Russia's 'flat tax'

SUMMARY

In 2001, Russia dramatically reduced its higher rates of personal income tax (PIT), establishing a single marginal rate at the low level of 13%. In the following year, real revenue from the PIT increased by about 26%. This 'flat tax' experience has attracted much attention (and emulation), making it perhaps the most important tax reform of recent years. But it has been little studied. This paper asks whether the strong performance of PIT revenue was itself a consequence of this reform, using both macro evidence and, in particular, micro level data on the experiences of individuals and households affected by the reform to varying degrees. It concludes that there is no evidence of a strong supply side effect of the reform. Compliance, however, does appear to have improved quite substantially – by about one third, according to our estimates – though it remains unclear whether this was due to the parametric tax reform or to accompanying changes in enforcement.

— Anna Ivanova, Michael Keen and Alexander Klemm

The Russian 'flat tax' reform

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1. INTRODUCTION

At the start of 2001, Russia unified its marginal rates of personal income taxation – previously at 12, 20 and 30% – at the single rate of 13%. In the year following the adoption of this 'flat tax',[1] revenue from the personal income tax (PIT) increased by about 46% (about 26% in real terms); relative to GDP, PIT revenues increased by nearly one-fifth. Such a strong revenue performance following a marked reduction of marginal tax rates quickly attracted attention and emulation in East Europe. While the Baltic countries had preceded Russia in introducing single rate income tax structures[2]

[1] This term has come to be used extremely loosely. The Russian version was not a flat tax in the sense of Hall and Rabushka (1995), which is essentially an expenditure tax implemented by combining a flat tax on wage income and a cash flow business tax levied at the same rate (although Rabushka (2003) has spoken positively of the Russian experience). More generally, the various 'flat taxes' adopted in Russia and elsewhere differ quite markedly from one another, including in whether the rate of corporation tax (or even VAT) is aligned with that of the PIT (with the Russian variant in this respect being one of the less dramatic reforms). Hence the inverted commas in our title.

[2] Outside Europe, Bolivia has had such a tax system since 1986. Other small states and territories (such as Jersey) have had flat rate income taxes for decades.

– Estonia leading the way in 1994 – the Russian experience was clearly the most influential. It was followed by the adoption of various forms of flat tax – the common element being a single, low marginal rate of PIT – in Serbia, the Ukraine and Slovakia (in 2003), and most recently by Georgia and Romania (in 2005), with rates ranging from 12 (Georgia) to 19% (Slovakia). Similar reforms have been under consideration in Belarus, Guatemala, the Kyrgyz Republic, El Salvador, Paraguay and Poland.[3] More recently, the 'flat tax' has come to feature prominently in policy debates in Western Europe and the United States.[4] The Russian reform has thus come to be extraordinarily influential, making it arguably the most important tax reform of the last decade.

Given the importance of the reform not only for Russia itself but also for the many countries that have adopted, or are considering adopting, similar measures, it is clearly important to understand the experience there, and the lessons that can appropriately be drawn from it. Did the reform indeed have the strong positive effects on compliance and/or labour supply (especially the former) that its advocates have claimed? Were these effects even so strong that the lower tax rates 'paid for themselves'?

The purpose of this paper is to address these and related questions, both by taking a macroeconomic perspective on wider revenue developments at this time and, in particular, by using the individual- and household-level panel data that are now available in the Russian Longitudinal Monitoring Survey (RLMS), spanning pre- and post-reform periods, to provide a clear assessment of the impact of the reform on tax revenue, work effort, wage rates and taxpayer compliance.

Though it has been much commented on, and admired, the Russian experience has been subject to very little rigorous empirical analysis.[5] The only econometric analysis of which we are aware is presented in a series of papers from the Institute for Economies in Transition. The empirical strategy in this work – as in Sinelnikov-Mourylev et al. (2003),[6] for instance – has been to use the RLMS to construct observations at the level of the regions of Russia and ask whether the implied PIT base has increased more in those regions where the weighted average marginal tax rate was most reduced. The conclusion drawn is that there has indeed been a significant effect of this sort, with the authors ultimately attributing about half of the revenue gain to the reduction in marginal rates. Though striking, these results are subject to a number of limitations. It could be the case, for instance, that those regions in which the proportion of incomes subject to the higher rates of tax prior to reform was greatest were also systematically those which saw, for some reason, the

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[3] As a variant, Armenia has redesigned its progressive PIT and regressive social insurance schedule so that the combination of the two has a single positive marginal rate.

[4] See, for example, the positive assessment in *The Economist* of 14 April 2005. A useful summary of recent flat tax proposals is in Grecu (2004).

[5] Informal accounts are provided in IMF (2002) and Chua (2003).

[6] See also Chapter 4 of Glavatskaya and Seryanova (2003).

greatest increase in the incomes of those subject to essentially the same marginal rate before and after reform (and hence also the greatest increase in the tax base). Micro-level panel data are needed to identify such possibilities, offering potentially the best basis upon which to assess the implications of the reform. That is the approach pursued here.

The concern in this paper, it should be stressed, is solely with positive aspects of the reform, in terms of its impact on revenue, compliance and labour supply; we do not attempt to gauge the extent of any efficiency or welfare gains, or to evaluate its distributional impact.[7] The focus is also only on relatively short-term effects, though it could be that full supply responses to the reform occur only after some time, either because of market rigidities, or because the reform was not initially perceived as permanent.

The structure of the paper is as follows. Section 2 describes the PIT and (important in understanding its effects) related tax reforms in 2001. Section 3 briefly reviews the lessons of theory as to the likely effects of the reform, and Section 4 takes a macro perspective on the assessment of the reform. The main analysis, based on micro panel data, is in Section 5, which describes the data and methodology used, and in Section 6, which reports results. Section 7 concludes.

2. PIT AND THE 2001 TAX REFORMS

This section describes the PIT reform and other tax changes that took place around the same time.

2.1. Reform of the taxation of income

The change in the rate structure of the PIT, which took effect on 1 January 2001, is summarized in Table 1.

The threshold level of taxable income at which the higher rates began prior to reform was high: about 187% of the average wage in 2000. It should be noted too that although the basic exemption grew by 30% in real terms between 2000 and 2001, it remained roughly unchanged relative to the average wage (at about 12%).[8]

Strictly, the post-reform PIT was not a single rate tax, since some kinds of income – from gambling, lottery prizes, some insurance payments, the benefit from loans obtained at less than market rates[9] and 'excessive' bank interest[10] – were taxed at

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[7] Sinelnikov-Mourylev *et al.* (2003) argue that reduced evasion (and hence higher tax payments) by higher-rate taxpayers actually increased the effective progressivity of the PIT with respect to wage income (while finding no conclusive result for its progressivity with respect to total income).

[8] Both before and after reform, this allowance was withdrawn in discrete jumps at higher levels of income (as described in the data appendix). This is taken fully into account in the empirical analysis reported below, but for simplicity ignored in the discussion that follows and in Table 1 and Figure 1.

[9] This was calculated as the difference between the interest implied by the market rate (calculated as 75% of the Central Banks' refinancing rate on the date of receipt of a ruble loan or 9% for loans received in foreign currency) and that actually paid.

[10] Bank interest became taxable if paid at a rate exceeding 75% of the Central Banks' refinancing rate on ruble deposits or 9% on foreign currency deposits. Since most deposits earned less than this, interest income was generally untaxed.

Table 1. The PIT rate structure before and after reform

Marginal rate before reform (2000)		After reform (2001)	
Bracket (rubles[a])	Rate	Bracket (rubles)	Rate
Below 3,168	0	Below 4,800	0
3,168 to 50,000	12	Above 4,800	13
50,000 to 150,000	20		
Above 150,000	30		

[a] For comparison, the average annual salary was 26,676 rubles in 2000 and 39,384 rubles in 2001. The official exchange rate expressed in rubles per US dollar was 28.13 in 2000 and 29.17 in 2001.

Source: Russian Tax Code, Part II.

35%, approximating combined rates of the PIT and unified social tax (discussed below), in an attempt to close popular avoidance schemes (some of which showed impressive adroitness).[11] For similar reasons, dividends were taxed at 30% (up from 15% in 2000) but with the introduction of a non-refundable credit for underlying CIT paid.

There were also changes in 2001 to the base of the PIT, with the elimination of various exclusions for military servicemen and expatriates and the introduction of a simplified system of deductions (standard, social, property and professional). More-over, there was a modification of the agreement for sharing PIT revenue between federal and regional governments: in 2000 regional governments received only 80% of PIT revenues, from 2001 they received 100%. This may have strengthened the collection incentive of regional governments.

Importantly, however, these changes to the PIT structure were not the only tax reform at this time. Most significant for present purposes, Part II of the new tax code also significantly altered the structure of social insurance payments, as shown in Table 2. Prior to the reform, separate contributions were paid to the pension, social, medical and employment funds at a combined rate, at all income levels, of 38.5% on the employer and 1% on the employee (this last to the pension fund). After the reform, a single 'unified social tax' (UST) was charged on the employer – for firms meeting various additional requirements[12] – at marginal rates decreasing from 35.6 to 5%, with the lowest marginal rate applying to salaries in excess of (the very high level of) 600,000 rubles.

[11] Under one scheme, for instance, the enterprise purchased insurance against a very low probability event (deducting its premiums). At the same time, its employees entered a contract with the same insurance company for a very high probability event. Employees thus received compensation in the form of an insurance payout, which was not taxable.

[12] To qualify for the regressive rate, the average payment per employee had to be above a threshold (2500 rubles in 2001) when a certain number of employees with the highest incomes were excluded from the calculation. The rationale for this was apparently to encourage compliance on a broad base by denying benefit to firms that declared only a few highly paid individuals. Moreover, to discourage income shifting, the regressive social scheme in 2001 could be applied only by taking into account average payment per employee in 2000.

Table 2. Social tax rate structure before and after reform[a]

Legal incidence	Before Reform (2000)		After Reform (2001)	
	Income range	Marginal rate	Income range	Marginal rate
Employee	All	1	All	0
Employer	All	38.5[b]	Below 100,000	35.6
			100,000–300,000	20
			300,000–600,000	10
			Above 600,000	5 (from 2002: 2)

[a] Different rates apply to agricultural workers, lawyers, self-employed and Northern ethnic communities. In some regions some additional charges were levied, for instance in Moscow an Education Levy of 1%.
[b] This was made up of contributions to the Pension Fund (28%), Social Insurance Fund (5.4), State Employment Fund (1.5), and Medical Insurance Fund (3.6).

Source: Russian Tax Code, Part II.
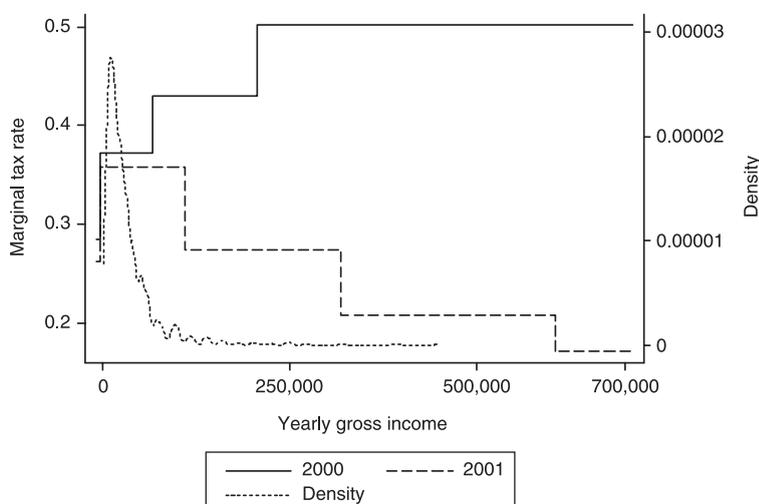
Figure 1. Marginal tax rates (including social taxes) before and after the reform

Note: The density shown is the kernel of the distribution of gross incomes in 2000. One individual reporting earnings of 2,353,564 rubles was dropped to improve clarity of the chart. (This individual did not participate in the 2001 survey, so is not included in the regression analysis either).

The combined effect of both reforms on effective marginal tax rates is shown in Figure 1, which plots marginal tax rates before and after the reform against the income level. For those initially paying PIT at the lower rate of 12% – a particularly important group for our later analysis—the net effect of the 2001 reforms was a reduction in the combined marginal rate of PIT and social insurance of about 1.3 percentage points.[13]

[13] Because the social taxes are charged on a tax-exclusive basis, this is calculated as the difference between (0.12 + 0.01 + 0.385)/(1.385) and (0.13 + 0.356)/(1.356).

Table 3. Major other changes in Russian Federation tax code in 2001

Type of tax	Rate	Base	Other changes
Corporate Income Tax	Combined maximum rate increased from 30 to 35%		Federal rate (11%) and regional rate (up to 19%) remained unchanged but municipalities were allowed to impose an additional rate of 5%
Value Added Tax	No changes	Scaling back of exemptions, including a narrowing of the exemption for pharmaceuticals	a) Shift from the origin to the destination basis for trade with other CIS countries (except Belarus, and on energy), b) adoption of measures directed at reducing the compliance burden for small traders.
Turnover taxes	The Social Infrastructure Maintenance Tax of 1.5% was abolished, and the Road User Tax reduced from 2.5 to 1.5%.		

2.2. Other tax changes

Several other tax changes that also took effect at the start of 2001 are summarized in Table 3.

Tax administration was also undergoing significant change at the time of the PIT reform, as described in Chua (2003). Through the latter 1990s, there is no doubt that the system was in something close to chaos, with very poor compliance, widespread use of tax offsets, and difficult relations between levels of government. Gaddy and Gale (2005) cite estimates that in the mid-1990s only 8% of large enterprises paid their tax bills in cash, 63% effectively paid in kind and the rest did not pay at all. Brooks (2001) quotes an estimate that 90% of private sector income was concealed from the tax authorities, and reports dramatic difficulties in collecting tax on the other 10%: in 1996, '26 tax collectors were killed, 74 were injured in the course of their work, 6 were kidnapped and 41 had their homes burnt down'. Salaries of tax officials were very low, contributing to an environment of corrupt practices and further undermining respect for government and tax administration.

Part I of the new tax code, which became effective on 1 January 1999, sought a thorough modernization of tax administration. It provided for the introduction of a common taxpayer identification number and allowed, in certain cases, for the indirect assessment of tax liability. More authority was also given to the State Tax Service, in particular, in allocating income, deductions and credits across related taxpayers, and in enforcing debt repayments by liquidated companies. Importantly, Part I also eliminated a ceiling on interest charged on overdue taxes. Some of its provisions, however,

worked in the opposite direction: for example, tax obligations were deemed discharged once the taxpayer had provided a payment order to a bank, which allowed taxpayers to claim fulfilment of their obligations without actually paying any tax. On balance, the general thrust of the administrative reforms at this time was to strengthen the effectiveness and powers[14] of the tax administration – at least potentially. In the oil sector, there are signs of strengthened enforcement, including a meeting between President Putin and 21 leading oil oligarchs to discuss the passage of new laws designed to curtail the use of tax avoidance schemes (Desai *et al.* 2004). But how and when the reforms in the legal framework and political environment changed practice through the wider tax system is very hard to judge; we have not been able to find any direct evidence on this, and opinions on how much actually changed at this time do vary.

There was thus much more going on at the start of 2001 than simply the change in the rate structure of the PIT. One key implication is that it is difficult to isolate effects of the PIT reform alone. The reductions in social insurance taxes, in particular, would be expected to trigger quite similar behavioural responses, making it especially difficult to disentangle the two.

3. PREDICTIONS OF THEORY

To provide a stylized framework for coming to grips with the anatomy of the 2001 reform, write revenue from the personal income tax, R, as $\tau.\lambda.w.L$, where τ denotes the (tax-exclusive) tax rate, λ the ratio of declared taxable income to true taxable income (so describing the degree of compliance, with $\lambda = 1$ corresponding to fully truthful reporting), w the gross wage rate and L the level of employment (here abstracting, for simplicity, from capital income components of the PIT base). Denoting proportionate changes by hats, the revenue effect of any reform is then approximated by

$$\hat{R} \approx \hat{\tau} + \hat{\lambda} + \hat{w} + \hat{L}. \tag{1}$$

Though some elements of the 2001 PIT reform tended to increase revenue at unchanged behaviour, these – though certainly important for some individuals – were relatively minor (the most important probably being the elimination of the exemption for military servicemen). Thus the reform corresponds, for those initially paying PIT at a higher rate, to a substantial reduction in τ. The question is whether the three types of response to the reform remaining on the right of (1) could have led to such an increase in the tax base as to account, to any substantial degree, for the strong performance of PIT revenue subsequent to the reform. The rest of this section considers each in

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[14] In particular, the tax police were authorized to conduct tax audits if sufficient evidence of a suspected tax crime was available, and to investigate non-tax commercial crimes such as money laundering.

turn, and the possibility that the reform may have led to some income-shifting between the CIT and PIT.

3.1. Gross wage rates

In the formal sector, one would expect the gross wage w to fall as a consequence of the reduced tax wedge (both PIT and social taxes).[15] Translated into the terms of the empirical exercise below, the implication is that gross wage rates of groups most affected by the reform should have fallen relative to those of groups less affected. In the informal sector, the gross wage might conceivably have risen (in order to leave take-home wages in line with those available in the formal sector); but this would have had no direct impact on tax revenue.

3.2. Work effort

Effects on labour supply might be expected from both the change in gross wage rates and the change in the parameters of the PIT and social taxes. The former depends routinely on the elasticity of labour supply, so the latter is the focus here.

To simplify, imagine a reform that leaves the exempt amount and starting marginal rate of tax unchanged but lowers, to the same level, the (single) top rate. (In fact, as seen above, the starting marginal rate – inclusive of social insurance – fell by 1.3 percentage points and the pattern of effects at the higher rate was more diverse). As shown in Figure 2,[16] by reducing the higher marginal rate to the level of the standard rate the reform has the effect of rotating the budget constraint relating before- and after-tax income anti-clockwise around the kink point (at the level of income at which that higher rate initially applied) until the budget constraint becomes a straight line.

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[15] Take, for instance, the natural benchmark case of a competitive labour market, characterized by equality between the demand for labour, $D(w)$, and the supply of labour, $S[w(1 - \lambda\tau)]$ (taken to depend on the wage net of taxes actually paid, so ignoring for simplicity the risks associated with non-compliance). Denoting the elasticities of labour demand and supply by e^D and e^S (both defined to be positive numbers) it is then routine to show that

$$\hat{w} = \left(\frac{e^S}{e^S + e^D}\right)\left(\frac{\lambda\tau}{1 - \lambda\tau}\right)(\hat{\tau} + \hat{\lambda})$$

so that the gross wage falls unless compliance increases by a greater proportion than the tax rate falls. Using this relationship (and now ignoring, counter-factually but for clarity, the social taxes that would also be expected to affect net wage and hence labour supply), it is straightforward to show that in this simple framework the overall effect on PIT revenue is

$$\hat{R} = \left[\left(\frac{e^S(1 - e^D)}{e^S + e^D}\right)\left(\frac{\lambda\tau}{1 - \lambda\tau}\right) + 1\right](\hat{\tau} + \hat{\lambda}).$$

A necessary condition for revenue to increase (given $\hat{\tau} + \hat{\lambda} < 0$) is thus that the elasticity of labour demand exceed unity. Given this, the increase is larger the greater is the elasticity of the supply of labour, the higher is the tax rate and the higher is the initial level of compliance.

[16] After-tax income, on the vertical axis, corresponds to consumption; and pre-tax income, on the horizontal axis, is proportional (assuming that the gross wage rate is independent of hours worked) to labour supply. So the figure just shows the consumer's choice between consumption and leisure.
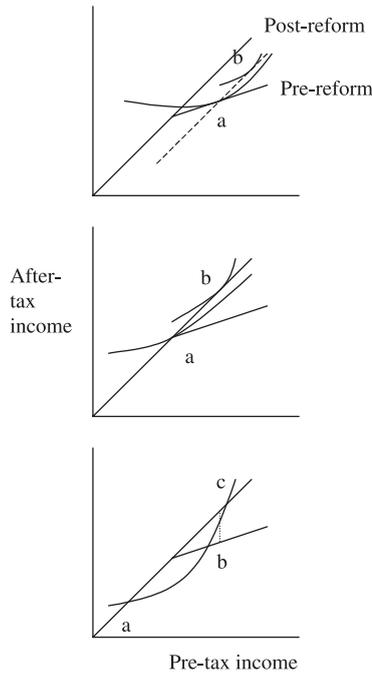
Figure 2. Labour supply before and after reform

The upper panel of Figure 2 illustrates the impact of this on a taxpayer who pays at higher than the standard rate prior to the reform. The substitution effect of the reform – isolated by comparing the initial choice at *a* to that which would be made under the hypothetical dashed budget constraint passing through *a* but parallel to the new budget constraint – is to increase pre-tax income, to a point like *b* (and hence also to increase the tax base), reflecting the reduction in the marginal tax rate. Acting in the opposite direction is an income effect – represented by the comparison between *b* and the choice that would be made under the post-reform budget constraint – that arises not only from the increase in the marginal wage but also from the increase in net income consequent upon the reduced taxation of intra-marginal income initially taxed at the higher rate. Under the standard assumption that leisure is normal, this tends to reduce work effort, and, hence, the tax base. For such an individual, the labour supply effect of the reform is thus ambiguous – a familiar conclusion. For an individual who, prior to the reform, locates interior to the segment of the budget constraint corresponding to the standard rate it is clear – and so not illustrated – that the reform simply has no effect on work effort or, hence, the tax base.

There is, however, another important possibility. The individual shown in the middle panel of the figure locates, prior to reform, exactly at the kink point at which the higher rate of tax begins. In this case the reform has only a substitution effect,

and work effort increases from that at *a* to that at a point like *b*. This may seem an extreme case – though one might in principle expect some 'bunching' of taxpayers at kink points of this kind – but points to a possibility of some importance to our empirical work. Suppose that individuals do not choose, as has been implicit in the figures so far, between a continuum of possibilities along the budget constraint but rather must choose between distinct alternatives located discretely along the budget line. Consider, for example, the individual shown in the third panel, who can choose only between gross income levels at *a* and at *b*. Prior to the reform, *a* is preferred: the individual pays tax at the standard rate. After the reform, however, the contract offering the higher level of gross income – the net income from which has now increased to *c* – becomes the more attractive of the two. In such a case the reform elicits a positive supply response even from an individual who, prior to the reform, paid tax at the lower rate. Similar effects may obviously arise if individuals simply make errors in their optimization. Recognizing this possibility – that the reform might increase the work effort of those not directly affected by it – will be important in the empirical work below.

There is another case in which the reform might increase work effort: workers who face some fixed cost in working might shift, as a consequence of the reform, from inactivity to earning a level of pre-tax income higher than that at which the higher rate previously began. It could not be optimal to enter work at a lower income level, since that option was available but rejected prior to the reform. But this seems very unlikely to have been important in practice, given the high income level at which the higher rates began.

3.3. Compliance

The analysis above assumes that individuals are perfectly truthful in their tax affairs. The second main route by which the reform might affect the tax base, however, is through an impact on compliance.[17] And indeed this is the route that tends to be stressed in positive assessments of the reform.

It seems to be widely believed – indeed taken as obvious – that a reduction in the rate at which a tax is levied will tend to improve compliance with it. The theoretical literature, however, paints a more subtle picture. In Slemrod's (2001), model of tax avoidance, for example, reducing the tax rate does indeed lead to reduction in the proportion of income that taxpayers shield, at some cost, from taxation.[18] On the other hand, in Allingham and Sandmo's (1972) model of tax evasion as a gamble, if

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[17] Labour supply and compliance decisions are in principle inter-related. But the analysis of that joint decision proves cumbersome, and for present purposes adds little to the insights gained by considering each in isolation (as discussed, for instance, by Slemrod and Yitzhaki 2002).

[18] There are other models that give the same conclusion. Engel and Hines (1999), for instance, show that increased tax rates may also lead to more evasion when individuals are aware that past declarations will be re-opened if they are selected for audit.

the fine in the event of being caught increases with the amount of tax evaded then, as shown by Yitzhaki (1974), a cut in the tax rate actually leads to an increase in the extent of evasion. The same is also true in simple models of bargaining between taxpayer and corrupt inspector, so long as the penalties depend on the tax evaded.

A key factor shaping the relationship between tax rates and compliance, and helping to reconcile these diverse results, is whether the costs of attempting to reduce tax payments depends on the extent of the tax reduction itself (as in the Yitzhaki version of Allingham–Sandmo) or on the extent of the income that must be concealed to bring it about (as in Slemrod).[19] The reason for this is straightforward. A taxpayer will presumably attempt to reduce tax liability up to the point at which the marginal benefit of doing so equals the marginal cost. Since the marginal benefit from a dollar of taxes saved is independent of the tax rate, so too, in equilibrium, must be the marginal cost. If that cost itself depends on the amount of tax concealed, then the amount of tax concealed must be constant. But the only way to keep the amount of tax concealed unchanged when the tax rate goes down is to conceal more income: that is, a reduction in the tax rate must worsen compliance, as in Yitzhaki–Allingham–Sandmo. If, on the other hand, the cost of concealment depends on the amount concealed, then since a reduction in the tax rate reduces the benefit of concealing a given amount of tax liability, it will lead to improved compliance, as in Slemrod.

It is natural, then, to wonder which form of cost structure better characterizes the very poor compliance in Russia in the late 1990s. Fines for tax evasion did in principle increase more than proportionately with the extent of the attempted evasion.[20] But the costs of evasion are more than simply the prospective penalty. Keeping out of the tax system, in particular, may require keeping out of the formal sector more generally, with attendant costs (perhaps such as restricted access to credit, including the ability to take a mortgage) that depend on the size of the business operations concealed. Thus theory gives no very firm guidance as to the likely sign of the relationship between the tax rate and the degree of compliance.

Nor has econometric work led to any clear-cut conclusion as to the sign of the effect in practice: the review by Andreoni *et al.* (1998) found that empirical conclusions have been mixed. The same is also true of more recent work: Schneider and Enste (2000) conclude that high tax rates encourage the concealment of activity; Friedman *et al.* (2000) find the opposite.

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[19] This is indeed the key point made by Yitzhaki as an observation on the Allingham–Sandmo model. The point seems, however, to be of even wider applicability. All this does not mean, however, that the nature of concealment costs is the only determinant of the sign of the relationship between the tax rate and compliance. One of the key lessons from Allingham and Sandmo (1974), for example, is that since the change in the tax rate affects the level of income at unchanged evasion its impact will be shaped in part by attitudes to risk.

[20] For instance, failure to pay taxes due as a result of understatement of the tax base or incorrect assessment was subject to a fine of 20% of the unpaid tax if the omission was unintentional, and 40% if intentional. Moreover, the effective penalty rate will be increasing with the amount evaded to the extent that the interest rate charged on overdue payments exceeds the taxpayer's cost of capital.

3.4. Income shifting

Apart for the incentive and compliance routes, there are two other ways in which the reform might have affected the PIT base.

The first is by inducing a reclassification of income as personal rather than corporate, either by an explicit change in organizational form or implicitly by firms paying out earnings to those with an ownership interest (or related parties) as salary or in other forms, such as pensions or interest, that generate deductions against the business tax but are taxable as personal income.[21] With both the maximum corporate tax rate and the tax on dividends increased at the start of 2001, at the same time as the higher rates of PIT and social taxes were cut, it might seem that receiving payments as personal income rather than in the form of retained earnings did indeed become more attractive. Two considerations seem likely to have mitigated this, however. The first is the adoption of imputation in 2001, which reduced the effective tax rate on distributed corporate earnings from 40.5% (= $1 - (1 - 0.15)(1 - 0.3)$) to 35% (the imputation credit being non-refundable). Second, whereas the most marked reduction in the PIT and social insurance rates only applied to income in excess of the pre-reform thresholds for the higher rate, this reduction in the rate on distributed corporate earnings applied essentially to all profits. Thus the tax advantage of personal income may not have increased by as much as it at first seems. Whether the reform is likely to have led to significant recharacterization is thus *a priori* unclear.

Second, since the reform was preannounced in June 2000, higher rate taxpayers will have had an incentive to shift taxable income into 2001, so generating an artificial increase in the taxable incomes of those who were in higher bands pre-reform. Thus if income shifting did take place it would reveal itself in the analysis below in the form of increased incomes of those initially paying tax at a higher rate.

4. THE PIT REFORM IN A WIDER CONTEXT

Before turning in the next section to evidence on behavioural responses to the reform at the individual and household levels, it is instructive to consider what the available macroeconomic data suggest to have been its impact. For this we look first at the revenue performance of the wider tax system over the same period, and then examine official data on movements in the aggregates underlying PIT revenue.

4.1. Revenue performance

To provide a broad context within which to evaluate the reform, Table 4 shows the level and composition of general government revenues – consolidated, that is,

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[21] Gordon and Mackie-Mason (1994) and Gordon and Slemrod (2000) find this to have been of some importance in the United States.

Table 4. General government revenues (in percent of GDP), 1994–2003

	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
Total Revenue	34.6	36.8	35.8	39.3	34.4	33.6	36.9	37.4	37.6	36.6
of which										
Personal income tax	2.9	2.6	2.8	3.2	2.7	2.4	2.4	2.9	3.3	3.4
Profit tax	8.0	8.2	4.9	4.4	3.7	4.5	5.5	5.8	4.3	4.0
VAT	7.0	6.9	7.6	7.3	6.4	5.9	6.3	7.2	6.9	6.6
Excises	1.2	1.7	2.8	2.7	2.7	2.2	2.3	2.7	2.4	2.6
Taxes on trade	1.0	1.7	1.1	1.2	1.3	1.8	3.1	3.7	3.0	3.4
Payroll taxes[a]	8.9	8.1	8.2	9.7	8.4	7.7	7.7	7.3	8.0	7.8
Resource taxes	0.0	0.9	1.1	1.5	0.9	0.9	1.1	1.4	3.1	3.0
Other tax revenue	5.1	3.4	3.6	4.9	3.6	2.9	2.8	2.4	2.8	2.3
Non-tax revenue	0.0	1.5	1.2	1.2	1.5	1.7	1.8	2.3	2.5	2.6
Budgetary funds[b]	0.5	1.9	2.4	3.2	3.1	3.4	4.0	1.8	1.3	0.9

[a] Payroll taxes include annual accumulation of a fully-funded state pension system.
[b] Budgetary funds inclusive of on-budget and off-budget regional road funds.

Sources: Ministry of Finance, CBR, Goskomstat, and IMF staff estimates.

across all levels of government – for the years up to and immediately after the 2001 reform.

Revenue from the PIT increased following reform at the start of 2001 by about 20% relative to GDP;[22] in nominal terms, it increased by about 46%, and in real terms by around one-quarter. But what is also striking from the table is that – with three exceptions, to which we shall return – revenue from all sources, not just the PIT, increased substantially in 2001, relative to GDP. The PIT showed the greatest increase, but the indirect and trade taxes performed almost as well. The breadth of this increase in revenues, which was itself to a large degree a recovery towards levels prior to the 1998 crisis, points to some common underlying cause.

The most obvious candidate is the increase in energy prices from late 2000. Natural gas prices reached a peak in 2001, declined in 2002 and then increased in 2003; oil prices increased and peaked somewhat earlier. Oil-related revenues (largely in the form of excises and export taxes) accounted at this time for about one-third of federal government revenues, so that – given the potential impact on the wider macroeconomy – one might expect substantial revenue gains as a result. Indeed Kwon (2003) attributes about 80% of the recovery of revenues after the crisis to the strength of the oil and gas sector (which accounted for about 20% of GDP). As one would expect, revenues from resource taxes, and excises and taxes on trade, track energy prices quite closely. The likely impact on profit tax receipts is less clear-cut, with gains from the sector itself and reduced profitability of oil/gas users acting in the opposite direction.

The link between energy prices and PIT revenue, however, is much less direct. There can have been only very limited positive effects through levels of employment,

[22] Officially reported GDP in Russia, used throughout this paper, includes an estimate of unreported activity (which is in the order of 25% of reported GDP).

which increased by just 1.3% over 2001,[23] mostly in small businesses (which account for about one-third of all employment). Moreover, it is striking that the increase in PIT revenues continued into 2003, when other sources declined – suggesting that this was not simply a consequence of strong energy prices. All this makes it hard to attribute the strong performance of the PIT to the strength of energy prices alone.

As noted, revenues from three sources actually fell, relative to GDP, between 2000 and 2001. The decline in budgetary fund revenue is attributable mostly to the reduction in the turnover taxes. There is no obvious single explanation for that in 'other tax revenue', which includes small business taxes, property taxes, and many other small items. Most interesting for present purposes is that payroll taxes, which are levied on a similar base to that of the PIT, fell by about 5% relative to GDP – increasing in nominal terms by only about 16% – at the same time as PIT revenues rose so strikingly. This seems to reflect the marked reduction in the combined rate of the social tax, which unlike the PIT reductions, reduced tax rates throughout the entire range of incomes. Still, revenues from this source fell by less than would have been expected had real incomes remained static.[24] Just like the boom in PIT revenues, this suggests – if more weakly – that the base for these taxes has expanded.

One other feature that stands out in Table 4 is the relatively poor revenue performance of profit tax revenue in 2001 and the decline in revenues thereafter, when that from PIT continued to increase. This is difficult to interpret, given the diverse range of potential influences at the time: the increase in energy prices will tend to have increased revenues from energy producing firms while reducing those from energy users; the increase in the maximum rate of profit tax, from 30 to 35%, will have tended to increase revenues; the extension of other deductions will have had the opposite effect; some enterprises may also have brought forward investment in anticipation of the pre-announced reduction in tax allowances from 2002. All this precludes any clear-cut conclusion from these data on the possibility, discussed in the previous section, that the tax reform may have led to income-shifting from corporate to personal incomes. Nevertheless, the continued and marked growth of PIT revenues in 2002 and 2003, despite significant cuts in the tax rates on both profits and dividends (while the PIT structure remained unchanged) suggests that any such income-shifting was of limited importance.

4.2. Wage developments

In Russia, as elsewhere, the bulk of revenue from the PIT comes from wages and salaries, so it is here that one must look first to understand the anatomy of PIT revenue developments. The first five rows in Table 5 report official estimates of

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[23] Similarly, year average employment increased by only 0.3%.

[24] This is so even taking into account that 7% of UST payments in 2001 were for arrears.

Table 5. Analysing official income and tax data

	1999	2000	2001	2002
	Billions of rubles			
Gross wage income[a]	1934	2937	3819	4995
of which				
Reported	1408	2126	2826	3749
Hidden	526	811	993	1246
PIT revenue	117	175	256	358
UST revenue	373	561	652	865
Reported wage income base[b]	1035	1565	2175	2883
Net wage income	918	1390	1919	2525
Average effective PIT rate	11.3	11.2	11.8	12.4
Average effective UST rate	36.0	35.8	30.0	30.0
Average effective tax rate[c]	34.8	34.6	32.1	32.6
Compliance[d]	72.8	72.4	74.0	75.1
	Percentage change			
Gross wage income[a]	52.9	51.8	30.0	30.8
Reported	41.7	50.9	32.9	32.6
Hidden	94.1	54.2	22.4	25.5
PIT revenue	64.5	49.3	46.3	40.1
UST revenue	67.9	50.4	16.2	32.8
Reported wage income base[b]	34.2	51.1	39.0	32.6
Net wage income	31.1	51.4	38.0	31.6
Average effective PIT rate	22.6	−1.2	5.3	5.7
Average effective UST rate	25.1	−0.5	−16.4	0.2
Average effective tax rate[c]	17.9	−0.5	−7.2	1.7
Compliance[d]	−7.3	−0.6	2.2	1.4

[a] Inclusive of taxes paid by employer and employee.
[b] Calculated ignoring the collection of tax arrears, which comprised 7% of UST revenue in 2001.
[c] Inclusive of social taxes.
[d] Calculated as the ratio of reported to total wages.

Sources: Goskomstat and authors' estimates.

reported and hidden wage incomes, and of PIT and social tax revenues. These estimates imply that the average effective PIT rate increased slightly from 11.2 to 11.8%. Thus, the one point increase in the PIT rate for lower income earners, together with the base expansion due to the elimination of exemptions,[25] slightly more than offsets the effect of the rate cut at the higher end. The average effective rate of the social taxes dropped markedly from 35.8 to 30%, reflecting the reduction in the statutory rate at all income levels. Overall, the average effective tax rate (inclusive of employer-paid taxes) decreased by only 2.5 percentage points, despite the dramatic reduction in marginal tax rates. Though the direct impact of the reforms was thus potentially very substantial for the very highly paid, the average rate cut was quite modest.

Official Russian statistics also include an estimate of hidden wage income, and so generate an estimate of the degree of compliance. The source and reliability of the

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[25] Sinelnikov-Mourylev *et al.* (2003) estimate the effect of removal of this exemption at 2% of total PIT growth, consistent with the evidence presented in this section.

Figure 3. Dynamics of average labour productivity, output and wages

estimate of hidden wage income is unclear,[26] but taken at face value the data imply that 72.4% of total wages were officially reported to the tax authorities in 2000, rising to 74%: an improvement of a little over 2%.

The implications of the official data for the structure of the increase in PIT revenues can be seen by writing these as $R = \tau_{PIT}.(1 + \tau_{UST})^{-1}.\lambda.I$, where the subscripts distinguish the rates of the PIT and UST and $I \equiv w.L$ denotes gross income (inclusive of PIT and UST). Approximating as in (1) above,[27] the official data imply that about 13% of the increase in PIT revenue reflects the increase in the effective rate of the PIT itself, about 10% is due to the lower rate of social taxation (through the effect of increasing the PIT base for any level of gross income), about 5% reflects improved compliance and the bulk – over 70% – is associated with an increase in gross incomes. The modest increase in UST revenues reflects the dominance of this increase in gross incomes over the large cut in the average effective rate of the tax.

Wage developments thus appear to be a large part of any explanation of the performance of PIT (and of social tax) revenues. The growth in real wage income over this period was indeed spectacular, as can be seen from Figure 3.[28] After-tax real wage income grew by 18.5% in 2001, while gross real wages grew more slowly (at only 11.6%), reflecting the reduction in tax rates. Still, both gross and net wages outpaced GDP growth, at 5.1%, and average labour productivity, which grew by 2.3% in 2001 – implying an increase in the labour share in this year.[29]

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[26] Goskomstat (the Russian federal statistics agency) does not provide details on the calculation of hidden wages. The data description only mentions that in addition to information on wages collected from monthly surveys of large and medium-size organizations and quarterly surveys of small organizations, a special methodology is applied to estimate total wages.

[27] More precisely, PIT revenue growth is approximately $\hat{\tau}_{PIT} - (\tau_{UST}/(1 + \tau_{UST})\hat{\tau}_{UST} + \hat{\lambda} + \hat{I}$.

[28] The minimum wage was increased in 2001 by 127% (in nominal terms). Starting from 2000, however, changes in the public sector wages were decoupled from increases in the minimum wage, so that the direct impact of this increase on wage income is likely to have been very limited.

[29] The labour share is calculated as the gross real wage relative to average labour productivity.
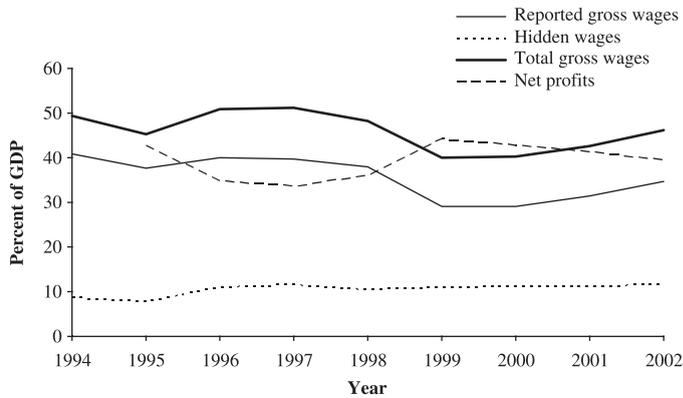
Figure 4. Share of wages and net profits in GDP

The income shares of labour and net profit from the mid-1990s are plotted in Figure 4. What is clear is that while there was a significant increase in labour share around the time of the tax reform, this was in effect a recovery towards its level prior to the 1998 crisis. The figure also demonstrates that fluctuations in the labour share have been driven mostly by the changes in reported wage income, with hidden wage income remaining constant at about 10% of GDP. The pattern suggests that labour took a stronger hit during the 1998 crisis than did other factors, and benefited more from economic recovery afterwards. While such procyclical behaviour of the labour share is unusual compared to other countries, an increase in the labour share in 2001 fits a pattern previously observed for Russia, with real wages tending to overshoot real GDP.[30] With relatively small changes in employment over the period, as can be seen from Figure 3 (average labour productivity closely follows real GDP growth), wage adjustments seem to be more common in Russia than employment adjustments. Explaining this, however, lies beyond the scope of this paper.

The picture that emerges from the macro data is thus a fairly straightforward one, with the strength of PIT revenues due overwhelmingly to a marked increase in gross incomes between 2000 and 2001, and any gain in compliance being very modest. But aggregate data of the kind just reviewed can be no more than suggestive as to the likely impact of the reform, since – even leaving aside data deficiencies, including in the measurement of hidden wages – it can cast no direct light on the underlying behavioural responses to the reform. For sharper insights into these key issues one looks to individual- or household-level data, and it is to this that we now turn.

5. MICRO EVIDENCE: DATA, METHODOLOGY AND HYPOTHESES

This section describes the RLMS panel data and methodology that we use.

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[30] See, for example, Konings and Lehmann (2002).

5.1. Data

The dataset best suited to analysing micro-level responses to the tax reform is the Russian Longitudinal Monitoring Survey (RLMS) of the Carolina Population Center at the University of North Carolina, which is described in the data appendix. It provides information on the incomes and other attributes of around 3,500 adults for every year (except 1997 and 1999) between 1994 and 2002, though here we use only data for 2000 and 2001.

The dataset does not contain all the variables one would ideally like. Most importantly, there are no data on tax payments or on pre-tax incomes, so that these have to be inferred from reported after-tax incomes. This requires some assumption – clearly critical given the importance of compliance effects in evaluating the reform – as to whether an individual did indeed pay taxes and whether only reported or also undeclared income is being reported in the survey. Moreover, the survey does not provide enough information to calculate all tax deductions. Another serious problem, common to all voluntary surveys touching on financial issues, is that both the best- and the worst-off individuals are under-represented. The former are commonly especially reluctant to disclose their incomes (perhaps for fear of investigation), or may simply value their time too highly to comply with the survey; the latter may not be included because they have no home (the RLMS being an address-based survey).

There are several income variables in the RLMS. That on which we focus is the response to the question: 'What was your average monthly wage after taxes over the last 12 months from the primary employer regardless of whether it was paid on time or not?' The answer to this may for some respondents include information from the pre-reform period, but this is unlikely to greatly bias the results: all interviews are undertaken in the last quarter of the calendar (and fiscal) year, so that pre-reform months will be a small part of the total. The survey also asks: 'How much money in the last 30 days did you receive from your primary job after taxes?' But this is available less frequently and is less well-suited for the calculation of taxes paid (see the data appendix). In any event, the results are essentially the same for both income variables. There are also questions on income from secondary and additional employment. These are not included in the results shown below, as it is less clear whether they are taxed: again, however, the results that follow are broadly robust to this choice.

Before using the data we do some limited cleaning. Individuals between 20 and 60 years old throughout 2000 and 2001 are kept; those who do not report how many hours they work, report working more than 84 hours a week, do not report any income from their primary employment, and/or who own their own business are all dropped. While this last group would be of particular interest, as such individuals are likely to have more possibilities to evade and avoid taxes, there are simply too few of them in the sample (17 in the year 2000) to make analysis worthwhile. All this leaves

Table 6. Comparisons of RLMS sample and official data

	Year	Estimated	Published data
Average monthly wage/salary	1998	1092	1051
	2000	2174	2223
	2001	3310	3282
	2002	4332	4426
Nominal increase in PIT revenue	2000/2001	45.2%	46.3%

Notes: The average wage quoted is gross of income tax, but net of employer's social taxes. Official wage data are from Goskomstat (website), tax data are from the Ministry of Taxation of the Russian Federation. Estimated data are based on RLMS sample, cleaned as described in the text; personal tax payments are calculated from reported average income over the last 12 months (*pjpayt*).

3,722 individuals. This is further reduced in the regressions, as we then only keep individuals who are present in both years and for whom the left hand-side variable is available.

Despite these various weaknesses, the key features of the RLMS sample match the corresponding official aggregates extremely closely, as shown in Table 6. Average salaries are very close to the corresponding population averages. Still more strikingly, at 45.2% the growth in PIT payments in the sample over the year following the PIT reform – which we have calculated by applying the tax schedule to reported after-tax incomes (as described more fully below) – almost exactly matches the growth in the population. Note that the official figures reported in Table 6 do not include any estimates of income from the informal economy. The close match between the estimates from the sample and their population counterparts suggests that in answering the RLMS income questions respondents tend to conceal their receipts from informal activities and report only the net earnings that have been properly taxed. This is certainly weak evidence for such an interpretation, but there is little else to build on. In any event, this is an interpretation that we shall make heavy use of below.

More details on the data used here, and on the calculation of variables, are given in the data appendix.

5.2. Methodology

The approach taken in using these panel data is to compare the experiences of individuals affected by the reform with the experiences of those who are not (or, at least, are much less) affected. This 'difference in differences' methodology has been used by Feldstein (1995) and Eissa (1995) to study the US 1986 tax reform and, combined with a structural approach, by Blundell *et al.* (1998) to study the effects of UK tax reforms. It is especially appropriate in the context of the Russian reform, because the structure of that reform is such that there are some taxpayers who are strongly affected by the reform, and so form a natural 'treatment' group (these are those taxpayers who, prior to the reform, were liable to PIT at a rate higher than

the minimum) and some other taxpayers who are largely unaffected and so form a natural 'control' group (those in the lowest tax bracket, who, as seen above, faced a one point increase in the marginal PIT rate and a 1.3 point reduction in the marginal rate of PIT and social insurance combined).

As social insurance taxes were changed at the same time as the PIT, they too need to be taken into account when analysing the PIT reform. Social taxes in Russia are formally incident on employers (except for the 1% pension fund levy), but of course this does not imply anything about their economic incidence: at least in the long run, the effective incidence of a tax is expected to be independent of its legal incidence. Moreover, both PIT and social taxes are generally levied by withholding, with the employer legally responsible for its proper payment. In the short run, it might be that labour supply decisions depend more on taxes levied on the employee, if contracts are specified in terms of nominal wages paid after deduction of social tax but prior to PIT. A case could thus be made for looking only at taxes levied on the employee. This case is weak, however, as there is no strong reason to believe that contracts in Russia are particularly sticky and because data were in any event collected in the last quarter of the year, allowing significant time for adjustments in response to the reform. Furthermore, to the extent that tax evasion decisions are taken jointly by employer and employee, they will be affected in the same way by each tax. Therefore, while we report both results focusing on revenues from the PIT and from the PIT and social insurance combined, we do not attempt to identify distinct behavioural effects from the synchronous PIT and social insurance reforms.

The effects of the reform on the pattern of marginal tax rates (PIT and social taxes combined) were shown in Figure 1 in Section 2 above. From this, it might seem simple to construct groups of individuals who are hardly affected, somewhat affected, and greatly affected by the reform. The actual distribution of incomes in the sample, however – also shown in the figure – is such that few people in the sample saw their marginal tax rates fall very noticeably. For most individuals, the higher tax rate brackets (before the reform) and lower rate social tax brackets (after the reform) are irrelevant. Most individuals are thus virtually unaffected, while a few are slightly affected. While about 10% of the sample paid PIT at a higher rate prior to the reform,[31] there is only one individual who after reform benefited from the lowest social insurance rate of 5% and so enjoyed the maximum possible benefit from the reform. Given this distribution of tax cuts, an obvious definition of the treatment group for empirical purposes would be those individuals initially paying a higher tax rate. The issue of whether or not social taxes are included in the analysis therefore does not affect the definition of treatment and control group. The only difference is that,

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[31] Although there appear to be no publicly available data on the numbers (or incomes) of taxpayers in the various rate bands prior to reform, it does seem to be widely believed that the vast majority of those who paid tax prior to reform did so at the lowest rate.

including social taxes, there is now a tax cut even in the control group. But since it is much smaller than for the treatment group (1.3 percentage points compared to between 7.1 and 33), one would still expect a differential response to the reform.

Apart from the tax rates, the small increase in the personal allowance also affects control and treatment groups differently. This is because the increase will be worth proportionally more to poorer tax payers. Furthermore, the personal allowance is withdrawn at a faster pace after the reform. The increased allowance is therefore likely to be more important for the control group.[32] But any effect is likely to be small, as the personal allowance is very low: while it could be up to two minimum wages before the reform, the minimum wage is extremely low, serving as a unit of calculation rather than an actual minimum required to cover the basic needs.

Once treatment and control group are defined, the methodology can be used to study not only PIT payments but also the various components shown in Equation (1). It can indicate whether a reaction occurred, and how large it was compared to other groups. The method has the drawback, however, of presuming that both groups would have had the same relative changes in incomes had there been no reform. This might be problematic, given that the high- and low-income individuals being compared may have different income dynamics. This difficulty is common in using this methodology. The typical alternative assumption is of constant trend growth over time in the absence of a reform. But this seems even less attractive, since there are many reasons why trend growth rates can change, not all of which could be controlled for. We have in any event conducted robustness exercises (building on the work of Chay *et al.* (forthcoming)) to allow for the possibility of mean reversion; these are reported in the Web Appendix and leave our conclusions broadly unaffected. A further potential difficulty is that the approach may misstate the effects of the reform if there are important general equilibrium effects at work. It is for instance possible that a positive supply response in the treatment group also benefits the control group, say by bidding up wages, which would diminish any differential effect.

Formally, the analysis involves regressions of the form:

$$y_{it} = \beta_0 + \beta_1 T_i + \beta_2 P_t + \beta_3 (T_i \times P_t) + u_{it} \tag{2}$$

where y_{it} is the endogenous variable of interest (such as PIT paid) for the ith individual/ household at time t, T_i a dummy taking the value unity for the treatment group, P_t a dummy indicating the post-reform period and u_{it} a random disturbance (which may be heteroscedastic). The coefficient β_0 is a constant, β_1 indicates by how much the endogenous variable is higher for the treatment group, β_2 by how much the endogenous variable is increased during the reform and β_3 is the difference in difference

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[32] Because the allowance is withdrawn as income increases, the value of the allowances (allowance times tax rate) is a decreasing function of gross income, although not a monotonic one. Earners who pass the next income tax threshold see the value of the allowance going slightly up, because of the higher tax rate and then fall again as it is further withdrawn.

estimator, indicating by how much *more* the endogenous variable increased for the treatment group.[33]

We also consider regressions in growth rates of the form:

$$\hat{y}_i = \gamma_0 + \gamma_1 T_i + u_i. \tag{3}$$

These are estimated both using ordinary least squares, allowing for heteroscedasticity, and by a median regression (also known as least absolute value model). The latter has the advantage that the median is less affected by outliers, which are especially likely to arise when using growth rates (for instance if the level in the first year is close to zero).

5.3. Hypotheses of interest

The primary question of interest is whether the 2001 reform caused the subsequent increase in PIT revenue. As discussed above, the reform is likely to have had effects on gross wage rates, labour supply and compliance. But in order to conclude that the revenue boom was caused by the flat-rate reform, it must be the case that PIT payments of the treatment group have grown faster than those of the control group. A convenient way to structure the discussion is thus in terms of the null hypothesis that:

$$H_{L0}^R : \Delta R_T \geq \Delta R_C \tag{4}$$

where R is tax paid, subscripts T and C indicate treatment and control group; and subscript L indicates that comparison is in levels. If H_{L0}^R is rejected,[34] then we conclude that the rate-reducing aspect of the reform was *not* the direct cause of the revenue boom; if, on the other hand, H_{L0}^R is not rejected, then we cannot reject the possibility that it was the direct cause.[35] We also consider the analogous null hypothesis on the relative growth rates of PIT payments in treatment and control groups:

$$H_{G0}^R : \hat{R}_T \geq \hat{R}_C \tag{5}$$

where the subscript G indicates the specification in growth rates. Both specifications – in levels and growth rates – are of interest. That in growth rates may make the

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[33] The coefficient β_3 can also be estimated by the simpler regression $\Delta y_{it} = \beta_0 + \beta_3 T_i + e_i$ if one is not interested in the other coefficients.

[34] The stars indicating significance in our tables are based on the 'standard' null hypothesis that increases in the two groups are the same. Rejection of this implies *a fortiori* rejection of the null of a higher increase in the treatment group when the estimated difference-in-difference coefficient is negative. There is no case among our results in which we do not reject the null of equality, but could have rejected the hypothesis above in a one-sided test.

[35] It could still be, however, that the revenue gain reflected general equilibrium effects operating also through the tax paid by the control group. Recall too that the reform reduced taxes slightly in the control group: if the null hypotheses are rejected, it could thus be that the overall revenue growth was the result of its impact on the control group. Nevertheless, rejection would still rule out that the revenue increase was directly due to the large reduction of the higher rates.

comparisons between the two groups more transparent, though as noted above, special care must be taken in this case to avoid results being contaminated by outliers.

Note that R in the nulls above could be either PIT revenue alone or PIT and social insurance revenue combined. The former is our principal concern. Nevertheless, since PIT and social insurance were changed together, and are likely to have similar incidence, it is also of interest to examine the effect on PIT and social insurance combined.

Rejection of the nulls on tax revenue would not mean that the reform did not have important effects. It might still have affected hours worked or compliance. Even if the nulls above are rejected there remains more to be analysed in understanding the anatomy of the impact of the reform.

The first question is whether declared income also increased faster in the treatment group:

$$H_{L0}^{I\lambda} : \Delta(I_T \lambda_T) \geq \Delta(I_C \lambda_C) \tag{6}$$

where $I \equiv w.L$ again denotes reported income. (Again, we also consider the hypothesis in growth rates $(H_{G0}^{I\lambda})$ – as we shall in all further hypotheses). If this is rejected, then the reform not only failed to boost taxable incomes sufficiently to offset the tax rate cut: it did not boost them at all.

Next, and whether or not $H_{x0}^{I\lambda}$ is rejected, it is of interest to test for effects on true gross pre-tax income and, especially, compliance:

$$H_{L0}^{I} : \Delta I_T \geq \Delta I_C$$

$$H_{L0}^{\lambda} : \Delta\lambda_T \geq \Delta\lambda_C$$

(and analogously in terms of growth rates). Whether the reform was associated with an increase in compliance, in particular, is of crucial importance to the assessment of the reform. The direction and extent of any supply-side effects is also key to the debate on the reform, so that we also separately analyse gross wage rates per hour and the number of hours worked, with corresponding null hypotheses H_{x0}^{w} and H_{x0}^{L}.

5.4. Dealing with the non-observability of tax payments

An immediate and fundamental difficulty in testing these hypotheses is that RLMS does not provide data on tax payments, compliance, or even declared gross incomes. It provides only reported net incomes, but we do not know what exactly these represent, and in particular to what extent they include untaxed incomes. The basic assumption in the empirical work reported below is that the net incomes declared by survey respondents, N, are those associated with the income that is actually declared for tax purposes. That is:

$$N = I\lambda - \tau(I\lambda) \tag{7}$$

where $\tau(Y)$ is the tax function. This seems a reasonable interpretation, since it is the answer that individuals would give if they referred to their last pay slip (literally or mentally) to answer the income question. It certainly seems plausible to suppose that reported net incomes will generally not include undeclared incomes, since individuals may not fully believe in the anonymity of the survey and so prefer not to disclose any income on which tax has been evaded. (And even if they did trust in the anonymity, they would have a strategic incentive not to reveal their illegal income so as not to allow this phenomenon to be detected and acted upon.) Moreover, such an interpretation is consistent with the close match between the estimates from the sample, calculated on the basis of (7), and their population counterparts that emerged in Table 6 above – for if the incomes reported in the sample did include unofficial incomes, then one would expect to find much higher incomes and estimated tax payments than in the official figures in the table, which do not include the hidden economy.

Denoting by $n(Y) \equiv Y - \tau(Y)$ the function giving net income as a function of gross, the assumption in (7) enables the gross reported income of a respondent to be calculated as

$$I\lambda = n^{-1}(\mathcal{N}) \tag{8}$$

and their tax payments as $\tau(n^{-1}(\mathcal{N}))$. These estimates enable tests of null hypotheses H_{x0}^{R} and $H_{x0}^{I\lambda}$.

To test the others we use the consumption data in the survey under the assumption that these measure true net income:

$$c = I - \tau(I\lambda) \tag{9}$$

The assumption here that savings are zero is clearly extreme, though it seems a reasonable approximation for many in the sample: only 61 households in the sample report any savings. What is really needed for our analysis below, in any event, is not that the savings be zero but rather that they not be affected by the reform – an assumption we shall later test, as best we can. Using (9), gross income and compliance can be estimated separately as:[36]

$$I = c + \tau(I\lambda) \tag{10}$$

$$\lambda = \frac{n^{-1}(\mathcal{N})}{c + \tau(I\lambda)}. \tag{11}$$

As total hours worked are reported directly by respondents, the hypotheses on labour supply are easily tested; and so, by dividing gross incomes by hours worked, are those regarding the wage rate.

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[36] With savings s, these formulae become $I = c + s + \tau(I\lambda)$ and $\lambda = n^{-1}(\mathcal{N}) \; / \; [c + s + \tau(I\lambda)]$ respectively.

Table 7. PIT payments at individual level, split by initial marginal tax rate

Marginal tax rate, 2000		0%	12%	20%	30%
Levels	No. of taxpayers	100	2130	173	11
	Pre-reform	0	373.1	2355.0	7621.1
	Change	80.6	165.6	−104.3	−3961.0
Growth rates (%)	No. of taxpayers	0	2130	173	11
	Mean	–	104.3	−0.4	−45.6
	Median	–	34.0	−6.3	−35.1

Source: Authors' calculations, figures in real 1995 rubles.

6. PANEL DATA RESULTS

The key step in testing the hypotheses above is to define the treatment and control groups. To deal with this clearly and systematically, we proceed first under the assumption that taxes are fully complied with (so that $\lambda = 1$), in which case individuals can be allocated between these groups simply on the basis of the income reported in the survey, and then turn to the more general case in which there may be some concealment from the tax authorities.

6.1. Results assuming full compliance

We use a variety of ways to split taxpayers into control and treatment groups.

The first is according to the marginal tax rate faced by each individual before the reform, which is the approach taken by Feldstein (1995) and others. Results for PIT payments are shown in Table 7.

The first row gives the number of taxpayers in each group. As mentioned above, the number of individuals paying tax at a higher rate pre-reform – and especially at the highest rate – is rather small. There are also relatively few individuals earning less than the personal allowance, since as noted above this is very low. The second row shows how much tax individuals paid on average in each group (in real 1995 rubles). The third row shows by how much tax payments changed between 2000 and 2001. Strikingly, tax payments have fallen for all groups except those initially paying low tax rates: those groups with the largest tax cuts have witnessed the largest falls in tax payments.[37]

The other rows in Table 7 repeat the analysis for growth rates of PIT payments. Since the mean growth rate can easily be affected by outliers, we also show the median growth rates for each group. (Note that no growth rate can be calculated for

[37] There are a number of possible differences in differences in levels that can be analysed at this point. Comparing each group with the one just below (those paying tax at 30% to those paying at 20%, and so on), all differences are statistically significant – and point to a greater increase in PIT payments in the lower marginal rate groups – except that between those paying 12% and those facing a zero marginal tax rate.

Table 8. PIT payments at individual level, split between lower- and higher-rate payers

Group		Control (0 to 12%)	Treatment (20 to 30%)	Difference-in-differences
Levels	No. of taxpayers	2230	184	
	Pre-reform	356.4	2669.9	
	Change	161.8	−334.9	−496.7*** (160.7)
Growth rates (%)	No. of taxpayers	2130	184	
	Mean	104.3	−3.2	−107.4*** (12.5)
	Median	34.0	−8.7	−41.9*** (4.2)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

those earning less than the allowance, as the base would be zero.) Again we find that the higher the initial tax rate, and hence the larger the tax rate reduction, the lower is the growth of tax payments.[38]

A second way of splitting the sample is between those who paid tax at no more than the minimum rate prior to reform and those who paid at a higher rate. The advantage of this split is that it avoids the very small group sizes obtained above. Results are in Table 8. Apart from the levels, changes and growth rates, this table also shows the difference in differences estimates obtained by running the regressions described in the previous section. These results clearly suggest that tax payments in the treatment group fell while those in the control group increased, in both levels and growth rates, implying a rejection of H_{x0}^{R}.

The third sample split we consider addresses the possibility, noted in Section 3 above, that if there is not a continuum of wage contracts then even some individuals paying tax at the lower rate might have been affected by the reform. To deal with this, we consider an extended treatment group consisting of all those individuals earning, before the reform, more than 75% of the threshold income level at which the higher rates began (which is likely to err on the side of including too many individuals in the treatment group). The results are shown in the first and fourth rows of Table 9, next to the results from the previous table. They lead to the same conclusion as above: that the reform did not cause the growth of PIT revenues.

In Table 9 we also show results on other variables of interest. Apart from PIT revenues, we consider combined PIT and social insurance revenues, gross incomes, hours worked and wage rates. In each case we present results for both the previous and the extended treatment group. For clarity, only the difference in differences estimators are shown, once more for both levels and growth rates.

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[38] The differences in differences in growth rates can also be analysed as in the previous footnote. With OLS regressions, all differences in growth rates are significant; with median regressions the difference between individuals in the 30% and 20% brackets becomes insignificant, although individuals in both groups have significantly lower PIT growth rates than those in the 12% bracket.

Table 9. Difference-in-differences estimators for different variables at individual-level

		Diff. in Diff., Treatment Group: 20–30%	Diff. in Diff., Extended Treatment Group
Levels	Change in PIT	−496.7*** (160.7)	−232.3** (104.2)
	Change in total tax	−1860.2*** (452)	−1110.0*** (303.9)
	Change in gross income	−158.8 (100.9)	−100.9 (67.6)
	Change in hours worked	−2.155** (1.174)	−2.135*** (0.811)
	Change in wage rate	−0.491 (0.564)	−0.280 (0.367)
Growth rates (%)	Mean growth rate, PIT	−107.4*** (125.3)	−102.0*** (13.2)
	Mean growth rate, total tax	−51.1*** (3.8)	−47.2*** (3.4)
	Mean growth rate, gross income	−41.9*** (3.9)	−41.2*** (3.3)
	Median growth rate, PIT	−41.9*** (7.0)	−32.9*** (3.7)
	Median growth rate, total tax	−28.3*** (4.7)	−24.2*** (2.4)
	Median growth rate, gross income	−26.0*** (5.7)	−25.5*** (2.3)
	Mean growth rate, hours worked	−5.6*** (1.5)	−5.9*** (1.2)
	Mean growth rate, wage rate	−37.7*** (5.5)	−38.1*** (4.4)
	Median growth rate, hours worked	0 (2.4)	0 (1.8)
	Median growth rate, wage rate	−22.4*** (3.8)	−23.4*** (3.2)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions these were obtained with a bootstrap procedure. Regressions on PIT, total tax and gross income based on 2,414 individuals, regressions on hours worked and wage rates based on 2,409 individuals (as not all individuals report hours worked). Total tax and PIT show annual figures, while the gross income is a monthly figure. Gross income is gross of PIT and social tax. The extended treatment group is defined to include all individuals earning at least 75% of the higher-rate threshold. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

The results show that it was not only PIT payments that fell in the treatment group relative to the control group, but also the sum of PIT and social insurance payments. Thus H_{x0}^{R} is also firmly rejected for the sum. So too is the null hypothesis that gross income grew more in the treatment groups than in the control group ($H_{x0}^{I\lambda}$), although only weakly so for the regression in levels.[39] Interestingly, this is in stark contrast to the results for the United States reported in Feldstein (1995), who found that following the 1986 US tax reform gross incomes increased more for those facing the greater cut in the tax rate. (Goolsbee 1999, however, argues that other reforms in the United States did not lead to significantly stronger growth among the most affected individuals.)

Having found that gross incomes tended to increase less in the treatment than in the control group, the question is whether this is because the treatment group reduced its labour supply and/or because gross wage rates for these individuals have fallen (in each case relative to the control group). The results in the remaining rows of Table 9 suggest that (relative) reductions in both hours worked and (especially) the wage rate play a significant role in explaining the relative decline in the gross incomes of the treatment group.

As noted earlier, the thrust of these results is robust to a number of extensions, including the use of different definitions of income (including secondary jobs and

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[39] The *P*-value, based on the one-sided test of $H_{L0}^{I\lambda}$, is 10.8% (11.2% with the extended treatment group).

casual employment), and excluding under-employed individuals (working less than 10 hours per week), but for brevity, these results are not reported here. We have also confirmed that the results are not likely to be driven by mean reversion, as detailed in the Web Appendix at www.economic-polic.org.

All this assumes, however, that survey respondents were fully compliant. The next subsection relaxes this heroic assumption.

6.2. Allowing for tax evasion and under-reporting

In the presence of tax evasion, the allocation of individuals into control and treatment groups is more complicated. One problem is that gross incomes can no longer be inferred from reported net incomes. As discussed in Section 5.4, we shall instead take the consumption data in RLMS as estimating true net income, to which we add taxes to obtain gross income. Armed with such estimates, the question arises as to whether an individual whose gross income so calculated is greater than the higher rate threshold, but who under-declares and so pays tax at the lower rate, should be allocated to the treatment or control group. If such individuals continue not to pay tax after the reform, it may seem that they were unaffected. In reality, though, they will be affected, because the cost and benefits of under-declaration have changed. We therefore include them in the treatment group.[40]

The attraction of using consumption as a proxy for true total net income is that it is less likely than income declared in the survey to be under-reported. And indeed we find that consumption, even restricted to non-durable goods, is for many survey families much higher than reported net income. Unlike income, however, consumption data are only available in RLMS at the family level. Individual level consumption data would have enabled the analysis to continue at the individual level, but family level consumption data may be more reliable in that the person providing the information about household consumption will presumably be aware of most major expenditure occurring in the household although not necessarily about the incomes financing the consumption. Formally, for household j comprising individuals k, we thus estimate gross family income as:

$$I_j = c_j + \sum_k \tau_k(I\lambda). \tag{12}$$

The use of consumption data provides a further unrelated advantage: consumption is a much better proxy for permanent income than even truthfully reported current income, since the latter may be affected by temporary shocks. Individuals with a positive (negative) income shock would be wrongly allocated to the treatment (control) group, biasing the results by making a fall in income among the treatment group (and a rise in the control group) more likely. To prevent similar effects from the

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[40] The point is similar to that which led us to consider individuals close to the threshold as being affected: while such individuals did not face higher tax rates, they are still affected by the tax cut since the costs of avoiding higher tax rates change.

Table 10. Changes in PIT paid at household-level, treatment and control defined on basis of reported income

		Control: No higher rate payer	Treatment: At least 1 higher rate payer	Difference-in-differences
Levels	No. of families	1108	138	
	Level, pre-reform	43.6	286.1	
	Change in total tax paid	23.5	−33.1	−56.6** (23.3)
Growth rates (%)	No. of families	1074	138	
	Mean growth rate	152.5	−2.9	−154.4*** (22.6)
	Median growth rate	39.0	−3.6	−42.6*** (6.8)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. The levels are monthly tax figures in 1995 rubles. The treatment group contains all families with at least one higher-rate taxpayer, on the basis of individual reported incomes. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

purchase of expensive consumer durables, the consumption variable used here includes only expenditure on non-durables.

We now define the treatment group as those families whose gross family income from employment (estimated as described above, less any pensions or unemployment benefits) per working adult suggests that they would have had one higher rate taxpayer if their incomes had been fully taxed. This, in turn, we take to be the case if gross employment income per working adult is equal to or greater than the threshold at which the higher rate applied prior to reform. Of course, this will exclude some families with members who would have been subject to the higher rate, since even lower family incomes can lead to higher tax rates if they are not equally distributed across working age adults.

To check that any differences in results from the previous section are not simply due to moving from individual to household data, Table 10 reports regressions in which the treatment group consists of families that (under the assumption of full compliance) included at least one higher rate taxpayer before the reform. Thus the only difference between this and Table 8 above is that the groups are defined by applying the same criterion at individual and household levels. The results indicate that simply moving to household data does not yield qualitatively different results, in that there is again significantly stronger performance of PIT revenues in the control group than in the treatment group.

Table 11 presents results obtained using the evasion-robust criterion for allocating households to groups. (Interestingly, with this criterion the treatment group almost doubles in size – which in itself already suggests enormous under-reporting, given that the consumption based definition of the treatment group is likely to be a lower bound on the number of families with at least one affected individual). The results show that even controlling for tax evasion and misreporting, income grew more slowly in the treatment group than in the control group, though it is only for the specification with median growth rates that we can formally reject the hypothesis of higher growth in the treatment group.

Table 11. Changes in PIT paid at household-level, treatment and control defined by consumption-based estimate of gross income

		Control	Treatment	Difference-in-differences
Levels	Number of families	1040	206	
	Level, pre-reform	53.9	153.8	
	Change in total tax paid	19.3	7.2	−12.1 (18.0)
Growth rates (%)	Number of families	1009	203	
	Mean growth rate	140.8	99.6	−41.2 (35.4)
	Median growth rate	36.7	20.7	−16.0** (6.5)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

Table 12. Difference-in-differences estimates for PIT, total tax and declared income, at household-level

		Difference-in-differences
Levels	Change in PIT	−12.1 (18.0)
	Change in total tax	−49.8 (58.2)
	Change in declared income	9.4 (159.1)
Growth rates (%)	Mean growth rate, PIT	−41.2 (35.4)
	Mean growth rate, total tax	−2.3 (15.1)
	Mean growth rate, declared income	−1.1 (13.4)
	Median growth rate, PIT	−16.0** (6.5)
	Median growth rate, total tax	−8.7** (3.5)
	Median growth rate, declared income	−7.8* (4.2)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Declared income is gross of PIT and social tax. Control and treatment group defined using the consumption-based estimate of gross income. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

Table 12 repeats the analysis for the payment of PIT and social insurance combined, and for declared incomes. The findings with total labour taxes are very similar to those with PIT only: all differences are negative, and those comparing medians even significantly so. For declared income, the increase in levels for the treatment group was higher than in the control group, although not significantly different from zero. Using growth rates, the difference is again negative, although significantly so only in case of median regressions: only in this latter case can we reject the hypothesis of higher declared income growth.

Table 13 shows that relative gross incomes fell in the treatment group. To establish whether this is due to reduced labour supply or to lower wage rates, Table 14 considers separately gross wages per hour and the number of hours worked, suggesting that the fall in gross income was due only to a reduction (as theory predicts) in the gross wage rate. Hours worked, for which the theoretical prediction is ambiguous, did not change differentially across groups.

Table 13. Difference-in-differences estimates for gross income, at household-level

		Control	Treatment	Difference-in-differences
Number of families		1040	206	
Levels	Pre-reform	969.4	3076.0	
	Change	293.6	−565.1	−858.6*** (204.6)
Growth rates (%)	Mean	50.3	−2.8	−53.1*** (5.6)
	Median	25.2	−13.9	−39.0*** (4.6)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Declared income is gross of PIT and social tax. Control and treatment group defined using the consumption-based estimate of gross income. Asterisk indicate the level of significance one (***) five (**) and ten (*) percent.

Table 14. Difference-in-differences estimates for hours worked and wage rate, household-level

		Difference-in-differences
Levels	Change in hours worked	1.80 (3.83)
	Change in wage rate	−12.7*** (3.0)
Growth rates (%)	Mean growth rate, hours worked	2.7 (4.2)
	Mean growth rate, wage rate	−55.8*** (7.2)
	Median growth rate, hours worked	0 (0.2)
	Median growth rate, wage rate	−36.9*** (5.1)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Declared income is gross of PIT and social tax. Control and treatment group defined using the consumption-based estimate of gross income.

6.3. Effects on compliance

A key concern in evaluating the 2001 reform is the nature and extent of any impact on compliance. The approach adopted here – using consumption data to proxy net incomes – enables this to be estimated. As reported in Table 15, the implication of this approach is that prior to the reform, individuals in the treatment group declared on average only 52% of their incomes, while those in the control group declared 74% (which in itself suggests that higher tax rates are associated with lower compliance). The reform leaves compliance among the control group virtually unchanged, but for the treatment group it increases by 17 points, to 70%. The weighted average growth rate of compliance in the treatment group is thus about one-third. The unweighted average and the median growth rates are very different at 125% and 36%, suggesting a very skewed distribution. All approaches, however, lead to the impression of very strong improvements in compliance in the treatment group, which implies a failure to reject $H_{x0}^{I\lambda}$.

The results above clearly depend on the assumption of unchanged savings behaviour. This is a concern, since it might plausibly be that members of the treatment group increased their savings rate following the reform, perhaps expecting the reform not to be temporary and so perceiving a transitory income gain. Our estimate of

Table 15. Difference-in-differences estimates for compliance, at household level

		Control	Treatment	Difference-in-differences
Number of families		1040	206	
Levels	Pre-reform	74.4	52.3	
	Change	−0.3	17.4	17.7*** (4.2)
Growth rates (%)	Mean	24.0	124.6	100.6*** (23.5)
	Median	2.4	35.8	33.6*** (6.5)

Notes: Heteroscedasticity-robust standard errors in parentheses. For median regressions they were obtained with a bootstrap procedure. Declared income is gross of PIT and social tax. Control and treatment group defined using the consumption-based estimate of gross income.

compliance would then be biased upwards, since in Equation (11) consumption would increasingly understate true net income. To check for this possibility we have examined savings data. We found that very few families report any savings: 49 in the control and 12 in the treatment group. While we do not find a statistically different change in savings rates between the two groups, this is not surprising given the small sample size and further analysis does not seem worthwhile. But while very few families report any monetary savings, they may be saving by purchasing durable goods. We have therefore also analysed expenditure on durable goods, and found that this has fallen in the treatment group relative to the control group – the opposite of what one would expect if the latter had perceived a transitory income gain. It therefore seems unlikely that a differential change in savings underlies the finding of an apparent increase in compliance.

Though the results in this respect appear reasonably robust, they clearly need to be interpreted with care; the point estimates of the degree of compliance, in particular, rest on strong assumptions. Moreover, it may be that any improvement in compliance was due not to the parametric tax reform but to a synchronous strengthening of tax administration. Nevertheless, the potential magnitude of the implied improvement in compliance is striking.

One other piece of indirect evidence to be found in the RLMS is of interest. Respondents are asked how far they feel that decisions not to pay tax reflect a perception that others do not pay. Their answers to this are likely to indicate their own beliefs as to the extent of compliance by others. Table 16 compares responses to this question before and after the reform. There is clearly a noticeable reduction in the proportion of survey participants – by around one-third – feeling that perceived non-compliance by others was very important to the compliance decisions. The proportion thinking it important to some lesser degree, however, increased, rather clouding the picture. Overall, however, the proportion of respondents thinking compliance not very important increased from about 71 to 82%.[41]

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[41] The decline in the stock of enterprise tax arrears around the time of the reform – in the year from January 2001, consolidated tax arrears declined from 4.3 to 3% of 2001 GDP – is also suggestive of improved compliance. Again, however, this does not bear as directly on the 2001 reform as one would wish.

Table 16. How important is the perception that no one else pays taxes to compliance decisions? (in percent of responses)

	Very important	Important	Somewhat important	Irrelevant
1998	26	39	21	13
2002	17	46	25	11

Note: These questions were not included in the 2000 and 2001 surveys. We have therefore chosen the surveys closest in time to the reform, which did contain these questions.

Source: RLMS.

6.4. Summary of results and anatomy of the increase in PIT revenue

The evidence presented here suggests that the increase in PIT revenues following the 2001 reform was mainly the result of developments among individuals who were largely unaffected by it. There is no evidence of positive supply responses, since gross incomes fall in the treatment group and hours worked are largely unaffected. The only, but potentially important, positive effect detected in the treatment group was an improvement in compliance.

While these results imply that the reform did not cause the revenue boom, it is still of interest to understand the contribution to that strong revenue performance of the various factors at work. Using the household data, we calculate that tax revenues (from PIT only) increased by 25.2% in real terms between 2000 and 2001.[42] This overall rise is due mainly to growth of 35.7% in the control group, while tax payments by the treatment group grew by only 4.7%.

The increase in the PIT rate on low incomes will explain part of this revenue increase. To investigate this, we have calculated how much tax would have been raised in each group if incomes and compliance rates had remained unaffected by the reform. In this case, tax receipts from the treatment group would have fallen by 11.4%, while those from the control group would have increased by 0.8%. Overall, tax receipts would have fallen by 3%. So the tax increases in the lower part of the income distribution were not sufficient to compensate for the tax cuts higher up the distribution.

The increase in PIT revenue is thus explained by the increase in declared gross incomes. The nature of this increase, however, differs greatly between treatment and control groups. The declared incomes of the control group increased by 27.5%, most of which (23.9 percentage points) come from higher income rather than improved compliance. In the treatment group, on the other hand, declared incomes increased by 17%, and this was all due to improved compliance – gross incomes for this group in fact fell quite strongly. Given their far greater importance in the overall tax base,

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[42] This is slightly different from the figure in Table 6, because it is based on household rather than individual data. This is necessary, as we can only estimate compliance rates at the household level.

however, it is the increase in the incomes of the control group – not the improved compliance of the treatment group – that drives the overall improvement in revenue.

If social security contributions are added to the picture, then revenues from the treatment group actually fell, even taking improved compliance into account. The reform did not 'pay for itself'.

7. CONCLUDING DISCUSSION: WHAT LESSONS FOR OTHERS?

The 2001 Russian 'flat tax' reform has been one of the most influential and widely emulated tax reforms of recent years. Understanding its effects is far from simple, however, given not only the limitations on data – the panel data we use do not include direct observations on tax payments or true gross incomes – but also the wide range of tax and other changes occurring at the same time. In particular, the synchronous substantial reduction in social insurance contributions means that the change in the PIT structure was not the only major change in labour taxation in 2001, so that changes in PIT revenue, and in such other quantities of interest as hours worked and wage rates, cannot be attributed to changes in the PIT alone. Nevertheless, the analysis here does provide some insights into the effect of the PIT reform.

Most important, it is hard to attribute the very strong performance of PIT revenues after the reform to tax reform itself. This conclusion emerges from both the broad macro evidence and, even more strikingly, from the analysis of micro-level data. Evidence of the latter kind is especially compelling in the context of the Russian tax reform, since its structure was such as to have little impact on many individuals and households, so lending itself to a difference in differences analysis, comparing developments between a control group of those little affected and a treatment group of those quite strongly affected. And that analysis shows quite robustly that the strongest growth in PIT payments came systematically from those who were little affected by the reform. This is true, for example, whether or not one assumes that survey respondents report their true disposable incomes truthfully, and is robust to a range of empirical strategies. Nor are there any clear signs that the increase in PIT revenue was due to large-scale shifting of income from the corporate to personal sectors. There is, in short, no strong evidence that tax reform itself caused the PIT revenue boom.

Nor is there any evidence that the rate reduction had any strong incentive effect, with labour supply changes over this period being essentially the same for both those affected and those unaffected. What can be found in the data is a significant reduction in the relative gross wage rates of those most affected by the reform. All this is consistent with, for instance, the elasticity of supply of higher-paid labour being low relative to that of labour demand.

This is not to say, however, that the reform did not have significant behavioural effects. For the evidence also points to a marked increase in tax compliance following the reform, with an increase of around 17 percentage points in the proportion of their

income declared by those affected by the reform. Though the precise estimate should be treated with great caution, there are clear signs of a significant effect.

One cannot necessarily attribute this improvement in compliance to the parametric tax reform itself, however. It might reflect the efforts at improved enforcement undertaken at around the same time. There is, however, no obvious way of exploring this potential explanation with the data available to us. One could hope to identify effects from any differential changes in enforcement (audit rates and the like) across the regions (enabling a further dimension of differencing), but we have not been able to obtain information of this kind. Whatever its cause, however, there are signs that a strong improvement in compliance did take place around the time of the flat tax reform, and that this at least mitigated the revenue loss otherwise associated with the parametric reform.

It is natural to ask what lessons can be drawn for other countries considering the adoption of similar reforms. The key conclusion is simply the need for prudence: the Russian tax cuts did not pay for themselves through their effects on work effort or compliance. The strength of PIT revenues in Russia over this period was largely driven by an increase in real wage rates apparently unrelated to the reform. This may have been associated with strong energy prices, wider structural reforms or simply a return to more normal trend levels, and in any event a full understanding is likely to hinge on features of the Russian labour markets – the explanation lies beyond the scope of this paper, but in any event is inessential to this key point. These structural differences may in part account for the difference between the present results for Russia and the finding of strong effects for the United States by Feldstein (1995), although Goolsbee (1999) argues that even in the United States such findings are rather atypical.

Many of those same structural features and rigidities will be present in other transition countries, at least the slowest reformers among them, and so would also be likely to intermediate the effects of similar reforms there. To the extent that the apparent improvement in compliance in Russia was conditioned on the initially high level of non-compliance there, similar effects might be expected, *prima facie*, in other countries with similar levels of non-compliance – and similarly chaotic tax administration. It is in such circumstances, for example – where non-compliance is rampant, and the prospects of monetary penalties for evasion have little impact – that cutting tax rates is in principle most likely to improve compliance. The estimates of the hidden economy[43] reported in Table 17 – which are subject of course to many reservations – show significant heterogeneity, but it seems that Russia has not been atypical in the extent of hidden activities, implying that similar improvements in compliance could be achieved elsewhere – although, it should be stressed again, it is not clear how far the improvement in Russia was due to the parametric tax reform or to improved administration.

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[43] Most measures used in transition economies are based on electricity use, the idea being to use differences in the growth rates of electricity usage (as a proxy for the real economy) and reported GDP to deduce changes in the size of the shadow economy. There are other methods, however, which are briefly summarized in Eilat and Zinnes (2000, pp. 17–20). All methods, it should be noted, are better suited to identifying changes in the extent of the hidden economy rather than its level.

Table 17. Estimates of the share of the hidden economy in transition countries

Country	EZ	JKZ	Lacko	FU	Average
Russia	33	41	28	35	34
Average of other countries	32	29	27	38	32
Armenia				37	37
Azerbaijan	48	59	33	55	49
Belarus	20	19	31	28	25
Bulgaria	28	33	26		29
Croatia	25		28		26
Czech Republic	19	15	19		18
Estonia	41	19	27	31	29
Georgia	55	63	38	55	53
Hungary	26	29	24		26
Kazakhstan	28	34	28	30	30
Kyrgyz Republic	67		27	59	51
Latvia	26	35	30	25	29
Lithuania	29	25	32	24	28
Macedonia	50		32		41
Moldova	56	38		47	47
Poland	14	14	21		16
Romania	11	18	24		17
Slovakia	12	11	23		15
Slovenia	25		19		22
Tajikistan	52				52
Turkmenistan	11			24.6	18
Ukraine	46	47	35	47.2	44
Uzbekistan	8	8	22	34	18

Notes: The figures give estimates of the share of the shadow economy in total economic activity. The estimates given are the average for 1994–95, except for FU which are the average for 1989–2001.

Sources: EZ, JKZ, Lacko: all quoted from Eilat and Zinnes (2000), where EZ are their own estimates, the other two are Johnson *et al.* (1998) and Lacko (1999). Eilat and Zinnes report the ratio of the shadow to the official economy, which we have converted to a share in the total economic activity. Feige and Urban (2003) report results for different specifications, we quote those estimated with GLS.

The lessons learned from the Russian experience, however, might be of interest not only to transition economies. According to Schneider and Enste (2000), the estimated size of the shadow economies in some OECD countries – such as, for example, Greece, Italy, Spain, Portugal and Belgium – is comparable to that estimated for transition economies, and the trend during the 1990s was an increase in the scale of shadow economies in many OECD countries. They conclude that 'an increasing burden of taxation and social security payments, combined with rising state regulatory activities and labour market restrictions' are the driving forces behind an increased size of the shadow economy. The Russian 2001 reform suggests that reducing the tax burden might indeed lead to the contraction of shadow economies, but reinforces the obvious warning that revenue may suffer in the process.

There is one widely heralded aspect of the Russian reform not addressed in the analysis reported above that, it is worth noting, seems unlikely to have had much effect. This is the simplicity of the rate structure – its 'flatness' – in itself. Table 18 reports the responses of RLMS participants to the question of whether the complexity of the

Table 18. How important is the perception the system is too complicated to compliance decisions? (in percent of responses)

	Very important	Important	Somewhat important	Irrelevant
1998	26	37	23	14
2002	21	45	23	10

Note: These questions were not included in the 2000 and 2001 surveys. We have therefore chosen the surveys closest in time to the reform that did contain these questions.
Source: RLMS.

tax system is itself an impediment to compliance. While there is some reduction in the numbers thinking complexity very important, there is also an increase in the proportions thinking it important or somewhat important. Little sign here that the tax system has become massively simpler. This is not to deny the potential value of simple tax design in fostering compliance and reducing the scope for corruption. The key point, it seems, is that complexity is much more than a matter of the number of rates: it depends too on the range of allowances available, procedures to be followed, and so on. Though obvious, the point is often overlooked by the more ardent advocates of flat taxes.

The Russian experience is potentially highly instructive for many countries, especially those facing severe compliance problems. But the central lesson is of the need for caution. Whatever the reason for the PIT revenue boom in Russia, it lies in something other than behavioural responses to the adoption of the 'flat tax'.

Discussion

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It is not surprising that the idea of a flat tax was first introduced and discussed in the United States. Yet the first countries to implement it were those of Eastern Europe. In 1994 Estonia introduced a flat tax on personal and corporate income at a uniform rate of 26%. Latvia and Lithuania followed suit, later came Russia with a rate of 13% on personal income, and then Slovakia. The conventional wisdom is that this reform, labelled the 'flat-tax revolution' by its proponents, has been a success.

This paper about the Russian flat tax reform (by far the most important of the recent reforms to take place in a country of such a size) is a well-balanced analysis, with equal appeal to both the advocates and the detractors of the flat rate tax. The gist of it is that the Russian tax reform, by resulting in an important increase in revenue (about 26%), has worked. However, this success has more to do with a strong enforcement policy than with the so-called advantage of flat taxation, namely the reduced excess burden.

Let me begin by stating the two main attributes of a flat income tax, which are that it applies the same tax rate to everyone and to each income component, and that it allows the tax base to be computed with no deductions, except for personal exemptions and strictly defined business expenses. Without personal exemptions the tax is proportional; with personal exemptions, it is progressive in that average taxation increases with income, while marginal taxation is constant.

Advantages and problems

Proponents of the flat income tax claim that lowering marginal tax rates for high income earners reduces both the excess burden of the tax system and the incentives to cheat. They also claim that the simplicity of the new tax lowers administrative costs and improves taxpayer morale.

By contrast, its detractors claim that the flat tax is less progressive, since it redistributes some of the tax burden from the rich to the middle classes. They also point to problems of transition from one system to the other, with heavy implications in terms of horizontal equity. These detractors tend to underscore the incentive issue by assuming labour supply to be quite inelastic. The proponents, on the other hand, argue that the flat income tax can be made progressive through a suitable choice of the exemption level.

The Russian reform

The Russian reform appears to consist not only of a flat income tax reform, but also of a reform of social contributions and corporate tax, as well as value added and turnover taxes. Hence, the observed revenue increase cannot be attributed exclusively to the flat tax reform. More importantly, these tax reforms have been accompanied by a serious increase in the legal power of tax authorities. It is difficult to isolate the effect of each one of these changes.

Using both macro- and micro-evidence, the authors show quite convincingly that most of the revenue increase does not result from the tax reform itself. Indeed, using micro data, they show that those taxpayers not affected by the reform contributed to the revenue increase as much as, if not more than, those taxpayers affected by the reform. Since the tax reform coincided with a reinforcement of controls and audits, they see there the main reason for the 26% revenue increase.

Other countries who might be tempted to undertake the same reform might consider this finding important, and thus decide not to change the rates so much as the enforcement.

Comments

Even though the paper is clearly presented and its arguments are convincing, some clarifying remarks are in order.

- The way tax evasion and hidden activities are introduced is very casual. Estimates of the shadow economy are not reliable. We know that these phenomena exist and are pervasive, but that is all. I do not see any purpose for the data presented in Table 17.
- The theoretical discussion of tax evasion, particularly of the effect of a tax cut on tax evasion, is too partial. General equilibrium effects have not been taken into account. Further, because of asymmetric behaviour, a tax increase can lead to more tax evasion even if a tax cut does not imply a reduction in tax evasion.
- The paper focuses on the personal income tax reform. But we know that there have been many other tax changes. Combining the social tax and the income tax, we end up with an overall regressive tax schedule.
- It would be interesting to have some information on the redistributive incidence of the Russian tax reform. My conjecture is that a flat tax reform could be desirable from a viewpoint of horizontal equity, and undesirable from a viewpoint of vertical equity.
- Finally, the flat tax reforms conducted in some Eastern European countries are often presented as successful in terms of additional revenue. Could this also be due to increased enforcement?

Despite the above reservations, I like this paper for its clear treatment of a difficult issue, and for the fact that its pragmatic and balanced conclusion could go a long way to tempering the passions of the proponents of the 'flat-tax revolution'.

Andrés Velasco
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A country reforms a policy (streamlines taxes, say) and then enjoys extraordinary results (revenue booms, the economy grows). That wise policy change is behind the boom, argue government economists and their friends. Policy makers from other countries, always on the lookout for a panacea, are tempted to imitate the new policy. Not so fast, reply sceptics. A number of other things changed at the time of the policy reform. They could well be responsible for the good results.

This debate has played itself out again and again in emerging market economies over the last couple of decades. In the early 1990s, countries in Latin America stabilized inflation, cut fiscal deficits, lowered tariffs, privatized and deregulated. Several of them boomed (in too many cases, only to crash later). Which of the policy changes, if any, was responsible for the burst in growth? Or was it capital inflows, favourable terms of trade, or some other exogenous factor? How could one tell?

Now the debate is being replayed in Russia, in the aftermath of a wide-ranging tax reform. Along with other countries in Eastern Europe and the former Soviet Union, Russia moved to a flat tax rate on personal income, in this case of 13%. Revenue boomed, with a 26% real increase the following year. Reported wages also grew, as did tax compliance. Advocates of the reform want to claim credit, arguing that tax

simplification pays for itself. The problem for analysts is that, during the same time period, the Russian economy was recovering from the financial crash of 1998, and energy prices – all important for Russia – were sky-high.

How to parse out the effect of different factors in assessing the impact of the reform? This paper proposes a clever answer. Using survey data, it divides respondents into a treatment group (the relatively rich, affected more by the reform given their initial income and tax presumably paid) and a control group (the rest). Then the paper looks at how the labour supply, reported wages and tax evasion of two groups behaved in the aftermath of the tax changes. If the treatment group displays larger changes in any of these variables, then they can plausibly be attributed to the reform.

The main result is that gross incomes increased less in the treatment than in the control group. Relative reductions both in the wage and in labour supply seem to explain the difference. This gives sceptics some ammunition. However, the paper also finds that compliance increased by a lot more in the treatment group. The authors conclude that the reform elicited no strong supply effect, but it did help reduce tax avoidance, and quite significantly so.

I have no deep quibbles with the approach or the results it yields. On the contrary, such a 'difference-in-difference' approach has the potential to help settle this important question, which is relevant not just to Russia but to many other countries applying broad-ranging reforms. And the general message of the paper feels right: there were some gains – particularly in compliance – from the reforms, but not enough to claim that reforms of this kind more than pay for themselves. But a few points ought to be kept in mind when interpreting the results.

An initial point is that it is quite demanding to look for an actual increase in taxes paid in the control group. Recall that the tax rate fell for this group. One has to be on the 'wrong' side of the Laffer curve for income (whether actual or reported) to rise so much that the effect on the tax base more than offsets the fall in the tax rate. So one should not be surprised that no such thing is found among the members of the control group.

The approach also requires that the treatment and the control group be affected differently by the reform, but that they be similar in other relevant respects. However, this is not always the case. One concern is that the income of the control group (the relatively poor) may be more pro-cyclical than that of the treatment group (the relatively rich). Given that the Russian economy was in a boom phase at the time, this could explain why gross incomes increased more in the control group.

A related concern is relevant to the result that tax compliance increased more in the treatment group, with the implication that this was a beneficial effect of the reform. But is this because the two groups responded differently to the new structure of tax incentives, or simply because at the time of the changes enforcement went up more for the control group? This is what you would expect at any time authorities announce they will crack down on tax evaders, since those in the treatment group are

the relatively rich, out of whose income the authorities can get the most additional revenue.

A final concern of this sort has to do with the role of savings. The survey includes only wage income, and capital income is completely omitted. Nor does the survey provide information on gross income or tax payments, only on net income. The former have to be inferred using some assumptions, one of which is that households do not save. This assumption is reasonable for the poorer households, but not necessarily for the richer ones. So we have another potentially distorting asymmetry between the treatment and the control groups.

Last but not least, I have a not entirely frivolous quibble with the title of the paper. 'The Russian "Flat Tax" Reform' suggests a comprehensive analysis of the reform and its consequences. Flat tax reforms are often argued to have efficiency effects (presumably positive) and distributional effects (presumably negative). The analysis here focuses mostly on revenue effects, with a little attention paid to efficiency (supply effects), and no attention at all to distribution. There is nothing wrong with that – good papers do not try to say something about everything. But the reader should be warned about the actual focus of the paper.

Panel discussion

Some panellists sought to clarify precisely what the authors were seeking to show. Eduardo Levy Yeyati was not sure that the paper actually said anything about the *flat* component of the tax, as it was shown the rate was not actually flat. Assaf Razin was similarly concerned that the question of the paper was not clear: were the writers investigating the effect of the tax cut via a Laffer curve effect, or the effect of the flatness of the tax? The authors confirmed that their aim was not to explain tax revenue in Russia, nor to test the consequences of 'flatness' *per se*, but rather to test whether changes in the tax rates were the direct cause of the revenue boom. They also pointed out that a flat tax is *not* much easier to administer, the most complicated part in tax administration typically being the calculation (and verification) of taxable income. Carlo Favero expressed some concern that the paper was about a flat tax of 13%, although it was quite possible that 13% was not the optimal tax rate.

There was an extended discussion of compliance. Giancarlo Corsetti suggested a mechanism for compliance could be through *coordination* where transactions are *visible*. For example, suppliers and buyers may need to coordinate their compliance, and in this way compliance may emerge in pairs or groups. Georges De Menil continued, explaining that peoples' decisions to avoid tax are not isolated, since individuals are affected by their beliefs of the actions of others. (Some survey evidence on this is reported in the published version). Corsetti also pointed out that total evaders may not be visible at all, which would bias the results, for example towards too many low

income individuals. Andrea Prat suggested that tax compliance may be higher when a tax is perceived as fair; the authors replied that the tax reform was 'sold' as being fair, on the basis of increased compliance by the rich, though the public may have been sceptical. The authors added that credibility of the tax reform was also important for compliance, and anecdotal evidence says this was not high. However, measurement of the effect of credibility would be difficult as reforms have been so frequent.

There was also some discussion of the assumption that savings were unaffected by the reform. Christian Gollier was not persuaded that it was reasonable to think that the tax reform had no effect on saving; for wealthier people it would be particularly implausible, particularly if the reform was not guaranteed for the long term. Assaf Razin also remarked that the use of consumption as a proxy for income might be particularly problematic for richer groups.

Pierre-Olivier Gourinchas remarked that Russia's current account turned around to move into significant surplus at that time, so there must have been an increase in savings at the aggregate level. This implies there was some mismeasurement of savings in the sample, which would also bias compliance rates. The authors replied that while the current account suggests an increase in gross national savings, dynamics of private savings might have been different.

Pierre Cahuc wondered to what extent the difference-in-difference methodology was appropriate for big changes in an economy. As the wealthy invest more, this will improve the wages of low skill workers; according to the difference-in-difference methodology, there will be no overall effect, but there will have been a causal effect of the reform on activity. Lans Bovenberg pointed out that elasticities differ between income groups and may also be higher in the long run. Another problem with the difference-in-difference methodology is that business cycles may affect income groups differently. It would be hard for the authors to measure the normal effect of the cycle, as constructing the counterfactual of what would have happened with the two groups in the absence of the tax reform would require the data for a period when no tax reform took place; this, however, is impossible with Russian data, as the tax regime was modified essentially in every year.

Ricardo Faini was interested to know more institutional detail, for example, what type of penalties were legislated. Bovenberg agreed and thought more information on tax enforcement, and whether it was targeted at particular income groups might be very useful. The authors responded that the evidence relating to such matters was too anecdotal to be reliable.

DATA APPENDIX

Data sources

Panel data: RLMS
GDP deflator: IMF *World Economic Outlook*.

RLMS

The individual and household data used in the panel data analysis are from the Russian Longitudinal Monitoring Survey (RLMS) of the Carolina Population Center at the University of North Carolina. The raw data comprise 10,975 (12,121) individuals and 4,006 (4,528) households in the year 2000 (2001). Data are also available for the years 1994 to 1996, 1998, and 2002, but are not used in this paper, except for the opinion surveys reported in Table 16 and Table 18. These are based on data from 1998 and 2002, as the 2000 and 2001 surveys did not include these questions.

The data set contains hundreds of variables. Those of interest are described in Table A1.

Calculation of tax payments

When calculating tax revenues and pre-tax incomes, care has to be taken to account correctly for the personal allowances individuals are entitled to. This is complicated by the fact that the total yearly allowance depends on the monthly pattern of income receipts. For instance, in the year 2000 an individual received a monthly allowance of 264 rubles until the month in which cumulative yearly income exceeded 20,000 rubles. From that month onwards the allowance was 132 rubles until the cumulative yearly income reached 50,000 rubles, beyond which there was no further entitlement

Table A1. Selected RLMS variables

Variable name	Explanation
Individual level data:	
pjpayt	Average monthly wage over the last 12 months after taxes from primary employer.
wagelm	Money received after tax in last 30 days from primary job.
otjwag	Money received after tax in last 30 days from secondary job.
inciea	Money or equivalent from additional employment.
pwrkwh	Hours worked in usual week at primary employment.
owrkwh	Hours worked in usual week at secondary employment.
hrsieapw	Hours worked in additional employment during last 30 days.
ownent	Whether respondent is owner or co-owner of enterprise.
Household level data:	
alcohln, breadn, dairyn, eatoutn, eggsn, fatn, fishn, fruitsn, meatn, ofoodn, potaton, sugarn, vegetn	Nominal expenditure on different food items.
fuelaun, clothn, fuelgsn, fuelwdn, paymntn, rentun, servicn, tobacn, assistn, luxurn	Nominal expenditure on different non-food non-durable items.
ncat1 – ncat6	Number of individuals falling into following categories: 1: children <7 yrs; 2: children 7–18 yrs; 3: working-age males; 4: working-age females; 5: post-work age males; 6: post-work age females.

to an allowance. In order to be able to calculate the yearly allowance, an assumption needs to be made about the distribution of wage payments over the year. We assume that the same nominal wage is paid evenly throughout the year, although in theory there was an incentive to postpone payments to later months, to benefit as long as possible from the allowances. This incentive is unlikely to be strong, however, given the low level of the allowance. Any additional allowances, such as those linked to dependents or incapacity, are ignored.

Having obtained an estimate of the allowance, the remaining calculations are straightforward applications of the published tax rates and thresholds. As taxes are assessed at the individual level, there is no further difficulty in calculating taxes at the household level, which are just the sum of individual taxes.

Cleaning

Individual data

The raw data consist of 23,096 observations, 10,975 from 2000 and 12,121 from 2001. Table A2 shows how many observations are lost owing to cleaning for each of a list of criteria. Note that the total number of lost observations is smaller than the sum for each individual criterion, because individuals can fulfil more than one of the criteria. Dropping individuals who own their own business, for example, only leads to the additional loss of 17 individuals in 2000, once the previous criteria have been applied.

Statistics reported in the paper are based on the data cleaned only on the criteria in the upper half of the table (7,913 observations). The regressions are generally based on the balanced panel with the *pjpayt* variable present (4,828 observations or 2,414 individuals).

Table A2. Cleaning of individual-level data

Criterion	Observations dropped (remaining)
Keep if between 20 and 60 years of age throughout 2000 to 2001	10400
Drop if do not report how many hours they work or claim to work more than 84 hours per week	4516
Drop if do not report any income from primary employment	14131
Drop if own their own business	63
All of the above	15183 (7913)
Additionally	
keep if report *wagelm* (necessary for Table 6)	1117 (6796)
keep if report *pjpayt*	704 (7209)
keep if present in both 2000 and 2001	2437 (5476)
keep if present in both 2000 and 2001 and report *wagelm*	3637 (4276)
keep if present in both 2000 and 2001 and report *pjpayt* (basis for regressions from Table 7 onwards)	3085 (4828)

Table A3. Cleaning of household-level data

Criterion	Observations dropped (remaining)
Keep if can be matched to individual data, in case of family split keep larger part of a family	743 (7791)
Additionally	
keep if at least one working-age adult in household	1977 (5814)
keep if report *pjpayt* for at least one member of household	2963 (4828)
both of the above	3207 (4584)
Additionally	
keep if households present in 2000 and 2001 and the household composition does not change	2092 (2492)

Household data

The raw data consist of 8,534 observations, 4,006 from 2000 and 4,528 from 2001. Table A3 reports the number of observations lost owing to cleaning.

WEB APPENDIX

Available at: www.economic-polic.org

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