n}\right\} \perp d^{IT}_{n,h+k_n} \mid p\left(y_{n,k_n-1}, X_{n,k_n-1}\right)$, 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 measures:

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

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 ITers.

If, additionally, one assumes, as LY does, 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 for the treatment and controls outcomes to occur at the same time, and for any $\tau \le (T-h)$, the above equations can be rewritten as:

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

or

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

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

$$\overline{ATT}_H = \frac{1}{(H+1)}\sum_{h=0}^{H} ATT_h \tag{4}$$

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

which, for the biggest H possible, approximates the cross-section ATT measure over all post-treatment years, usually calculated in the PSM literature:[6]

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

that is the PSM analogue of the cross-section difference-in-difference β^y .

LY's PSM results are flawed because, when attempting to compute (4), 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 $p\left(y_{ni,\, k_{ni}-1}, X_{ni,\, k_{ni}-1}\right)$ 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:

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[6] 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 ones, which makes their results not comparable to LY's and mine.

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

thus matching on observables that have already been influenced by previous h years of treatment. Just taking into account the conditioning variables $y_{n,\, h+\tau-1}$ and $y_{ni,\, h+k_{ni}-1}$, for illustration, 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, after *(h-1)* years of treatment, $y_{ni,\, h+k_{ni}-1} \approx y_{n,\, h+\tau-1}$. This violates the exogeneity assumption for $h>0$, and clearly results in underestimation of the treatment effect through time.

2. Results

Next, in subsection 2.1, I present the linear regression results. As explained in section 1, a dynamic panel that controls for observable covariates is as able to address the IT causal effect on inflation as the PSM, with the advantage of taking relevant non-observables into account and making explicit the roles of past policy actions and observable covariates in the inflation determination (or in another macroeconomic variable). The samples used are described in Table 1 and detailed in Appendix A.

To make sure that the main results I obtained in subsection 2.1 are not technique sensitive, in subsections 2.2 and 2.3 I show that similar qualitative results could have been obtained by cross-section difference-in-difference and PSM, had BS and LY controlled for the existing macroeconomic differences at the time of IT adoption.

< Insert **Table 1** around here >

2.1. Main Results

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

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

Beyond the current and lagged inflations ($\pi_{n,t}$ and $\pi_{n,t-1}$), GDP growth ($GDPG_{n,t-1}$), and terms of trade growth ($TTG_{n,t-1}$) – usual variables in an open economy adaptive Phillips curve [7] – following Ghosh *et al.* (2002) and LY, I include other potential determinants of inflation 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

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[7] See Hutchison and Walsh (1998), Andersen and Walsher (1999), Mankiw (2001) or Mehra (2004) for similar adaptive Phillips curves applied to one-country time series data.

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.

<Insert **Table 2** around here>

The first 4 columns of Table 2 show estimates for sample B22-8499 (described in Table 1, column (1)) with Mishkin and Schmidt-Hebbel's (2007) [denominated MSH hereafter] calendar of IT adoptions (described in Table 1, column (5)). This is the sample that gets more attention in this paper, not only because it was the sample used by LY, with whose results mine differ, but also because it is a balanced panel that covers the inception of the IT system. After 1999, 11 of these 22 industrial countries unified their monetary policy under the European Central Bank, making it debatable whether they should be analyzed as independent countries from then on. MSH's IT adoption dates are the closest to Bernanke's et al. (1999) country-case studies and the IMF's (2006). They also better represent the common central bank operating procedure of starting an IT regime with explicit targets for the coming year on rather than for the current year, as described in Johnson (2002).

I present pooled cross-sectional OLS estimates in column (1), 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, it can be seen 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 decreases over-time lagged inflation on the right-hand side, are significant.

Columns (3) to (8) of Table 2 present panel estimates with time and fixed-effects for the various samples and two calendars of IT adoption described in Table 1. From column (3) to (4), I change from MSH's to BS's IT calendar – the one used by LY – to check how important it is to vary the dates of IT adoption. 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 reduce 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 a one-year lag in policy effects matches better with MSH's calendar.

In column (5), 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. The direct effect of IT is -0.77 percentage point, lower than the -1.37 percentage points from column (3), but is still significant at 6% significance.

The estimates in columns (6) to (8) are based on U20-8501 and U20-8009 (described in Table 1, columns (3) and (4)). They are unbalanced panels of the 20 countries studied by BS and derived from a vintage data set other than that of Ghosh *et* *al.* (2002) (as described in Appendix A). The results in column (7) are comparable to BS's IT irrelevance findings (in their Table 6.3), given that BS's IT adoption dates are used in it. From the comparison of columns (6) and (7), 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, but it is still significant.

Although smaller than column's (6) estimates for data until 2001, in column (8), 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 incorporated best IT practices during the 2000s, as indicated by Geraats (1998), among others.

Regarding the goodness of fit statistics, 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 (LSDV, in columns (3)-(8)), respectively represented by the p-values of the AR(1) and AR(2) test statistics. Besides weakening suspicion that the significances found are the spurious result of autocorrelated residuals, these tests show that the one-year period frequency suits the AR(1) model parameterization well.

If one agrees that the set of variables controlled for by LY is just the right set of covariates, their inclusion in regression (6) 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", or the "confidence effect", as in Hutchison and Walsh (1998). 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.[8] 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.

Further, with respect to columns (3) to (8) of Table 2, it can be seen that besides lagged inflation, output growth and terms of trade growth, the 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 regime, 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 that 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 explaining inflation in Table 2, and return to a more parsimonious adaptive Phillips curve, where:

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

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[8] The significance of CBTURN5 in sample U20-8501 might be the consequence of two facts. First, as described in Appendix A, 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 relatively high inflation and low governor turnover, is not included.

with the $CBTURN5_{n,t-1}$ kept in the equation to isolate the IT regime from other observable differences of the central bank.[9]

<Insert **Table 3** around here>

The IT effects shown in Table 3 only change slightly from the pattern of Table 2, generally becoming stronger and more significant. In columns (1) to (3), I again show how non-observables affect the inflation process. Once again, different from LY's irrelevance conclusion, column (3) presents significant IT accomplishments, and the IT effects are stronger in column (7), in contrast with BS's irrelevance conclusion.

To analyze how serious the dynamic panel bias problem is, in columns (4) and (8) I show Difference GMM estimates that treat the $Z_{n,t-1}$ covariates as strictly exogenous (DGMM-S). Note that the autoregressive coefficients are not significantly different from their LSDV pairs (respectively in columns (3) and (7)), and that the IT effect does not change significantly either. Because it is reasonable to suspect that past inflation shocks feed back into future realizations of the other explanatory variables, I also show Difference GMM estimates with $Z_{n,t-1}$ predetermined explanatory variables (DGMM-P in columns (5) and (9)). In spite of some loss of precision and unreasonably large absolute sizes for the IT coefficients, caused by over-fitting (too many moments relative to countries), the IT effect remains significant.

<Insert **Table 4** around here>

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[9] I am aware that, in the intuitive case where successful IT adoption increases central bank independence and this independence decreases inflation, the inclusion of CBTURN5 in an equation to measure 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 effect of IT on inflation.

In Table 4, I estimate the IT achievements on the inflation and output growth volatilities, as in equation (1), obtaining results that are comparable to those of BS's and LY's tests. The inflation and output growth standard deviations went down because of IT, although not as much as the inflation levels. Given that most countries were going through unavoidable volatility to lower their long-run mean inflation, the fact that IT adoption did not cause an increase in volatility is actually evidence of IT's improvement in macroeconomic performance.

To rule out the suspicion that the results presented so far spuriously reflect a relatively more important role played by supply shocks across the pre- and post-IT periods, and to confirm it is truly the consequence of a more credible policy, I first show that similar results hold when I allow for time and cross-section heterogeneity in the inflation sensitivity to output growth. In columns (6) and (10) of Table 3, I make the output growth interact with (i) the standard IT dummy, $d_{n,t-1}^{IT} = L.Inflation\ targeting_{n,t}$; (ii) a dummy variable equal to 1 for all countries in every period t after the average year of IT adoption, $Post\ 1994_{n,t}$; and (iii) a dummy variable equal to 1 for all year*s* in every country n that adopts IT, $Future\ ITers_{n,t}$. The fact the IT intercept is still negative and significant when the inflation responses to output are allowed to be different between ITers and non-ITers (*L.GDP growth*Future ITers*), to vary across pre- and post-IT periods in general (*L.GDP growth*Post 1994*) and to change after IT adoption for the ITers (*L.GDP growth*L.Inflation targeting*), excludes spurious effects of decreases in the variance of supply shock that may have occurred at different times in the different countries.

Second, and more intuitively, I study the impact of IT on the nominal long-term interest rate process, believed to be a proxy for long-run inflation expectations. Because the nominal long-term rate reflects not only expectations of future inflation, but also future activity levels, monetary policy tightness and the risk premiums related to those factors, I model it as:

$$\begin{aligned} long_{n,t} = \alpha^{L} \cdot long_{n,t-1} + \beta^{L,IT} \cdot d^{IT}_{n,t-1} + \gamma^{L}_{short} \cdot short_{n,t-1} \\ + \gamma^{L}_{gdpg} \cdot GDPG_{n,t-1} + \gamma^{L}_{\pi} \cdot \pi_{n,t-1} \\ + \delta^{L}_{t} + \eta^{L}_{n} + \upsilon^{L}_{n,t}; \end{aligned} \qquad (8)$$

where: $long$ is the nominal long-term interest rate and $short$ is the nominal short-term interest rate.[10] In Table 5, as in the inflation case, the IT lowering impact on the long rate is already significant by simple OLS in column (1) and, although it becomes unimportant with common-time effects in column (2), it confirms its significance when the country-fixed effects are also included. IT had a significant direct impact on the average long-term interest level of around -0.5 percentage point in columns (1), (3) and (6), or an implied cumulative effect close to -2.00 percentage points, in spite of the simultaneous decrease in risk premiums happening among future Euro members.[11] The results are similar for Difference GMM, although the autoregressive coefficient seems overestimated in column (4). When I additionally control for the Maastricht Treaty effect, the IT effect becomes even more significant in columns (5) and (8).

<Insert **Table 5** around here>

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[10] I thank Niamh Sheridan for providing the nominal long-term interest rate data used in BS.

[11] See Pagano and von Thadden (2004) for the convergence of risk premiums during the Euro adoption.

It is worth noting that the results presented so far are not incoherent with Brito (2010), who, in a debate with Gonçalves and Carvalho (2010), shows that IT disinflations were not less costly in the OECD. They discuss Ball's (1994b) disinflations, i.e. events in which the nine-quarter moving average of inflation went down by more than 2%, when it is better to have a lower output-loss-to-inflation-reduction ratio, called output sacrifice ratio. However, a lower output sacrifice ratio in Ball's (1994b) context is not a necessary or a sufficient condition for a policy to be considered more efficient in general, when not only sizable disinflations but the whole business cycle has to be handled by monetary policy. In other words, the fact that IT did not matter for "big" disinflations does not imply that IT does not matter at all. And, indeed, the 2% cut-off used in these studies is too coarse for the already low OECD inflation levels at the IT inception. Instead, given the OECD economies adopted IT more as a regime to fine tune the whole business cycle than a transient mean to disinflate, the current paper evaluates the average effect of the IT treatment on inflation and output, conditional on their continuing tradeoffs and on other covariates.

Nevertheless, the current paper's findings are qualitatively different from those of Brito and Bystedt (2010), who conclude that IT rendered no credibility bonus among emerging economies. According to Brito and Bystedt (2010), the emerging ITers' relative inflation reduction was the compensation for their relatively lower output growth. Given the early 1990s' prevalent gap in credibility between industrial countries' monetary authorities and those of emerging economies, both papers' results put together seem to illustrate Ball's (1995) imperfect credibility theory, where inflation control costs less for more credible central banks. They also provide support to Bernanke and Woodford's

(2005) and Mishkin's (2000) suspicions that IT works better in already strong institutional environments, and is thus not a panacea for emerging economies.

2.2. Ball and Sheridan (2005) Revisited

In Table 6, I revisit BS's 20 country cross-section difference-in-difference regressions. First, to show that their methods with my data also produce the irrelevance result for IT, I present estimates by equation (2) for inflation, GDP growth and long-term interest rates, respectively in columns (1) to (3) of panel 6.A, which are comparable to columns (6) of BS' Tables 6.3.B, 6.6.B and 6.8.B. In percentage points, the IT impacts are close to -0.50 on inflation, 1.00 on output growth and 0.20 on the long-term interest rate, all insignificant at 10% significance level, as in BS.[12]

<Insert **Table 6** around here>

Second, in an attempt to control for common-time-variable and country-fixed non-observables, I subtract these effects from the variables and re-estimate equation (2) in panel 6.B. Even though the cross-section difference-in-difference set-up does not permit accurate control of the processes' short-run dynamics and tradeoffs with other observable covariates, the IT effects become economically important. The IT simultaneous enhancing effects to relative inflation and relative output growth confirm the credibility bonus, suggesting additionally a context of relatively perfect credibility close to Ball (1994a), where (relatively) credible disinflations can cause (relative) economic booms.

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[12] Although I used BS' data for the nominal long-term interest rate, I do not get their result exactly (0.17 instead of 0.20) because I do not exclude any year from the sample, like they do.

2.3. Lin and Ye (2007) Revisited

As explained in section 1 (*and detailed in Appendix C for the referee's benefit*), LY's PSM application to a panel is not standard. Instead of one unit of treatment per ITer (7 countries), they consider every year-country under IT as an independent unit to be treated (totaling 45 events). Next, they match these units by their contemporaneous year observable characteristics, instead of by their pre-treatment characteristics, and base their evaluation on that same year inflation, $E\left[\pi^{1}_{ni,t'} \mid d^{IT}_{ni,t'}=1, p\left(\pi_{ni,t'-1}, X_{ni,t'}\right)\right]-E\left[\pi^{0}_{n,\tau} \mid d^{IT}_{n,\tau}=0, p\left(\pi_{n,\tau-1}, X_{n,\tau}\right)=p\left(\pi_{ni,t'-1}, X_{ni,t'}\right)\right]$ for all $t' \geq k_{ni}$ and τ, including $(t'-1) > k_{ni}$. By violating the CIA, LY's results are subject to the selection bias they propose to solve. The fact that current ITers have (or do not have) current lower inflation rates, as measured by LY, is not conditional on ITers and matched controls being similar before IT adoption. In other words, for the purpose of inferring the IT treatment effectiveness, it does not imply that a country is more (or less) likely to lower its inflation if it becomes an ITer.

<Insert **Table 7** around here>

In Table 7, I reformulate LY's Probit model of IT adoption (replicated in column (0); *or in Table C.1 for the referee's benefit*), fulfilling the requirements of: (i) one unit of treatment per ITer and (ii) matching of 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 ITer calculated by LY, ITer observations are included in the Probit estimation only if the country had not adopted IT previously, for $\tau \leq k_{ni}$ – i.e., ITers observations are not used in the Probit estimation after the first year of treatment, for $t' > k_{ni}$. To assure exogeneity, all seven observable variables controlled for by LY are lagged by one year, $p(\pi_{n,\tau-1}, X_{n,\tau-1})$ instead of $p(\pi_{n,\tau-1}, X_{n,\tau})$. Finally, because I will match at horizon h, I need the outcome variable to exist at year $(\tau+h)$ and have to avoid ITers' outcomes already under treatment from becoming controls for themselves. I thus drop the observations of all countries dated $\tau > (T-h)$ and the observations of pre-treatment ITers' years from which the term h ends up in their treatment periods. These two latter precautions generate a different subsample and its respective Probit for each horizon h.[13]

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, from columns (1) to (5), I present the IT adoption Probit estimates, $E[d_{n,\tau}^{IT} | \pi_{n,\tau-1}, X_{n,\tau-1}] = p(\pi_{n,\tau-1}, X_{n,\tau-1})$, for each of these five subsamples.[14] 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 likely to adopt IT. This is in accordance with LY, but runs counter to BS' assumption that IT adopters had higher inflation, at least on the eve of IT adoption. Among the seven observables studied by LY, inflation was the only one they lagged and is the only one that has preserved its sign and significance. Compared to LY's Table 2, broad money growth, central bank turnover and pegged exchange rate regimes are now

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[13] For illustrative purposes, take as examples the sample B22-8499, the inflation difference two years after IT adoption $ATT_2 = E[\pi_{n,k+2}^1 | d_{n,k}^{IT} = 1, p(\pi_{n,k-1}, X_{n,k-1})] - E[\pi_{n,\tau+2}^0 | d_{n,\tau}^{IT} = 0, p(\pi_{n,\tau-1}, X_{n,\tau-1})]$, and Sweden's IT adoption in year $k_{ni}=1995$. Because $\pi_{n,\tau+2} \mid p(\pi_{n,\tau-1}, X_{n,\tau-1})$ does not exist in the sample for $\tau=1998$ *and* 1999, all countries observations for those years are dropped. Given Sweden's just-before-treatment observables in $(k_{ni}-1)=1994$ are similar to Sweden's observables in $(\tau-1)=1993$ *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.

[14] To check for the balancing of the covariates, in Appendix B, Table B, I present t-tests of differences between treated and control groups.

insignificant. Government balance surplus becomes negative and significant, with the non-intuitive implication that IT adopters have looser fiscal policies.[15] A greater difference, and with an important implication, is that sensitivity to per capita GDP growth changes to negative and significant in most of the subsamples, meaning that countries with weaker output growth are also more probable to adopt IT in the sample. Although PSM protocols would suggest the exclusion of observables that do not influence IT adoption or inflation, since my objective in this subsection is just to review LY's results, I keep all of them to the end.[16]

As stated before, another criticism to LY is that in trying to make 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 the targets – and such 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 monetary policy tradeoffs, I present estimates of the IT policy effects for inflation and output growth in Tables 8 and 9. 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 0 in Table 8 replicates LY's approach (*also reproduced in Appendix C, Table C.2 for the referee's benefit*) and panel F presents the five-year average $\overline{ATE}_5$ and $\overline{ATT}_5$ values, calculated according to equation (4).

<Insert **Tables 8** and **9** around here>

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[15] 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.

[16] See also Vega and Winkelried (2005) and Mishkin and Schmidt-Hebbel (2002) for different propensity score models of IT adoption. The qualitative differences of this paragraph's results, as well as those of LY, BS and other authors regarding the determinants of IT adoption, suggest further research is necessary.

From Tables 8 and 9, 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 8, and the *ATT*s for output growth are positive in 30 out of 35 matches in Table 9. If one looks at the five-year average $\overline{ATT}_5$, calculated from equation (4) in panels F, to infer the effects of monetary policy on the inflation-output tradeoff, all inflation-output growth pairs of differences have a negative-positive sign, respectively meaning that lower inflation was achieved with higher growth. As in subsection 2.2, this is an unexpected result from the perspective of an inexorable short-run positive inflation-output growth tradeoff, and closer to Ball's (1994a) credible disinflation. Except for the "local linear regression", ITers presented an average inflation around 1 percentage point lower and/or an average output growth one percentage point higher in the first five-years of IT treatment, with one, if not both, of the outcome differences significant at the 99% confidence level.

3. Conclusion

In a comprehensive survey on the empirical evidence on IT, Walsh (2009, p. 195) concludes that "… macroeconomic experiences among both inflation targeting and non-targeting developed economies have been similar," a puzzling contrast with policymakers' enthusiasm for 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 the industrial country data studied by BS and LY, with some sample extensions. Through a dynamic panel that controls for observables and non-observable covariates, I find significant evidence that IT positively mattered for pioneering industrial ITers, meaning that IT achieved lower inflation without compromising output, similar to Ball's (1994a) credible disinflation. These IT achievements survive in the face of several kinds of controls, sample variations and to the different IT dating calendars tested.

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 that 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, subject to the limitations imposed by the cross-section difference-in-difference and PSM frameworks, if one approximately controls for the existing macroeconomic differences on the eve of IT adoption, BS's and LY's irrelevance results are overturned, concluding that IT did improve 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	B22-7199	U20-8501	U20-8009	Converging-IT adoption date:	Converging-IT adoption date:
	(1)	(2)	(3)	(4)	(5)	(6)
Australia	1984-1999	1971-1999	1985-2001	1980-2009	1995	1994
Austria	1984-1999	1971-1999	1985-1998	1980-1998		
Belgium	1984-1999	1971-1999	1985-1998	1980-1998		
Canada	1984-1999	1971-1999	1985-2001	1980-2009	1991	1992
Denmark	1984-1999	1971-1999	1985-2001	1980-2009		
France	1984-1999	1971-1999	1985-1998	1980-1998		
Finland	1984-1999	1971-1999	1985-1998	1980-1998	1993	1994
Germany	1984-1999	1971-1999	1985-1998	1980-1998		
Greece	1984-1999	1971-1999				
Iceland	1984-1999	1971-1999			2001	
Ireland	1984-1999	1971-1999	1985-1998	1980-1998		
Italy	1984-1999	1971-1999	1985-1998	1980-1998		
Japan	1984-1999	1971-1999	1985-2001	1980-2009		
Netherlands	1984-1999	1971-1999	1985-1998	1980-1998		
New Zealand	1984-1999	1971-1999	1985-2001	1980-2009	1990	1990
Norway	1984-1999	1971-1999	1985-2000	1980-2009	2001	
Portugal	1984-1999	1971-1999	1985-1998	1980-1998		
Spain	1984-1999	1971-1999	1985-1998	1980-1998	1995	1995
Switzerland	1984-1999	1971-1999	1985-1999	1980-2009	2000	
Sweden	1984-1999	1971-1999	1985-2001	1980-2009	1995	1995
United Kingdom	1984-1999	1971-1999	1985-2001	1980-2009	1992	1993
United States	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 (6) - rates in %

Sample and authors'	B22-8499				B22-7199	U20-8501		U20-8009
dates of IT	MSH			BS	MSH		BS	MSH
Estimator:	OLS	TE-OLS	LSDV					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
L.Inflation targeting	-0.78***	-0.27	-1.37***	-1.01**	-0.77*	-0.99***	-0.59*	-0.68***
	(0.24)	(0.24)	(0.35)	(0.38)	(0.40)	(0.25)	(0.32)	(0.14)
L.Central bank turnover rate	0.81	1.03	2.00	1.94	-0.05	1.56**	1.45**	0.52
	(1.47)	(1.38)	(1.66)	(1.67)	(1.19)	(0.66)	(0.65)	(0.49)
L.Pegged regimes	0.53	0.51	0.51	0.52	0.80	0.57	0.58	0.37
	(0.36)	(0.35)	(1.31)	(1.34)	(0.53)	(0.62)	(0.78)	(0.29)
L.Trade openness	-0.01***	-0.01**	0.03	0.03	-0.03	0.02	0.01	0.01
	(0.00)	(0.00)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
L.Money growth	0.00	-0.01	-0.03	-0.03	0.01	0.01	0.01	0.00
	(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**	-0.01	0.01	0.02	0.01
	(0.04)	(0.05)	(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*	-0.04*	-0.06***	-0.06***	-0.03**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)
L.GDP growth	0.19**	0.18**	0.14**	0.14**	0.15***	0.19***	0.20***	0.16**
	(0.07)	(0.08)	(0.07)	(0.07)	(0.04)	(0.06)	(0.06)	(0.06)
L.Inflation	0.54***	0.53***	0.40***	0.40***	0.51***	0.56***	0.57***	0.57***
	(0.09)	(0.09)	(0.11)	(0.11)	(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	0.06	0.00	0.01	0.00
AR(2) test	0.50	0.52	0.81	0.82	0.23	0.85	0.87	0.21
ICE of IT	-1.69***	-0.57	-2.28***	-1.68***	-1.56*	-2.24***	-1.36*	-1.58***
Observations	329	329	329	329	595	275	275	442
Countries	22	22	22	22	22	20	20	20
Adj. R2	0.81	0.83	0.76	0.76	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 (LSDV) in columns (3)-(8), 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 $\text{dummy}_{t-1}/(1-\text{Inflation}_{t-1})$, with significance tested by a nonlinear test. Column (5) 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 (7) - rates in %

Sample and authors'	B22-8499						U20-8501			
dates of IT	MSH						BS			
Estimator:	OLS	TE-OLS	LSDV	DGMM-S	DGMM-P	LSDV	LSDV	DGMM-S	DGMM-P	LSDV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
L.Inflation targeting	-0.81***	-0.31*	-1.26***	-1.14***	-7.73**	-1.36***	-0.73**	-0.56**	-2.01*	-1.12**
	(0.23)	(0.15)	(0.30)	(0.27)	(3.80)	(0.22)	(0.27)	(0.22)	(1.14)	(0.50)
L.Central bank	0.87	1.02	1.56	1.44	4.56*	1.52	1.19*	1.21**	1.42	1.24**
turnover rate	(1.41)	(1.30)	(1.63)	(1.47)	(2.54)	(1.66)	(0.61)	(0.48)	(0.96)	(0.54)
L.GDP growth	0.17**	0.15**	0.16**	0.17**	0.21*	0.15	0.22***	0.22***	0.19***	0.19*
	(0.06)	(0.07)	(0.07)	(0.07)	(0.12)	(0.11)	(0.05)	(0.05)	(0.06)	(0.10)
L.Inflation	0.55***	0.53***	0.39***	0.50***	0.43***	0.39***	0.63***	0.64***	0.58***	0.62***
	(0.10)	(0.10)	(0.12)	(0.13)	(0.13)	(0.12)	(0.04)	(0.03)	(0.07)	(0.04)
L.Terms of trade	-0.03	-0.04	-0.06*	-0.06*	-0.05	-0.06*	-0.06***	-0.06***	-0.05***	-0.06***
growth	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)
L.GDP growth						0.05				0.10
*L.Inflation targeting						(0.13)				(0.13)
L.GDP growth *Post						-0.10				-0.07
1994						(0.12)				(0.11)
L.GDP growth						(0.10)				(0.11)
*Future ITer						(0.15)				(0.12)
L.High inflation	7.47***	7.37***	8.39***	7.19***	9.11***	8.44***				
regime	(1.82)	(1.82)	(2.00)	(2.08)	(1.23)	(1.96)				
AR(1) test	0.72	0.73	0.09	0.09	0.09	0.09	0.01	0.01	0.01	0.01
AR(2) test	0.46	0.47	0.93	0.98	0.82	0.92	0.68	0.69	0.60	0.70
ICE of IT	-1.79***	-0.66*	-2.06***	-2.26***	-13.54*	-2.21***	-1.95**	-1.58**	-4.79**	-2.96**
Observations	330	330	330	330	330	330	287	287	287	287
Countries	22	22	22	22	22	22	20	20	20	20
Adj. R2	0.81	0.82	0.75			0.75	0.69			0.69

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 (LSDV) in columns (3), (6), (7) and (10), with robust standard errors clustered by country in parentheses. Two-step difference GMM of Arellano and Bond (1991) with strictly exogenous Z_{t-1} in columns (4) and (8) (DGMM-S) and predetermined Z_{t-1} in columns (5) and (9) (DGMM-P). *, ** 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.

Table 4 - Estimates of the volatility equations (1) - rates in %

Dependent variable:	Inflation standard deviation				Output growth standard deviation			
	B22-8499		U20-8501		B22-8499		U20-8501	
	MSH		BS		MSH		BS	
	LSDV	DGMM-S	LSDV	DGMM-S	LSDV	DGMM-S	LSDV	DGMM-S
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
L.Inflation targeting	-0.31	-0.27	-0.55*	-0.52*	-0.16	-0.10	-0.30	-0.29*
	(0.28)	(0.22)	(0.30)	(0.28)	(0.23)	(0.23)	(0.18)	(0.16)
L.Dependent variable	0.51***	0.58***	0.19**	0.25***	0.49***	0.65***	0.32***	0.51***
	(0.10)	(0.09)	(0.09)	(0.09)	(0.06)	(0.07)	(0.04)	(0.07)
L.High inflation regime	0.69*	0.58			0.74***	0.44**		
	(0.39)	(0.37)			(0.15)	(0.20)		
AR(1) test	0.06	0.05	0.01	0.01	0.22	0.22	0.00	0.00
AR(2) test	0.72	0.77	0.02	0.02	0.61	0.56	0.01	0.02
ICE of IT	-0.63	-0.65	-0.68*	-0.70*	-0.31	-0.29	-0.44	-0.59*
Observations	286	286	247	247	286	286	247	247
Countries	22	22	20	20	22	22	20	20
Adj. R2	0.52		0.23		0.3		0.28	

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. Pooled cross-sectional OLS, including time and country effects (LSDV) in columns (1), (3), (5) and (7), with robust standard errors clustered by country in parentheses. Two-step difference GMM of Arellano and Bond (1991) with strictly exogenous Z_{t-1} in columns (2), (4), (6) and (8) (DGMM-S). *, ** 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 first-differenced residuals in all columns.

Table 5 - Estimates of the long-term interest rate equation (8) - rates in %

Sample and authors'	B22-8499					U20-8501		
dates of IT	MSH					BS		
Estimator:	OLS	TE-OLS	LSDV	DGMM-S	LSDV	LSDV	DGMM-S	LSDV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
L.Inflation targeting	-0.45**	0.00	-0.45*	-0.63**	-0.48**	-0.47*	-0.46*	-0.47**
	(0.16)	(0.13)	(0.24)	(0.23)	(0.21)	(0.25)	(0.26)	(0.22)
L.GDP growth	0.06	0.03	0.03	0.04	0.03	0.13**	0.13***	0.14***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)
L.Inflation	-0.05	-0.02	0.03	-0.03	0.04	0.13*	0.13***	0.14*
	(0.08)	(0.05)	(0.07)	(0.04)	(0.07)	(0.07)	(0.04)	(0.07)
L.Short-term interest rate	0.09***	0.01	-0.06	-0.15***	-0.07	-0.07*	-0.12**	-0.06*
	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)
L.Long-term interest rate	0.80***	0.82***	0.76***	0.96***	0.76***	0.74***	0.74***	0.74***
	(0.05)	(0.04)	(0.07)	(0.05)	(0.06)	(0.04)	(0.07)	(0.04)
L.Maastricht Treaty					-0.54**			-0.57***
					(0.22)			(0.14)
L.High inflation regime	3.52***	3.93***	3.88***	4.28***	3.92***			
	(0.62)	(0.57)	(0.53)	(0.46)	(0.58)			
AR(1) test	0.45	0.88	0.01	0.02	0.01	0.00	0.00	0.00
AR(2) test	0.45	0.26	0.96	0.81	0.95	0.15	0.17	0.15
ICE of IT	-2.18**	-0.01	-1.89*	-14.53	-1.98*	-2.32*	-1.79*	-1.80*
Observations	313	313	313	313	313	287	287	287
Countries	22	22	22	22	22	20	20	20
Adj. R2	0.89	0.94	0.90		0.90	0.91		0.91

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 (*Long-term interest rate* $_t$). . Pooled cross-sectional OLS (OLS) in column (1), including time-variable effect (TE-OLS) in (2), and time and country effects (LSDV) in columns (3), (5), (6) and (8), with robust standard errors clustered by country in parentheses. Columns (3) and (6) use the two-step difference GMM of Arellano and Bond (1991) with strictly exogenous Z_{t-1} (DGMM-S). *, ** 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.

Table 6 - Cross-section regressions of the mean annual rates (in %) - sample U20-8501

Dependent variable:	Inflation (1)	Output-growth (2)	Long-term interest (3)
	A. Levels		
Inflation targeting	-0.51	1.00	0.17
dummy	(0.32)	(0.77)	(0.43)
L.Dependent variable	-0.80***	-0.87**	-0.66***
	(0.06)	(0.40)	(0.06)
Constant	1.28***	2.24*	3.00***
	(0.28)	(1.27)	(0.63)
Adj. R2	0.93	0.33	0.87
	B. Mean-deviations		
Inflation targeting	-2.58**	1.64**	-1.29
dummy	(1.20)	(0.76)	(1.40)
L.Dependent variable	2.08	-1.11	1.06
	(4.01)	(2.29)	(3.67)
Constant	0.56	-0.61	0.25
	(0.72)	(0.45)	(0.72)
Adj. R2	0.13	0.14	-0.06

Notes: Cross-sectional OLS estimates on levels data in panel A and on mean-deviations transformed data in panel B. Mean-deviations to each variable are computed by subtracting the country and time effects according to Vega and Winkelried (2005). *, **, and *** indicate the significance level of 10%, 5%, and 1% respectively, infered from OLS standard errors in parentheses. The data used in this table for inflation and output growth is from the same source but a different vintage than Ball and Sheridan's (2005). The nominal long-term interest rate data was kindly provided by Niamh Sheridan.

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

Outcome variable:	Lin and Ye's ❖ (0)	Contemporaneous (1)	One year ahead (2)	Two years ahead (3)	Three years ahead (4)	Four years ahead (5)
L.Inflation	-0.1867***, ###	-0.1584*, #	-0.1916**, ##	-0.2265**, ##	-0.2287**, ##	-0.2444**, ##
	(0.0570)	(0.0880)	(0.0883)	(0.0886)	(0.0914)	(0.0995)
	[0.0862]	[0.0959]	[0.0956]	[0.1020]	[0.1044]	[0.1089]
L.Trade openness	-0.0004	-0.0073	-0.0064	-0.0079	-0.0085	-0.0099
	(0.0031)	(0.0056)	(0.0061)	(0.0064)	(0.0065)	(0.0064)
	[0.0081]	[0.0070]	[0.0086]	[0.0090]	[0.0091]	[0.0090]
L.Money growth	-0.0381**	-0.0097	-0.0093	-0.0079	-0.0078	-0.0079
	(0.0181)	(0.0294)	(0.0289)	(0.0295)	(0.0296)	(0.0296)
	[0.0240]	[0.0292]	[0.0296]	[0.0295]	[0.0295]	[0.0293]
L.Central bank turnover rate	-0.0247***	0.6676	0.5952	0.2621	0.1060	0.0111
	(0.0079)	(0.6491)	(0.6787)	(0.7805)	(0.8072)	(0.8905)
	[0.0153]	[0.7018]	[0.7418]	[0.8965]	[0.9329]	[1.0412]
L.GDP per head growth	0.0298	-0.1382	-0.2334**, ##	-0.3557**, ##	-0.3509**, ##	-0.3476**, ##
	(0.0296)	(0.0873)	(0.1104)	(0.1641)	(0.1607)	(0.1613)
	[0.0369]	[0.1013]	[0.1076]	[0.1659]	[0.1625]	[0.1628]
L.Government balance	0.0047	-0.1424***, ###	-0.2133***, ###	-0.2457***, ##	-0.2393***, ##	-0.2369***, ##
	(0.0297)	(0.0527)	(0.0733)	(0.0888)	(0.0883)	(0.0904)
	[0.0445]	[0.0429]	[0.0656]	[0.0985]	[0.0981]	[0.1007]
L.Pegged regimes	-0.0064***	-0.2269	-0.6150	-0.6333	-0.5928	-0.5567
	(0.0026)	(0.4815)	(0.6500)	(0.7345)	(0.7398)	(0.7506)
	[0.0059]	[0.4205]	[0.7182]	[0.8177]	[0.8251]	[0.8393]
No. of obs.	321	289	267	245	223	201
No. of treated	45	7	7	7	7	7
Pseudo-R^2	0.22	0.23	0.30	0.35	0.35	0.36

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

❖ In accordance with Lin and Ye (2007), in column (0), Inflation is the only independent variable actually lagged, all other independent variables being contemporaneous.

Table 8 - Matching estimates of treatment effect of IT on inflation rates - sample B22-8499

	ATE	*ATT*						
Matching estimator:	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)
8.0. Replication of Lin and Ye's (2007) treatment effect of non-constant IT on CPI growth								
Difference		-0.09	-0.08	-0.18	-0.18	-0.31	-0.19	-0.22
S.E.		(0.36)	(0.28)	(0.55)	(0.30)	(0.35)	(0.55)	(0.54)
No. of treated		45	45	45	44	38	45	45
No. of controls		35	79	276	189	120	276	276
8.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
8.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
8.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
8.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
8.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
8.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 9 - Matching estimates of treatment effect of IT on per capita GDP growth rates - sample B22-84

	ATE	*ATT*						
Matching estimator:	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)
				9.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
				9.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
				9.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
				9.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
				9.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
				9.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. Sample descriptions and data sources:

A.1. Samples' description

I examine samples of industrial countries similar to BS's and LY's, in which evidence of IT effectiveness in improving inflation and output growth was not found. The BS sample is composed of 20 major OECD economies, divided into a treatment group of 7 ITers: Australia, Canada, Finland, New Zealand, Spain, Sweden and United Kingdom; and a control group of 13 non-ITers: Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Switzerland and the United States. The LY 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 period. Given that monetary policy is assumed to be neutral in the long run, it is at IT inception that we are better able to evaluate its real short-run effects and tradeoffs. Moreover, with the passing of time, many of the novel operating procedures introduced by the pioneering IT central banks were 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. Besides this, after 1999, 11 of these 22 industrial countries unified their monetary policy under the European Central Bank, which makes it debatable whether they should be analyzed as independent countries from then on.

To provide comparison with those previous works and robust evidence, I study four 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)). As in LY, my data come from Ghosh *et al.* (2002), who derived their data from IMF sources as described in A.2.

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 (2)), useful to address the concerns that the results were influenced by the short length of the time period observed.

The third sample, U20-8501 (in Table 1, column (3)), is an unbalanced panel from 1985 to 2001 of the 20 countries studied by BS. As theirs, 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 fourth sample, U20-8009 (in Table 1, column (4)), is an extension of U20-8501 that starts in 1980 for all 20 countries. It still covers the 10 Euro area countries until 1998, but covers the other 10 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 A.2.

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), the 1980s (Ireland, Italy, Portugal, New Zealand), and the early 1990s (Greece, Iceland). Given that I work 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 annual inflation above 20%, and zero, from then on. The variable $big40_{n,t}$ is a standard dummy that equals one in the year 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 literature diverges on when to date the adoption of IT (for examples of different IT calendars, see Johnson (2002), Corbo *et al.* (2002), Roger and Stone (2005), BS, and MSH). In column (5) of Table 1, MSH's IT adoption dates are the closest to country-case studies of Bernanke *et al.* (1999) and the IMF (2006). They also better represent the common central bank operating procedure of commencing an IT regime with explicit targets for the coming year onward rather than for the current year, as described in Johnson (2002). The time lag recognizes the stylized fact documented by Batini and Nelson (2002) 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 column (6) of Table 1.

A.2. Data sources

The data for samples B22-8499, B18-8499 and B22-7199 come 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 the notation used in Ghosh's *et al.* (2002), the variables that LY and I use are: CPIG – 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 five 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 similar 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 the year from the WEO by the IMF. The series of real GDP growth rates is the average of the year from the WEO by the IMF in U20-8501 (for comparison purposes with BS) and the annual growth of GDP per head (expenditure approach, in US$, constant prices, constant PPPs) from the OECD in U20-8009. The series of annual 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 revise and extend Ghosh's *et al.* (2002) CBTURN5 until 2009, using the information available in the database of Dreher's *et al.* (2010) (available online), which covers central bank governor turnovers in 140 countries during 1970-2009. Like Ghosh *et al.* (2002), who follow Cukierman (1992), I construct the updates of the variable as the number of central bank governor changes within a five-year period, divided by 5 to give the annual rate of turnover. The variable is constructed for fixed five-year periods, and assigned to each year within that period. During this update process, I found some discrepancies in Ghosh *et al.* (2002), probably due 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 used CBTURN5 exactly as made available in Ghosh *et al.* (2002) for B22-8499, B18-8499 and B22-7099. I then used my updated measure in samples U20-8501 and U20-8009.

Appendix B. t-tests

To check for the balancing of the covariates in the five subsamples, in Table B, 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 B, after matching, the 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. [17]

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[17] *pstest* does not make sense in the local linear regression matching (in column 7) because it first smoothes 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 presentation of the results.

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

	t-stat. (p-value)							
Matching estimator:	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 B cont. -
***t* -Tests and p-values of differences between treated and untreated - sample B22-8499 - rates in %**

	t-stat. (p-value)							
Matching estimator:	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.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 B 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

Appendix C. Lin and Ye replication (*provided for the referee's benefit*)

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

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 ITers 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.

Table C.1 - Probit estimates of propensity scores of IT adoption - sample B22-8499

Independent variables						
CPIG_1	OPEN	BMG	CBTURN5	GDPPCG	CGGDP	FIX
-18.696***,###	-0.041	-3.807**	-2.472***	2.982	0.468	-0.640***
(5.699)	(0.312)	(1.810)	(0.793)	(2.959)	(2.967)	(0.256)
[8.615]	[0.814]	[2.395]	[1.528]	[3.693]	[4.454]	[0.587]
Descriptive statistics -			No. of obs.:			321
			No. of treated:			45
			Pseudo-R2:			0.22

Notes: Probit estimate of the balanced panel as in Lin and Ye (2007). Constant term is 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.

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

In Table C.2, 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 does not change their results.

Table C.2 - Matching estimates of treatment effect on the level of inflation -sample B22-8499

	Nearest-neighbor matching	3-Nearest-neighbor matching	Radius matching			Local linear regression matching	Kernel matching
			r=0.03	r=0.01	r=0.005		
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

Notes: Average Treatment Effect on Treated (ATT) estimates of non-constant IT for the balanced panel as in Lin and Ye (2007). Expressed in decimail rates. 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 $H_1: ATT>0$.