

CHAPTER 2

THE EFFECT OF TAXATION ON INFORMAL EMPLOYMENT: EVIDENCE FROM THE RUSSIAN FLAT TAX REFORM

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ABSTRACT

The 2001 Russian tax reform reduced average tax rates for the personal income tax and the payroll or social tax. It also made the tax structure more regressive. Because individuals in the lower income bracket were for the most part not affected, it is possible to estimate the effects of the reform using a differences-in-differences approach. I study the effect of the reform on informal employment. Informality is defined using information on employment registration and self-employment. Applying parametric and semi-parametric techniques, I find evidence that the tax reform led to a significant reduction in the fraction of informal employees. Among the different forms of informality I study, the reform seems to have had the strongest effect on the prevalence of informal irregular activities. I also document stronger effects on individuals who benefited from the largest reductions in tax rates. The strong response to the tax reform is consistent with the emerging consensus in the literature on

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taxation that changes to the tax system lead to significant behavioral responses, although not necessarily in the form of a reduced labor supply.

Keywords: Informal sector; entrepreneurship; tax reform; differences-in-differences; transition; Russia

JEL classification: H24; J3; O17; P2

INTRODUCTION

The high prevalence of informality is a well-known characteristic of labor markets in developing countries.¹ Informal work is by nature heterogeneous. It includes self-employed individuals, as well as those working under them (often a few family members and friends). It also extends to those employed by larger organizations but who are not effectively covered by any of the institutions – such as the pension system and other social insurance – that protect formal employees. While for the most part the economic activities of informal workers are legal, they are often not taken into account in official statistics, and much of the income they generate goes untaxed.

An important unresolved issue is to what extent informality in the labor market is responsive to the level of taxation. Trying to answer this question brings about some serious challenges. First and foremost, observable variation in taxation levels is generally endogenous and therefore inappropriate for estimating a causal effect. In this paper, I exploit a natural experiment in order to establish a causal link between the tax wedge – the overall tax burden imposed on employers and employees – and the incidence of informal employment.

In 2001, Russia introduced a tax reform that drastically reduced taxation levels for upper income brackets. The pre-reform progressive personal income tax rates were replaced by a flat and low rate of 13%. Payroll taxes were also modified. Before the reform, employers had to make contributions – adding up to 38.5% of the gross salary – to four different social funds. Starting in 2001, these contributions were unified into a single social tax with a regressive scale. If lower levels of taxation causally affect informality, then such a comprehensive tax reform should have had a measurable impact. Because individuals in the lower income brackets were almost unaffected, the reform also created well-defined treatment and control groups. Thus, the effect of the tax reform on informality can be

estimated following a differences-in-differences (DID) strategy. Intuitively, the DID estimator captures the post-reform average drop in the probability of participating in the informal sector experienced by the treatment group relative to the control group.

Other specific institutional features of the tax reform make it a worthy object of study. First, the speed with which the reform was discussed and implemented made any anticipation effect extremely unlikely. The details of the changes to the tax code only started being discussed in the middle of the year 2000. Also, less than 50 days elapsed between the day the reform was announced to the day of its final presidential approval. Second, the flat tax schedule eliminated any incentives to report income just below the critical threshold separating treatment and control groups. Had the reform reduced taxes for the upper brackets while retaining “progressive” marginal tax rates, the treatment variable would be susceptible to systematic measurement error around the threshold. With a flat tax, however, income misreporting is still present but probably a much less serious problem for estimation purposes.

While the tax reform provides an extraordinary opportunity to shed light on the research question, it is nevertheless not perfect. Treatment status is determined by the individual’s tax bracket, which in turn is bound to be correlated with informal employment. The paper addresses this issue in a number of ways. First, the treatment effect is estimated conditional on time-varying observable characteristics as well as time-invariant observables and unobservables. Second, I show that these estimates are robust to a wide range of reasonable modifications to the treatment definition. Third, I use two pre-reform years to study the “effect” of a placebo reform (and, reassuringly, find none). Fourth, I implement a weighted DID estimator that identifies the treatment effect by relying heavily on individuals whose income is closer to the bracket threshold and who therefore are less likely to differ systematically in time-varying unobservable characteristics. Finally, I also estimate the treatment effect using the matching DID estimator originally developed by Heckman, Ichimura, and Todd (1997).

The research question also requires addressing a second challenge, namely measuring labor market informality to a reasonable degree of precision. My main data source is the Russian Longitudinal Monitoring Survey (RLMS). As is standard in the literature, informal status is determined on the basis of self-reported information on the type of production unit and registration of the employment relation. The wealth of data in the RLMS permits identifying informality in up to three distinct “jobs” or remunerated activities: the main job, a secondary job, and

irregular activities. In addition, the paper takes advantage of a special supplement on informal employment that was added to the main questionnaire of the RLMS in 2009. I use these additional data to cross-validate the definitions of informal employment.

The main findings of the paper are that after controlling for observable characteristics and individual fixed effects, employed individuals who were affected by the tax reform were on average 2.5% less likely to be informal employees on the main job and 4% less likely to perform informal irregular activities.² On the extensive margin, I find that individuals who were not employed right before the reform and found a job in its aftermath were also less likely to be informally employed.

This paper builds upon the small number of existing studies of the Russian tax reform.³ Using a similar methodology to the one employed here, Ivanova, Keen, and Klemm (2005) and Gorodnichenko, Martinez-Vazquez, and Sabirianova-Peter (2009) documented positive effects of the reform on public revenue and tax compliance at the household level. Duncan and Sabirianova-Peter (2009) find that the reform resulted in very modest increases in male and female hours of work. Taken together, these studies and the present paper support the conclusion common in the modern literature on taxation, that the main form of response to changes in the tax system is not through labor supply but through other – equally important – margins of adjustment (Saez, Slemrod, & Giertz, forthcoming).

This paper is also closely related to the burgeoning literature on the determinants of the size of the unofficial economy. The unofficial – also called shadow, hidden, or underground – economy refers to the production, whether legal or illegal, of goods and services for the market that escapes detection in the official estimates of GDP (Schneider & Enste, 2000). While the definition and the units of measurement of informal employment are different, in practice there is a strong overlap between the two concepts since a large proportion of informal work is probably not registered in official statistics and vice versa. One widely accepted interpretation is that underground economic activity is a response to excessive involvement of the State in the economy in the form of intrusive regulations and high levels of taxation. Among post-communist countries, there is evidence that only those which succeeded in limiting the political control of economic activity (at the same time as they improved the provision of key public goods necessary for the good functioning of markets) seem to have managed to keep the growth of the unofficial economy under control (Johnson, Kaufmann, Shleifer, Goldman, & Weitzman, 1997; Mcmillan & Woodruff, 2002).

Modern Russia seems like a perfect illustration of the theory linking excessive government intervention and the shadow economy. Russian managers face higher effective tax rates, worse bureaucratic corruption, greater incidence of mafia protection, and have less faith in the court system than their peers in Slovakia, Poland and Romania, and that seems to go some way in explaining why Russia's underground economy is relatively larger (Johnson, Kaufmann, McMillan, & Woodruff, 2000). Also, Russia inherited an unregulated sector from the Soviet times. Grossman (1977) coined the term "second economy" for the set of illegal and quasi-legal economic activities that individuals engaged in to put up with or exploit the severe rationing of goods and services under communism. Such activities encompassed the cultivation of small plots of land, simple stealing from state enterprises, speculation, illicit production at secondary occupations, and many others. In 1990, almost 15% of personal income of workers and employees had informal sources (Kim, 2003). In other words, the incipient Russian market economy inherited the ability to avoid regulation by the state when such regulation is too costly or otherwise excessive (Gerxhani, 2004; Guariglia and Kim, 2006). Using different methods and definitions, several studies have documented a rising share of the underground activity in Russia during the 1990s (Lacko, 2000).⁴

There are, however, reasons to believe that the statistical association between excessive regulation and a growing unofficial sector is not causal. Firms might decide to operate underground mainly in order to avoid predatory behavior by government officials rather than regulations per se (Johnson, Kaufmann, and Zoido-Lobaton, 1998). If that is the case, then it is not so much the letter of the law – for example, mandating high taxes – that influences informality but rather the discretionary authority of administrative officials in the context of a corrupt administrative system. To the extent that informal employment is a good proxy for unofficial activities, my estimates of the effect of the tax reform can also be interpreted as a test of the theory that the size of the shadow economy is boosted by excessive regulation.

The paper is organized as follows. The next section briefly discusses the data and the definition of informal employment. In the third section I present a descriptive analysis of informal workers in Russia using alternative data sources. The fourth section focuses on the structure of the tax reform and the definition of the treatment and control groups. The fifth section presents the main results for the intensive and extensive margins, as well as a number of robustness checks. The sixth section concludes.

INFORMALITY DEFINITION AND MEASUREMENT

The main data source for this study is the RLMS. In this section I briefly describe the RLMS and discuss my working definition of informal employment.

Data and Variables

The RLMS is a household panel survey based on the first national probability sample drawn in the Russian Federation.⁵ I use data from rounds VIII–XVIII covering the period 1998–2009. In a typical round, 10,000 individuals in 4,000 households are interviewed. These individuals reside in 32 oblasts (regions) and 7 federal districts of the Russian Federation. A series of questions about the household (referred to as the “family questionnaire”) are answered by one household member selected as the reference person. In turn, each adult in the household is interviewed individually (the “adult questionnaire”).

The adult questionnaire includes questions regarding a primary and a secondary job. In addition, individuals are also asked whether they perform what I will refer to as “irregular remunerated activities.” The exact phrasing of this last questionnaire item is as follows: “Tell me, please: in the last 30 days did you engage in some additional kind of work for which you were paid or will be paid? Maybe you sewed someone a dress, gave someone a ride in a car, assisted someone with apartment or car repairs, purchased and delivered food, looked after a sick person, sold purchased food or goods in a market or on the street, or did something else that you were paid for?” The questionnaire structure is such that no one may answer questions on a secondary job unless they have a primary job. However, questions on the irregular activities are independent.⁶

Fig. 1 shows the employment and the unemployment rate, according to the RLMS and the standard labor force survey conducted by Rosstat. While the two data sources display some minor discrepancies,⁷ all series show that the period under analysis was – at least employment-wise – one of relative economic prosperity and stability.

In order to gain further insight into the meaningfulness of my informality variables, I also make use of a special supplement of questions on informal work (INFSUP⁸) that was added to the RLMS adult interview in 2009 (round XVIII). The INFSUP questionnaire was administered to all employed individuals after the regular interviews had been completed.⁹

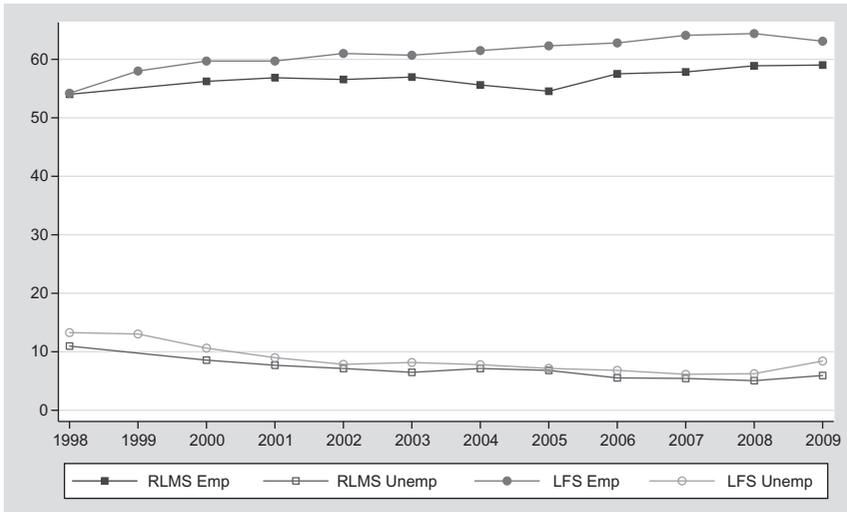


Fig. 1. Employment and Unemployment Rates. Note: RLMS, rounds VIII–XVIII and Rosstat labor force survey (1998–2009).

Definition of Informal Employment

As has been clearly put in a recent book-length study by the World Bank: “The term informality means different things to different people, but almost always bad things: unprotected workers, excessive regulation, low productivity, unfair competition, evasion of the rule of law, underpayment or nonpayment of taxes, and work ‘underground’ or in the shadows” (Perry et al., 2007). The idea of the informal sector was originally adopted and popularized by economic anthropologist Keith Hart (1973) and a series of studies sponsored by the International Labour Office (ILO, 1972). Since the beginning, the concept was meant to comprise heterogenous labor practices including petty trading, self-employment of different sorts, own-account professionals, family workers, and other forms of non-standard (from a Western perspective) work prevalent in developing countries. Moreover, many of the initial bounds of the concept were eventually trespassed in one way or another. For example, while the informal sector was originally thought to be predominantly urban, it was quickly accepted that it should also include some forms of small-scale agricultural work. Despite these ambiguities – and partly thanks to them – the concept has proved useful to researchers with a wide range of interests.¹⁰

While the literature widely recognizes the blurry bounds of the concept, there are two most commonly used definitions of informality. On the one hand, the so called “productive” definition focuses on a number of characteristics of the production unit (Husmanns, 2004). First, informal sector enterprises typically include only private unincorporated units, that is, enterprises not constituted as separate legal entities independently of their owners. Second, at least part of the goods or services they produce is meant for sale or barter. Lastly, their scale of operations is assumed to be very small. In fact, when better data is lacking, informal enterprises are often defined as those whose size in terms of employment is below a given threshold (typically less than five employees).

On the other hand, the “legalistic” or social protection definition focuses on the status of workers in relation to labor law and the social safety net. It measures to what extent workers are effectively – as opposed to only *de jure* – protected by labor market institutions. Informal employment occurs in cases of noncompliance to the State in terms of labor regulations and the social security system.

In this paper I use both legalistic and productive criteria to determine if an individual is informally employed. Table 1 shows a schematic representation of the different employment types and my working definition of informality in each case. Throughout the paper, I analyze informality at the main job, the secondary job and the remunerated irregular activities separately.

At the main job, I start by distinguishing between entrepreneurs and employees. The former group is composed of those doing entrepreneurial activities who are either owners of firms or self-employed individuals who work on their own account with or without employees but not at a firm or organization.¹¹ Following the productive definition, those not working at firms or organizations are considered informal. For those working at firms or organizations, the RLMS questionnaire includes an item that permits determining whether they are registered, that is, working officially.¹² The Russian labor code mandates that all employees sign a written contract and deposit their “labor book” with the employer. Therefore, following the social protection criterion, I classify unregistered entrepreneurs and employees as informal.

Some firms in Russia register their employees but declare a fictitious salary that is lower than the real amount in order to reduce the base of payroll and other taxes. The difference between the declared and the real salary is settled with an “envelope payment” at the end of the month. If such a practice were widespread, the registration criterion could err on the side of underestimating the extent of informality. Fortunately, the 2009 round of the RLMS included an item on envelope payments. As I show below,

Table 1. Working Definitions of Informal Work.

Employed	Main job	Entrepreneur	Firm owners ^a	Formal
				Informal
			Individual entrepreneur ^b	Informal ^c
		Employee	For firm	Formal
				Informal ^d
			For individual entrepreneur	Informal ^c
	Second job			Formal
				Informal ^e
	Irregular activities			Formal
			Informal ^f	

^aFirm owners work for a firm or organization that they own and where they perform entrepreneurial activities. Considered informal if unregistered.

^bIndividual entrepreneurs do entrepreneurial activities independently (not within a firm or organization).

^cRegistration information is not available for those not working within firms or organizations.

^dEmployees are considered informal if they are not registered.

^eInformal in second job if unregistered or not working for a firm or an organization.

^fIrregular activities involve remunerated work like sewing a dress for someone or giving someone a ride in a car. Considered informal if not employed under official contract or agreement.

workers employed formally rarely admit to high levels of income under-reporting. Thus, considering envelop payments would not significantly increase measured informality levels.

While using the productive definition to classify all self-employed individuals and their employees as informal is standard practice, it would be reassuring if the social protection criterion could be applied as well. Unfortunately, a limitation of the RLMS data is that non-enterprise individuals are not asked about registration, so it is not possible to apply the legalistic definition to them.¹³ However, thanks to the INFSUP we have some good indication of the extent to which self-employed individuals comply with the regulations. As I show below, the level of compliance is quite low, so choosing between the legalistic and productive definition does not make a big difference for these workers. The supplementary questions also confirm that there is a high correlation between lack of registration and other forms of noncompliance with labor regulations.

In principle, the RLMS questionnaire contains enough detail to treat the main and the second job symmetrically. However, the number of observations would not be large enough for a meaningful statistical analysis of the resulting sub-categories. For example, only about 40 individuals per round do entrepreneurial activities in the second job. Therefore, a single category of informal work in the second job is considered, consisting of those unregistered¹⁴ plus those not working for a firm or organization.

Finally, I consider remunerated irregular activities. Based on the productive definition, all employment of this kind could be classified as informal. However, since not much information is available regarding these activities I only consider them informal if the respondent gives a negative answer to the question: “Tell me, were you employed in this job officially, for example by an agreement, an official contract, or a license?”¹⁵ This methodological decision is unlikely to affect results¹⁶ since almost 87% of irregular work is done without a contract.

According to these definitions, Fig. 2 shows the evolution of informal employment participation rates over the period and provides some

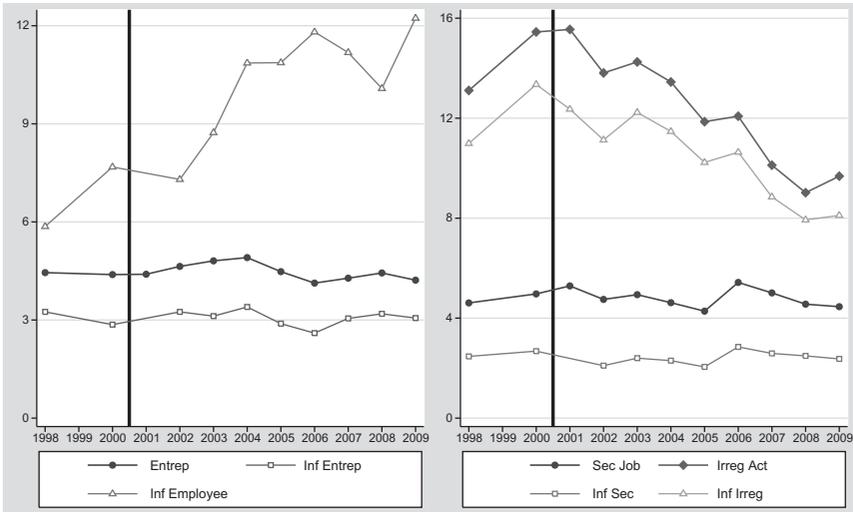


Fig. 2. Informality at Main Job, Second Job, and Irregular Activities. *Note:* RLMS, rounds VIII–XVIII (1998–2009). The left panel shows informality at the main job, disaggregated into employees and entrepreneurs. The panel on the right shows informality at the second job and remunerated irregular activities. The series for the main and the second job are defined as a percentage of those with a main job. For the irregular activities, the base are all those employed.

preliminary evidence on the likely effects of the tax reform. First, the fraction of entrepreneurs in the main job – both formal and informal – has remained stable at around 4.5%. Second, informality among employees has risen almost uninterruptedly and toward the end of the period is well into the double digits. Eyeballing the time series suggests the tax reform might have caused a deceleration of the rate of growth of informal employment in the short run. However, no long run effect is apparent. Third, the percentage of second job holders of any kind has also not changed much during these 11 years. Informality in the second job is relatively uncommon. Finally, *prima facie* there seems to be a strong negative effect of the tax reform on the prevalence of irregular activities, informal or otherwise. This is important since, at least until the reform was implemented, irregular activities were the most common form of informal work in Russia.

However, simple before-after comparisons are risky. Fig. 3 presents the evolution of real hourly wages for workers in the formal and informal sector. Real incomes were increasing over the period and, to the extent that a growing economy induces formalization, it might well be the case that the tax reform had very little to do with the downward trend in informal irregular activities after 2001. On the other hand, if what matters are *relative*

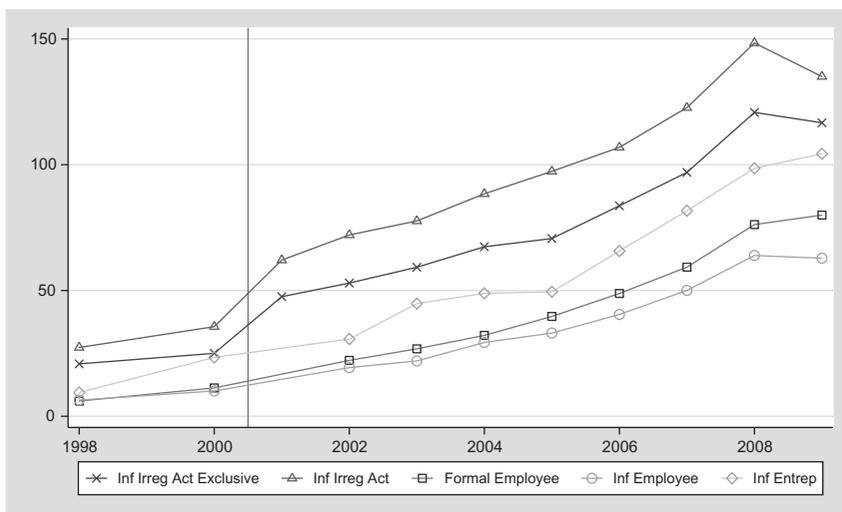


Fig. 3. Real Hourly Earnings (in 2,000 Rubles). Note: RLMS, rounds VIII–XVIII (1998–2009). Real hourly earnings are monthly receipts divided by usual hours and deflated by the CPI. Informal irregular activities exclusive means that the individual held no other job.

rewards between the formal and informal sector, then it seems unlikely that the modest changes in relative wages shown in the figure might explain quantitatively large sectoral shifts.

DESCRIPTION OF INFORMAL EMPLOYMENT IN RUSSIA

While my working definition of informality has many antecedents,¹⁷ it is also somewhat idiosyncratic to the extent that the questionnaire items of the RLMS are unique and that not much is known about the informal sector in modern Russia. There could be legitimate concerns regarding the correctness of the resulting measure of informality.

Fortunately, some insight can be gained thanks to the wealth of information in the regular RLMS survey and in the INFSUP. In this section, I show that workers that are informal according to my definition have many of the characteristics found in other studies. I also present evidence that alternative definitions, while reasonable, would probably not affect the results.

Demographics

Table 2 provides demographic information on informal workers in Russia toward the end of 2009. The table confirms many of the empirical regularities observed in other countries. For example, informal employees tend to be low skilled. Only around 12% of them has a college degree and their level of schooling is below that of the average Russian worker. They are also relatively younger, predominantly male and less experienced. Workers performing informal irregular activities¹⁸ seem to show many of the same characteristics, although a larger share of them live in rural areas and belong to one of the many ethnic minorities.

As in other countries, individual entrepreneurs in Russia are relatively well off. While they are less educated than average workers, their qualifications are not as low as those of informal employees. Entrepreneurs are also relatively more likely to marry and form a family.

Individuals who participate in the informal sector through a secondary job also have higher than average incomes. In almost all other respects, however, they are difficult to be distinguished from the average worker.

The 2009 round of the RLMS included an item on “envelope payments.”¹⁹ Formal employees answered that 92% of their earnings were reported

Table 2. Background Characteristics of Informal Workers in Russia.

	All Employed	Informal Employee	Informal Entrepreneur	Informal Second Job	Informal Irregular Activities
Female	0.54	0.49	0.42	0.56	0.45
Age	39.5	36.4	40.1	38.9	38.6
College degree	0.27	0.12	0.23	0.28	0.15
Schooling (years)	12.3	11.5	12.1	12.5	11.4
Experience	14.3	9.2	14.4	14.8	11.3
Married	0.51	0.42	0.66	0.48	0.42
Urban location	0.77	0.76	0.80	0.88	0.63
Russian national	0.87	0.86	0.77	0.86	0.81
Russian born	0.91	0.88	0.82	0.87	0.92
Size HH	3.4	3.5	3.6	3.0	3.4
<i>"After-tax" income</i>					
This job (rubles)	13,194	11,043	18,661	7,142	7,043
% Reported for tax	86.6	32.0	62.9	NA	NA
All jobs (rubles)	13,446	11,132	18,878	17,024	12,470
Observations	7,192	815	204	158	583

Note: The data source is RLMS, round XVIII (2009). Employed workers are those with a job or who do remunerated irregular activities. Informal employees are those who work for a self-employed individual or who work for a firm or an organization but are not registered. Informal entrepreneurs are either self-employed or owners of a firm who do entrepreneurial activities but are not registered. Informal second job includes both informal employees and informal entrepreneurs in their second job, regardless of the main job status. Informal irregular activities are other remunerated activities conducted without formal contracting.

to tax authorities. In turn, informal employees and individual entrepreneurs reported having paid taxes on a significantly lower fraction of earnings. While responses to such sensitive issues cannot be taken at face value, the high correlation between informality and declared tax avoidance is reassuring.

Informal workers overwhelmingly belong to unskilled and service occupations, and work in the trade and construction industries.²⁰ This is consistent with the idea that informal workers work in occupations/industries with low barriers to entry – that is, requiring almost no start-up capital or specific knowledge.

Job Characteristics

The standard RLMS survey offers some detailed information regarding the characteristics of the job,²¹ which I present in Table 3.

Informal employees have relatively weak attachment to the job, as indicated by the low observed average tenure. Moreover, the probability of

Table 3. Job Characteristics for Informal Workers in Russia.

	All Employed	Informal Employee	Informal Entrepreneur	Informal Second Job
Tenure (years)	7.3	2.8	7.2	2.5 ^a
Changed jobs	0.16	0.35	0.13	NA
Changed occupation	0.11	0.21	0.06	NA
Has subordinates	0.20	0.08	0.38	0.10 ^b
<i>Firm characteristics^c</i>				
Enterprise size (# of employees)	584.4	61.8	–	76.2
State owns share	0.50	0.06	–	0.20
Russian individual owns share	0.56	0.91	–	0.70
Firm from Soviet times	0.59	0.09	–	0.40
Firm owes money	0.07	0.13	–	0.19 ^d
Firm pays in kind	0.01	0.03	–	0.02 ^d
<i>Job benefits^c</i>				
Paid vacation	0.90	0.17	–	0.19
Paid sick leave	0.87	0.11	–	NA
Paid maternity leave	0.79	0.07	–	0.17
Paid health care	0.24	0.01	–	0.05
Paid trips to sanatoria	0.28	0.01	–	0.03
Paid child care	0.05	0.01	–	0.01
Assistance w/food	0.12	0.04	–	0.03
Assistance w/transport	0.12	0.03	–	0.01
Paid educational activities	0.25	0.02	–	0.04
Assistance w/loans	0.05	0.01	–	0.00
Observations	7,192	815	204	158

Note: The main data source is RLMS, round XVIII (2009). Definitions are the same as for Table 2.

^aFrom round XVI (2007).

^bFrom round XVII (2008).

^cOnly for those working for firms or other organizations.

^dFrom round XIV (2005).

transition implies a mean job duration of only 1.5 years ($\approx 1/1-0.35$). Informal second jobs seem to have short durations too. While we lack information regarding average duration of irregular activities, over 66% of workers answered affirmatively to a specific item asking whether these activities were “incidental.” Interestingly, however, this is not the case with informal entrepreneurs, who have below average transition probabilities.

According to the RLMS, almost 90% of non-enterprise individuals work alone or with a few family members.²² Consequently, a number of items in the adult questionnaire are only asked to individuals who work for firms.²³ First, respondents are asked about the size of the firm. Table 3 confirms that informal employees work for firms that, while larger than a family enterprise, are still much smaller than average.²⁴ This is also true of individuals who are informal in the second job.

Second, there are questions regarding firm ownership and origin. The issue of informality and the shadow economy in Russia is often discussed in the context of the transition from the Soviet system (Johnson et al., 1997; Mcmillan & Woodruff, 2002). A familiar argument is that the incipient capitalist sector makes use of informal arrangements to escape confiscatory intrusions by the State. The data is consistent with this story. The involvement of the Russian State in the economy is very substantial. This is reflected not only in the relatively high prevalence of state ownership but also in the fact that almost 60% of employment in Russia is still supplied by enterprises that originate in Soviet times. Informal employment is, however, almost exclusively provided by new private firms.

A third important set of questions touch on the issue of wage arrears. Faced with a negative shock, firms in Russia often choose to adjust via delaying the payment of wages (Gimpelson & Kapeliushnikov, 2011; Lehmann, Wadsworth, & Acquisti, 1999). Predictably, Table 3 shows that wage arrears and payments in kind happen relatively more frequently to informal employees.

Finally, the RLMS asks enterprise workers regarding fringe benefits. Paid vacation, sick leave, and maternity leave are mandatory benefits according to the labor code and a large majority of employees claim to have them. However, many firms do not provide these benefits in practice. For example, only 66% of those employed had *actually* been on paid vacation in the previous 12 months, compared to the 90% who claim to have entitlement. In any case, the proportion of informal employees who are given the mandatory benefits is substantially lower than average.²⁵ Non-mandatory benefits are infrequent in Russia, and almost non-existent for informal employees.

Table 4. Informal Activities Last Year.

	All Employed	Informal Employee	Informal Entrepreneur	Informal Second Job	Informal Irregular Activities
Worked extra job	0.09	0.08	0.08	0.96	0.33
Raised cattle for sale	0.04	0.03	0.04	0.03	0.14
Agriculture on own plot for sale	0.04	0.02	0.03	0.04	0.14
Performed services for pay	0.08	0.08	0.06	0.11	0.61
Observations	7,192	815	204	158	583

Note: The data sources is RLMS round XVIII.

While informative, these questions provide insight into only a minority of informal jobs, that is, those which happen at firms or organizations. Nevertheless, the RLMS includes a series of questions on informal activities during the previous 12 months that are asked to everyone. A summary of these items is in Table 4.

Two points are noteworthy. First, 9% of those employed reported having worked an informal second job in the previous year. Reassuringly, the agreement with informality in the second job according to my definition is almost perfect.

Second, almost 40% of individuals who perform irregular activities live in rural areas. Not coincidentally, a significant proportion of them are involved in small-scale agriculture and husbandry. However, by far the largest share of these activities involves personal services: taxi rides, repair work, hair styling, tutoring, nursing, etc.

Compliance with the Law

Table 5 contains statistics based on answers to the INFSUP. An important cautionary note is that the INFSUP consisted of a stand-alone questionnaire that was administered to all individuals who had any form of employment. Respondents answered informality-related questions about two jobs (henceforth²⁶ job-A and job-B). Unfortunately, these jobs are not certain to correspond to those of the standard adult questionnaire.²⁷ I proceed as follows. I assume that the information about job-A corresponds to the main job if such a job is present. For individuals without a main job, I assume job-A must refer to (the main) remunerated

Table 5. Compliance with the Law.

Supplement for Employees	All Employed	Informal Employee	Informal Second Job	Informal Irregular Activities
Under oral agreement	0.11	0.69	0.81 ^a	0.96 ^b
% Labor law compliance	83.1	52.9	NA	53.2 ^b
% Contract compliance	86.1	64.3	NA	65.5 ^b
% of income declared for SS	87.6	31.2	NA	10.5 ^b
Observations	6,453	777	80	186
Supplement for Entrepreneurs	All Employed	Formal Entrepreneur	Informal Entrepreneur	Informal Irregular Activities
Unregistered	0.48	0.03	0.27	0.98 ^b
% Labor law compliance	64.4	85.9	53.6	21.3 ^b
% Contract compliance	66.3	87.5	55.5	27.5 ^b
% Formal employees	64.0	85.7	53.4	8.3 ^b
Contributes to SS fund	0.47	0.95	0.60	0.06 ^b
Observations	397	64	194	126

Note: The data sources are RLMS round XVIII and the supplementary questionnaire on informality by the Centre of Labour Market Studies, Higher School of Economics, and the Labor Markets in Emerging and Transition Economies Research Program, IZA (2009).

^aBased on job-B answers by individuals who do not perform irregular activities.

^bBased on job-A answers by individuals who do not have a main job.

irregular activity. In fact, all statistics on informal irregular activities are based on the latter group. Finally, I assume that job-B refers to the secondary job as long as the individual does not also perform irregular activities. This is the source of information on informal secondary jobs.

A second issue is that the INFSUP asks a different set of questions regarding job-A depending on whether the individual is an entrepreneur or an employee. While for the most part individuals who identify themselves as entrepreneurs in the INFSUP are also classified as such based on the adult questionnaire, the correspondence is not perfect. I base the statistics only on individuals for whom the classifications coincide.

A positive spillover is that the INFSUP provides us with some idea of the composition of remunerated irregular activities. A stunning 40% of these workers consider themselves entrepreneurs.

Working under an oral agreement is strictly forbidden under Russian labor law. The INFSUP asks all employees in job-A whether they have a written contract. This question is important for validating my working definition of informality, since the adult questionnaire only has registration information for enterprise workers in the main job. Remarkably, over 97% of those who work under an oral agreement according to the INFSUP are classified as either informal employees or individuals whose only source of income originates in informal irregular activities.

The supplement also asks employees about the extent to which their employers comply with labor law and the specifics of the individual labor contract or agreement. These items are interesting because registration is only one of the many mandates of labor law. The average workplace has compliance levels well over 80%. Informal workers report significantly lower levels of compliance. These figures are consistent with the finding (Table 3) that absence of mandatory benefits and wage arrears are more frequent for informal employees. Finally, employees are also asked about the percentage of their earnings that is reported for social security purposes. In general, responses are very much in agreement with a similar item in the RLMS adult questionnaire (Table 2). Thanks to the INFSUP, however, we have information on those performing irregular activities. Predictably, tax compliance is extremely low for this kind of jobs.

The questionnaire for entrepreneurs provides information regarding registration of business operations. In Russia, the self-employed can either register individually or as a company. While some form of registration is necessary to operate formally, it is unclear whether it is sufficient. Practically all of the few formal entrepreneurs in the RLMS sample are registered according to the INFSUP. Moreover, 64% of registrations are in the form of incorporated businesses. On the other extreme, individuals performing irregular activities are overwhelmingly unregistered. In between, a majority of those classified as informal entrepreneurs in the main job are registered, but only 17% of them have an incorporated business.

Entrepreneurs are also asked a number of questions regarding their employees. On the one hand, in formal firms labor law and contract compliance is high, the share of informal work is low and contributions to social security are very frequent. Informal entrepreneurs, on the other hand, report much lower levels of compliance, specially in the irregular activities sector.

Overall, the information in the regular adult questionnaire of the RLMS and the INFSUP confirm that my working definition of informality is

meaningful and that informal workers in Russia share many of the characteristics documented in other countries.

THE TAX REFORM

In January 2001, Russia introduced a radical reform of its tax system. The main components of the reform are shown in Table 6. A number of changes involved the personal income tax (PIT). Before 2001, the PIT had a progressive scale with marginal rates starting at 12% and reaching 30%. The new system fixed a flat and low rate of 13%. The reform touched other aspects of the PIT. The standard allowance was slightly increased, from 3,168 to 4,800 rubles but now could only be claimed by those earning less than 20,000 rubles. Also, the number of permissible deductions and other loopholes was greatly limited.

Before the reform, employers were supposed to make separate contributions – adding up to 38.5% of the gross salary – to four independent social funds. The reform replaced this system with a unified social tax (ST) with a regressive scale. It also eliminated the 1% employee contribution to the social fund.

Table 6. The Russian Tax Reform.

	Gross Yearly Income (RUR)	Before 2000			After 2001		
		PIT	ST		PIT	ST	
			Employee	Employer		Employee	Employer
<i>Control</i>	< 3,168 ^a	0			0		
	3,168–4,800 ^a	12	1	38.5	0	0	35.6
	4,800–50,000	12			13		
<i>Treat</i> ¹	50,000–100,000	20					35.6
<i>Treat</i> ²	100,000–150,000	20					20
<i>Treat</i> ³	150,000–300,000	30	1	38.5	13	0	20
<i>Treat</i> ⁴	300,000–600,000	30					10
	> 600,000	30					2 ^b

Note: The data source for the Personal Income Tax (PIT) and Social Tax (ST) is the Russian Tax Code, part 2 (2001–2002).

^aThe tax allowance in 2001 was only available to those with income below 20,000 rubles.

^bRate initially set to 5% and lowered to 2% in 2002.

Overall, the message of the reform was unambiguous. The government was offering a new deal to the Russian public: lower taxation levels and a more reasonable system. In exchange, it expected higher levels of compliance. The response from the public has been widely regarded as positive. Tax compliance improved significantly and government revenue increased despite the lower average tax rates (Gorodnichenko et al., 2009; Ivanova et al., 2005).

Identification of the Tax Reform Effect

The combined effect of the PIT and ST reform can be seen in Fig. 4. The tax reform affected the costs and benefits of informality faced by all economic agents. However, some groups were more affected than others. Specifically, people earning less than 50,000 rubles per annum had a net tax reduction of only 1.4%. In comparison, those earning between 50 and 100 thousand rubles faced a reduction of 7.2%. Finally, it is clear from the graph that the greatest reductions in tax burden were received by those earning 100 thousand rubles or more.

The design of the reform created a natural experiment that can be exploited to obtain a DID estimate of the effect of lower taxation levels on

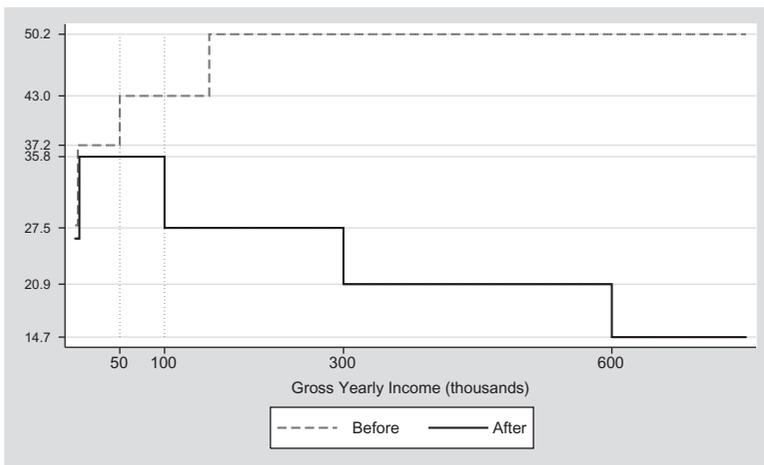


Fig. 4. Combined Tax Burden. *Note:* Russian Tax Code, part 2.

informality. Individuals earning less than 50,000 rubles a year constitute a “control group” whose marginal tax rate remained practically unchanged. People with higher incomes faced lower tax rates and therefore are considered “treated.” The DID identification strategy assumes that the evolution of participation in the informal sector for the control group can be used to estimate what would have happened to individuals in the treatment group had they not been treated.

One potential source of concern for studies that exploit natural experiments like the Russian tax reform is anticipation effects. Individuals might try to play the system by changing their behavior before the reform is implemented. While this has been found to be a real issue in the case of capital gains tax reform (Saez et al., forthcoming), it is likely not to be very relevant for labor market behavior. In any case, the speed with which the Russian tax reform was discussed and implemented made any anticipation effect extremely unlikely. The details of the changes to the tax code only started being discussed in the middle of the year 2000. Also, less than 50 days elapsed between the day the reform was announced to the day of its final presidential approval.

In practice, the determination of who belongs to the treatment group is complicated by the fact that people misreport income. In particular, a progressive tax system might provide specifically strong incentives to under-report to individuals whose actual income is just above the lower bound of a tax bracket. As formally argued by Gorodnichenko et al. (2009), the flat tax schedule in post-reform Russia means that this is not a reason of concern. Individuals just above the critical threshold of 50,000 rubles did not have special incentives to work less hours or misreport income. Moreover, because tax rates were generally lower and regressive after 2001, it is plausible that misreporting decreased in general.

Other elements of the tax system that could potentially introduce biases are itemized deductions and other allowances. The former were almost completely eliminated in the reformed tax code. Moreover, only individuals earning up to 20,000 rubles per year may claim the standard allowance of 4,800 rubles. Finally, the reform also eliminated the exception of income taxes to military personnel. Based on this discussion, the treatment group will be defined based on post-reform reported income only.

The RLMS adult questionnaire asks individuals to report their after-tax monthly earnings in each remunerated activity. These items include not only labor income but also “benefits, revenues, and profits,” while

excluding pensions and other nontaxable transfers. In order to determine treatment status, I construct an aggregate income variable that adds the amounts received from all sources. In the absence of misreporting, individuals with after-tax monthly income above 3,625 rubles²⁸ can be considered treated. If, however, income is under-reported, some individuals will be incorrectly included in the control group. Thus, the resulting DID estimate is a lower bound of the true effect of the reform on informality.

One complication is that an individual's income may be above the threshold only in some of the post-reform rounds. I consider anyone whose income is ever above the threshold as treated. I also run a series of robustness tests in which treatment is defined based on shorter periods. The control group is given by those untreated and employed in at least one post-reform period. The latter definition means that employed individuals who do not report income are included in the control group. In practice this is a very small number of individuals and, as I show below, excluding them from the control group does not affect the results.

In principle, individuals with post-reform annual income between 3,168 and 4,800 rubles could be considered treated. However, for this group the annual savings associated with the tax reform have an upper bound of 212 rubles.²⁹ Since this amount is economically insignificant, my baseline specification includes these individuals in the control group. However, as shown below, the results are almost identical if this group is included in the treatment group instead.

I report selected statistics on the control and treatment groups in Table A1 in Appendix. Over three-fourth of the sample is in the treatment group in my baseline definition. In short, the treatment group is younger and has less labor market experience, tends to be better educated, and is more likely to be married than the control group. The households of treated individuals are relatively more likely to be in urban areas, are slightly larger, and have more members who are female or young.

RESULTS

As a first step into understanding the effect of the reform, I plot the informality time series for the treatment and control groups. The upper left panel of Fig. 5 shows that the reform probably affected informal employees. Before 2001, participation in this kind of informal work was approximately the same in both groups. However, their post-reform behavior was

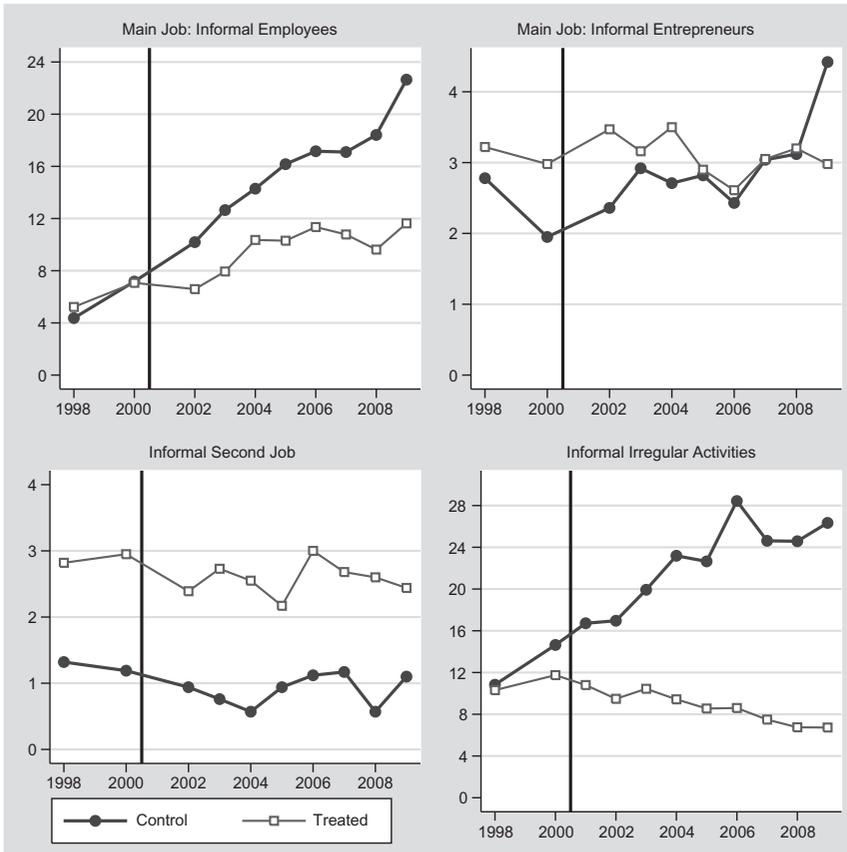


Fig. 5. Informal Employment by Treatment. Note: RLMS, rounds VIII–XVIII (1998–2009). Treatment defined based on total after-tax monthly income in the post-reform period.

very different. The prevalence of informal employees in the control group experienced a steady increase. The increase of informality among treated individuals was much less significant.

While less conspicuous, this pattern is also present for informal entrepreneurs (upper right panel). Before the reform, informality was more prevalent among the treated. By 2009, the control group had a higher proportion of informals. The bottom left panel of the figure shows that the reform did not seem to affect informality in the second job.

Finally, the bottom right panel provides compelling graphic evidence that the tax cuts worked toward reducing informal irregular activities.

Overall, Fig. 5 suggests that the tax reform was a success beyond the realm of tax compliance. The reduction in taxation levels seems to have pulled a large number of people into formal status.³⁰ However, there is some chance that the visual evidence is not statistically significant. More importantly, as shown in Table A1, there are some marked observable differences between the treatment and control groups. The figures in the previous section do not control for any of these factors. It is possible that the visual evidence is an artifact of spurious correlation.

In order to obtain statistical evidence on the effect of the reform and control for the possible confounding effect of observable characteristics, I estimate the following DID equation:

$$INF_{it} = \theta_t + X_{it}\beta + Z_i\gamma + \psi Post_t + \mu Treat_i + \alpha(Treat_i \times Post_t) + u_{it} \quad (1)$$

where INF_{it} is one of the informality-related dependent variables, θ_t are time dummies, X_{it} and Z_i represent sets of time-varying and time-invariant individual characteristics respectively, $Post_t$ is a post-reform dummy, $Treat_i$ is the treatment group indicator, and u_{it} is the error term. The main object of interest is α , the DID parameter that measures the average change in the probability of informal status for the treatment group relative to the control group, conditional on all the observables.

Table 7 presents OLS estimates of Eq. (1). The main identifying assumption of OLS-DID is that none of the unobservable characteristics that influence informality participation are correlated with treatment status. Attempting to correctly model the binary outcome variable would impose extra identification conditions, so I avoid it. I report Arellano (1987) standard errors that allow for heteroscedasticity and autocorrelation of arbitrary form.³¹

The results provide confirmation that the tax reform reduced the prevalence of informal employees. After controlling for all observable individual and household characteristics and for any macroeconomic shocks absorbed by the year dummies, the expected probability of informal status for the control group was 8% higher in the period after the reform. In contrast, informality grew 4% less among those facing lower levels of taxation. These estimates are both economically and statistically significant. The coefficients for the control variables have the expected signs. Informality is less likely among women, Russian nationals, and high-skill and married workers.

Table 7. The Effect of Tax Reform on Informality: DID OLS.

	Informal Employee	Informal Entrepreneur	Informal Second Job	Informal Irregular Act
<i>Household characteristics</i>				
Number of members	-0.0006 (0.002)	0.0003 (0.002)	-0.0054*** (0.001)	-0.0033 (0.002)
Number of female members	0.0059* (0.003)	-0.0027 (0.002)	0.0018 (0.001)	0.0039 (0.003)
Number of youth, 18–	-0.0090*** (0.003)	0.0057** (0.002)	0.0058*** (0.001)	0.0129*** (0.003)
Number of elderly, 65+	-0.0106** (0.004)	-0.0003 (0.003)	-0.0024 (0.002)	-0.0050 (0.004)
Urban location	0.0043 (0.008)	0.0178*** (0.006)	0.0045 (0.003)	-0.0228*** (0.009)
<i>Individual characteristics</i>				
Female	-0.0189*** (0.004)	-0.0161*** (0.003)	-0.0001 (0.002)	-0.0555*** (0.004)
Russian national	-0.0096** (0.004)	-0.0106*** (0.004)	0.0006 (0.002)	-0.0011 (0.004)
Age	0.0025 (0.002)	0.0087*** (0.001)	-0.0012 (0.001)	-0.0008 (0.002)
Age ² /100	0.0056** (0.003)	-0.0060*** (0.002)	0.0006 (0.001)	0.0158*** (0.003)
Experience	-0.0117*** (0.001)	-0.0043*** (0.001)	0.0017*** (0.000)	-0.0111*** (0.001)
Experience ² /100	0.0056** (0.003)	0.0011 (0.002)	-0.0027*** (0.001)	-0.0048** (0.002)
Secondary schooling completed	-0.0026 (0.007)	0.0053 (0.005)	0.0007 (0.003)	-0.0190*** (0.007)
Vocational schooling completed	-0.0023 (0.010)	-0.0044 (0.006)	-0.0000 (0.005)	-0.0171* (0.010)
Technical schooling completed	-0.0349*** (0.007)	0.0021 (0.004)	0.0011 (0.003)	-0.0484*** (0.007)
College comp	-0.0942*** (0.007)	-0.0077 (0.005)	0.0036 (0.004)	-0.0751*** (0.007)
Graduate level comp	-0.1270*** (0.010)	-0.0198*** (0.007)	0.0224** (0.011)	-0.1244*** (0.016)
Married	-0.0248*** (0.003)	0.0012 (0.002)	-0.0040** (0.002)	-0.0335*** (0.003)
<i>DID estimates</i>				
<i>Post</i>	0.0774*** (0.010)	0.0017 (0.007)	-0.0026 (0.005)	0.0089 (0.010)
<i>Treat</i>	0.0109 (0.007)	0.0072 (0.005)	0.0112*** (0.004)	-0.0049 (0.009)

Table 7. (Continued)

	Informal Employee	Informal Entrepreneur	Informal Second Job	Informal Irregular Act
<i>Treat</i> × <i>Post</i>	-0.0427*** (0.009)	-0.0060 (0.006)	-0.0010 (0.004)	-0.0722*** (0.010)
Region Dummies ^a	YES	YES	YES	YES
Year Dummies ^a	YES	YES	YES	YES
Constant	0.1475*** (0.042)	-0.1544*** (0.027)	0.0649*** (0.017)	0.2299*** (0.039)
Observations	44,452	44,452	44,452	53,769
<i>R</i> ²	0.061	0.022	0.012	0.115

Note: RLMS, rounds VIII–XVIII (1998–2009). Definitions are as in Table 2. Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form. Omitted category is no educational degree.

^aThirty-eight regional dummies, including Moscow and St. Petersburg, and nine year dummies were included but not reported.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The effect of the reform on informal irregular activities is estimated to be 7.2%. This is a very large effect considering that the overall share of workers in this category was just above 13% in 2000.

As anticipated, the regression results also show that the effect on informal entrepreneurs and informality in the second job was neither economically nor statistically significant. I conclude that the reform did not have a strong impact on these groups.

The reduction in the share of informal employment among wage and salary workers and those performing irregular activities could be due to omitted variable bias. Specifically, it could be the case that unobservable characteristics of people in the control group systematically differed from those of individuals that were treated. The panel structure of the RLMS can be used to control for individual heterogeneity by relying on within-individual changes only. The key identifying assumption of the fixed effects model is that the effect of unobservables is constant over time. Formally, this is stated by assuming that the error term in Eq. (1) can be written as: $u_{it} = c_i + \varepsilon_{it}$, where c_i is the constant individual heterogeneity and ε_{it} is an idiosyncratic error term with zero mean conditional on treatment, the other covariates, and the individual heterogeneity. As is well known, the price to be paid for the robustness of the fixed effects estimator is that none of the parameters of the time-constant regressors are identified.

Table 8 presents the fixed effects estimation results for Eq. (1). The effect on informal employees is now estimated as -2.5% , while the effect on informal irregular activities is -4.0% . Both results are still statistically significant. Attenuation in the absolute size of fixed effects estimates occurs

Table 8. The Effect of Tax Reform on Informality: DID FE.

	Informal Employee	Informal Irregular Activities	Any Informal Employment
<i>Household characteristics</i>			
Number of members	0.0010 (0.003)	-0.0088*** (0.003)	-0.0121*** (0.004)
Number of female members	-0.0040 (0.005)	0.0083 (0.005)	0.0095 (0.007)
Number of youth, 18–	-0.0003 (0.004)	0.0112*** (0.004)	0.0105** (0.005)
Number of elderly, 65+	-0.0100* (0.006)	0.0005 (0.006)	-0.0011 (0.008)
<i>Individual characteristics</i>			
Age	-0.0091 (0.010)	-0.0135* (0.008)	-0.0062 (0.012)
Age ² /100	0.0130*** (0.004)	0.0173*** (0.004)	0.0213*** (0.005)
Experience	-0.0025 (0.002)	-0.0048*** (0.002)	-0.0061*** (0.002)
Experience ² /100	-0.0008 (0.003)	0.0006 (0.003)	-0.0013 (0.004)
Secondary schooling completed	-0.0053 (0.010)	-0.0066 (0.009)	0.0037 (0.011)
Vocat schooling completed	-0.0113 (0.011)	-0.0075 (0.010)	-0.0029 (0.013)
Tech schooling completed	-0.0132* (0.008)	-0.0214*** (0.007)	-0.0174* (0.010)
College comp	-0.0276** (0.011)	-0.0394*** (0.011)	-0.0506*** (0.015)
Graduate level comp	-0.0321* (0.019)	-0.0704*** (0.025)	-0.0649* (0.034)
Married	-0.0086** (0.004)	-0.0098*** (0.004)	-0.0137*** (0.005)
<i>DID estimates</i>			
Post	0.0495 (0.099)	0.0350 (0.075)	-0.0315 (0.119)

Table 8. (Continued)

	Informal Employee	Informal Irregular Activities	Any Informal Employment
<i>Treat</i> × <i>Post</i>	-0.0250** (0.010)	-0.0403*** (0.010)	-0.0584*** (0.014)
Year dummies ^a	YES	YES	YES
Constant	0.2799 (0.306)	0.4481* (0.232)	0.2996 (0.365)
Observations	44,452	53,769	47,718
No. of individuals	11,263	12,411	11,969
<i>R</i> ² overall	0.04	0.03	0.01

Note: RLMS, rounds VIII–XVIII (1998–2009). Any informal employment includes informality at the main job, the second job or irregular activities. Other definitions are as in Table 2. Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form. Omitted category is no educational degree.

^aNine year dummies were included but not reported.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

frequently, since within-individual variation is relatively more sensitive to measurement error (Griliches & Hausman, 1986). I interpret these results as indication that, while unobservable ability bias might be a factor influencing the OLS estimates, the tax reform caused a significant reduction in informality levels.

Rather than reflecting a real reduction in overall informality, the results in this section could be illusory if the tax reform pushed individuals from one form of informal employment into others. To check against this possibility, I estimate the same equation for an index of overall informality. The estimates in the third column of Table 8 suggest that, if anything, the results for the detailed informality categories are conservative.

Robustness Checks

In Table 9, I present estimates of the tax reform effect under alternative specifications.³² I also provide estimates for all irregular activities (contractual or otherwise) and for informal irregular activities as the exclusive source of earnings.

In order to control for changes in characteristics at the regional level – such as local tax enforcement efforts, financial markets, etc. – I add to the

Table 9. Robustness Checks.

	Informal Employee	Informal Irregular Activities	Any Informal Employment	All Irregular Activities	Informal Irregular Activity as Main Job
Baseline	-0.0250** (0.010)	-0.0403*** (0.010)	-0.0584*** (0.014)	-0.0421*** (0.010)	-0.0343*** (0.009)
Including interactions	-0.0246** (0.011)	-0.0337*** (0.010)	-0.0467*** (0.015)	-0.0373*** (0.011)	-0.0295*** (0.009)
<i>District</i> × <i>Year</i>					
Control group restricted ^a	-0.0256** (0.010)	-0.0408*** (0.010)	-0.0588*** (0.014)	-0.0427*** (0.011)	-0.0350*** (0.009)
Treatment group expanded ^b	-0.0251** (0.011)	-0.0339*** (0.010)	-0.0518*** (0.015)	-0.0347*** (0.011)	-0.0276*** (0.009)
Treatment defined using income from all sources	-0.0363** (0.014)	-0.0219** (0.011)	-0.0708*** (0.019)	-0.0339** (0.013)	-0.0219** (0.011)
Treatment defined using 2001 labor income only ^c	-0.0183 (0.012)	-0.0455*** (0.014)	-0.0637*** (0.019)	-0.0514*** (0.015)	-0.0365*** (0.010)
Treatment defined using 2001–2004 labor income ^c	-0.0223** (0.011)	-0.0421*** (0.010)	-0.0517*** (0.015)	-0.0429*** (0.011)	-0.0340*** (0.009)
<i>Treat</i> × <i>Trend</i> ^d	-0.0063** (0.003)	-0.0148*** (0.003)	-0.0187*** (0.003)	-0.0159*** (0.003)	-0.0137*** (0.003)
<i>Placebo Reform</i> ^e	-0.0008 (0.012)	0.0128 (0.015)	0.0251 (0.019)	0.0055 (0.016)	-0.0074 (0.010)

Note: RLMS, rounds VIII–XVIII (1998–2009). Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form. “All irregular activities” includes those done under contract. “Informal irregular activities as main job” excludes individuals with any other form of remunerated work.

^aExcludes from control group individuals whose earnings were never reported in post-reform period.

^bThe treatment group includes individuals with yearly income between 3,168 and 4,800 rubles in any post-reform round.

^cExcludes individuals who receive treatment in the late post-reform years (see main text for details).

^dIncludes a post-reform time trend (2000 = 1) instead of the post-reform dummy.

^eThe placebo “reform” estimates are obtained by assuming that a similar change in the tax code happened between the years 1998 and 2000 (it did not). All other covariates are the same as in Table 8.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

equation interactions between the 39 districts and the year dummies. Including these additional controls does not affect the results significantly.

I also try a number of modifications in the definitions of the treatment and control groups. First, I exclude from the analysis individuals whose labor income is never reported in the post-reform period, which under my baseline definition fell in the control group. Second, I include in the treatment group individuals whose yearly income was between 3,168 and 4,800 rubles in any post-reform round. These small modifications in the implementation of the DID strategy do not lead to any significant change in the estimates.

Third, I define treatment based on an alternative income item in the adult questionnaire. This alternative includes income from all sources – some of them nontaxable – and is therefore not entirely appropriate to use for the determination of the treated.³³ Nevertheless, it is reassuring to verify that the main results hold with this alternative definition.

The table also presents estimates when treatment is defined based on income received during the early post-reform years only. As shown in Fig. 3 above, real wages were increasing over the post-reform period. As a result, in my baseline definition many individuals enter the treatment group late. It is possible that these “late comers” also had a higher propensity to become formal and are therefore driving the results. To guard against this possibility, I consider two re-definitions of treatment: (1) considering income from 2001 only and (2) considering income during the subperiod 2001–2004 only. In each case I exclude from the analysis all individuals who receive treatment (i.e., higher incomes) after the relevant subperiod. Overall, the results from these sensitivity tests are satisfactory. The estimates of the effect of the reform on informal irregular activities are larger in absolute value than in the baseline case and remain highly statistically significant. The estimates for informal employees are somewhat smaller and become statistically insignificant when treatment is defined based on 2001 income. I conclude “late comers” are not driving the main results. I further investigate the robustness to different definitions of treatment in the next subsection.

Another robustness test involves obtaining an estimate of the effect of the reform on the *time trend* of informality in the post-reform period. This alternative specification implies a much larger overall effect. For example, by 2009 the reform is predicted to have reduced informal irregular activities by $1.5 \times 8 = 12\%$.

In all these exercises, treatment is defined based on an individual’s income bracket. This raises the concern that what is driving the results is

the fact that individuals in the control group are worse-off and therefore less likely to become formal. The final set of estimates in Table 9 corresponds to a placebo regression. I (wrongly) assume that a similar tax reform happened sometime between the years 1998 and 2000. The new “treatment” variable equals 1 if the individual is in the upper income brackets (> 50,000 rubles) in the year 2000. If it were true that low-income individuals are less likely to become formal, we should find that the placebo reform had a negative and significant “effect” on informality. However, none of the estimates of the placebo reform are significantly different from zero and most have the wrong sign. It is possible to conclude that, conditional on the covariates, individuals in the lower tax brackets were not really less likely to become formal than upper-bracket individuals. It is still possible that things changed and lower-bracket individuals were less liable to become formal in the post-real-reform years. Alas, this is not testable.

*Estimating Average Treatment Effect on the Treated (ATT) Using
a Matching Estimator*

The fixed effects DID estimates seem to be robust to minor changes in specification. However, there are a number of assumptions underlying the estimating equation that are hard to relax within this parametric setting. First, the dependent variable in Eq. (1) is binary. While OLS consistently estimates the parameter of interest under the stated identifying assumptions, it has the undesirable property that the implied conditional probability of the dependent variable is linear. Second, the set of controls are assumed to enter the equation additively and under a specific (maybe incorrect) functional form. Finally, the fixed effects estimates do not restrict estimation to the region of common support of the independent variables between the treated and the control group.

In this subsection I use the matching differences-in-differences (M-DID) estimator first introduced in Heckman et al. (1997) to estimate the effect of the reform year-by-year. This semi-parametric estimator has no implications regarding the specific form of the conditional probability function. It also allows me to check whether results are robust to changes in the functional form of the control function, as well as to restricting the estimation to the region of common support.³⁴ I also use the estimator to investigate the sensitivity of the results to alternative definitions of the treatment group. The M-DID estimator is given by

$$\widehat{M}_{DID} = \sum_{i \in T} \frac{1}{N_{T,t}} \left[(INF_{i,t} - INF_{i,2000}) - \sum_{j \in C} W(i,j)(INF_{j,t} - INF_{j,2000}) \right] \quad (2)$$

where T and C are the sets of indexes for treated and control individuals respectively, and $N_{T,t}$ is the number of observed treated individuals in year t of the post-reform period ($t \in \{2001, \dots, 2009\}$).

Intuitively, the M-DID estimator compares *changes* in informality status between year t and the (pre-reform) year 2000 for each treated individual to similar changes for a set of appropriate control individuals. Which individuals are selected as controls for each treated individual depends on the weighting function $W(i,j)$. The estimates presented here use nearest-neighbor matching based on the propensity score.³⁵

Figs. 6 and 7 presents M-DID estimates (and one-standard-deviation confidence intervals) for informal irregular activities and informal employees respectively. For each year in the post-reform period, the figures present estimates obtained when treatment is defined based on labor income

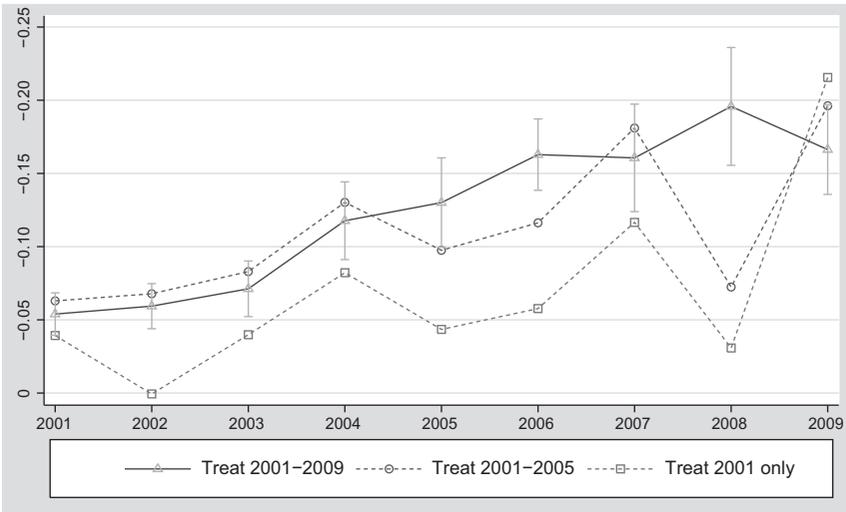


Fig. 6. Year-by-Year ATT for Informal Irregular Activities. *Note:* Reversed scale in y-axis. ATT obtained with a matching DID estimator for each post-reform year. Treatment defined based on labor income in years 2001–2009, 2001–2005, or 2001. The I-beams are one-standard-deviation confidence intervals.

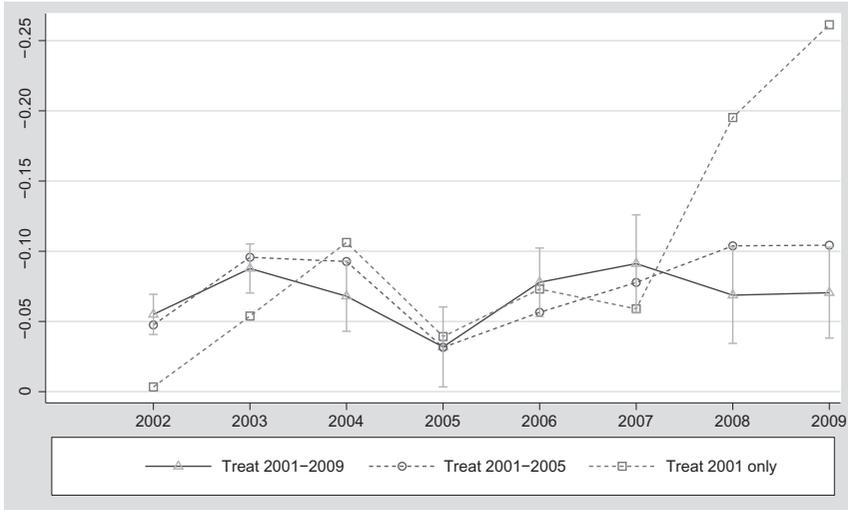


Fig. 7. Year-by-Year ATT for Informal Employees. *Note:* Reversed scale in y-axis. ATT obtained with a matching DID estimator for each post-reform year (except 2001 when no registration questions were asked to employees). Treatment defined based on labor income in years 2001–2009, 2001–2005, or 2001. The I-beams are one-standard-deviation confidence intervals.

received during the whole 2001–2009 period, when the period is reduced to 2001–2005, and when only income in 2001 is considered.

Several points are worthy of note. First, the M-DID estimates of the impact of the reform using the baseline treatment definition are substantially higher in absolute value. The effect on irregular activities is estimated to have been -5.4% in 2001 and to have increased thereafter. By 2009, individuals affected by the reform were 16.6% less likely to perform informal irregular activities. For employees, the reform is estimated to have decreased informality by 5.5% in 2002. In this case, estimates do not trend upwards but neither do they wither away in time.

Second, I check whether the time pattern of the effects is an artifact of the treatment definition. I estimated M-DID estimates under restricted treatment definitions. Restricting treatment to years 2001–2005 does not affect results. With the exception of a pronounced dip in the effect on irregular activities in 2008, all estimates lay within the one-standard-deviation band of estimates using the whole 2001–2009 period. Further restricting treatment definition to individuals treated in 2001 leads to a

uniform downward shift in the time series of estimates for informal irregular activities. Interestingly, the time pattern of effects is not affected, suggesting that the reform had significant long run effects.

Summing up, the experiment in this subsection is interesting for three reasons. First, it shows that the tax reform led to a reduction in informality regardless of the time period considered either for defining treatment or for measuring the effect. Second, the effect of the reform is robust to a non-parametric specification of the control function and to restricting estimation to the region of common support. Finally, the time pattern of the effect was very different for informal irregular activities and informal employees.

Detailed Treatment Groups

The tax reform affected individuals with annual earnings of 50,000 rubles or more. However, the effect was heterogeneous even within this group. In particular, as Fig. 4 shows, those in relatively higher tax brackets experienced a larger reduction in marginal tax rates. It is natural to expect that the effect of the reform was stronger for them.

Following this intuition, I define four detailed treatment variables that distinguish among individuals with after-tax monthly earnings in the following intervals: 3,625–7,250, 7,250–10,875, 10,875–21,750, and 21,750+. I refer to these variables as $Treat^1$ through $Treat^4$, respectively.³⁶ Naturally some individuals fall into different brackets in different periods. I operationalize the definition so that the groups are mutually exclusive.³⁷ I then re-specify the DID equation as follows:

$$INF_{it} = \theta_t + X_{it}\beta + Z_{it}\gamma + \psi Post_t + \sum_{h=1}^4 \mu_h Treat_i^h + \sum_{h=1}^4 \alpha_h (Treat_i^h \times Post_t) + u_{it} \quad (3)$$

As above, I assume that the error term has the constant unobserved effect structure, so I estimate Eq. (3) using fixed effects. In order to save space, Table 10 only reports the coefficients of interest.³⁸

For informal employees, the estimates follow a simple pattern. The reform had the strongest effect in the highest income bracket. The effects on

Table 10. Detailed Treatment Groups: DID FE.

	Informal Employee	Informal Irregular Activities	Any Informal Employment
<i>Post</i>	0.0494 (0.099)	0.0358 (0.075)	-0.0298 (0.120)
<i>Treat</i> ¹ × <i>Post</i>	-0.0172 (0.012)	-0.0209* (0.012)	-0.0310* (0.017)
<i>Treat</i> ² × <i>Post</i>	-0.0235* (0.013)	-0.0601*** (0.013)	-0.0768*** (0.018)
<i>Treat</i> ³ × <i>Post</i>	-0.0267** (0.011)	-0.0501*** (0.012)	-0.0793*** (0.016)
<i>Treat</i> ⁴ × <i>Post</i>	-0.0388*** (0.014)	-0.0276* (0.015)	-0.0390* (0.020)
Observations	44,452	53,769	47,718
No. of Individuals	11,263	12,411	11,969
R ² overall	0.04	0.03	0.01

Note: RLMS, rounds VIII–XVIII (1998–2009). *Treat*⁴ are individuals with after-tax monthly earnings above 21,750 rubles in any post-reform period. *Treat*³ are individuals with earnings above 10,875 rubles at least once but never above 21,750. *Treat*² and *Treat*¹ are similarly defined using 7,250 and 3,625 rubles as cutoffs. The control group includes all those untreated and employed in the post-reform period. Other definitions are as in Table 2. Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form. Covariates are the same as in Table 8. All estimated coefficients have the same sign and level of significance and are available upon request.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the other groups were still negative but smaller in absolute value. Indeed, the estimate for *Treat*¹ is not significant at the conventional levels.

The pattern for informal irregular activities is nonlinear. The effect of the reform peaked among those in *Treat*² and declined thereafter. One simple explanation could be that informal irregular activities are infrequent in the highest brackets. Moreover, it could be the case that informal activities are heterogeneous and that relatively wealthy individuals only perform the most profitable among them. Thus, it would take an ever larger reduction in taxes to lure these individuals out of the informal sector.

An alternative explanation is that the reductions in the PIT and the ST had different effects on this kind of informal employment. As shown in Table 6, reform-wise the difference between *Treat*¹ and *Treat*² involved a reduction in the ST of over 15%, while the difference between treatment group 2 and groups 3–4 was mostly about the PIT.

Weighted Differences in Differences

The analysis of the detailed treatment effects suggests that the effects of the reform may be heterogeneous. It also raises the concern that the reduction in informal sector participation was endogenously determined, and not a consequence of the reform. Even though we control for observable and (to some extent) unobservable characteristics that differ across groups, it could still be the case that individuals in higher brackets are somehow different in ways we fail to take into account.

Because the reduction in tax rates occurred in discontinuous jumps at different income thresholds, it would in principle be possible to analyze the effect of the reform in a regression discontinuity framework. There are, however, not enough individuals in the RLMS to apply the RD method meaningfully. An alternative approach involves weighting observations by the distance of reported earnings from the threshold of 50,000 rubles.³⁹ Specifically, the weighted DID estimand is:

$$\sum_{i=1}^n \omega_i [INF_{it} - \theta_t - X_{it}\beta - \psi Post_t - \alpha(Treat_i \times Post_t) - u_{it}]^2 \quad (4)$$

where ω_i is the individual weight and I omit the time-constant regressors. The weights are a decreasing function of the distance of the individual's post-reform income from the threshold at 50,000 rubles. Specifically, given reported monthly income Y_{it} , the weights are calculated as $(K((Y_{it} - 3,625)/h))/(\sum_{i=1}^n K((Y_{it} - 3,625)/h))$, where $K(\cdot)$ is a Gaussian kernel and h is the optimal bandwidth.⁴⁰ I interpret the resulting weighted DID estimates as robustness check in the spirit of regression discontinuity, since individuals with incomes close to the threshold are probably relatively closer in terms of all unobservable characteristics.

Table 11 reports the estimation results for Eq. (4) with individual fixed effects. The point estimates are fairly close to those in Table 8. The number of observations goes down because of a number of individuals who are assigned zero weights, which is the intended effect of the strategy. As a consequence, including the weights almost doubles the standard errors of the treatment interaction term. These results confirm that the reform caused a reduction in the prevalence of informal irregular activities.

The estimate for informal employees is not statistically significant at the conventional levels. This could lead to the conclusion that the reform did not affect this category of informality. However, the lower levels of statistical significance affect all other regressors.⁴¹ For example, none of the

Table 11. Weighted DID with FE.

	Informal Employee	Informal Irregular Activities	Any Informal Employment
<i>Post</i>	-0.0658 (0.121)	0.0245 (0.063)	-0.1852 (0.141)
<i>Treat</i> × <i>Post</i>	-0.0178 (0.019)	-0.0329* (0.019)	-0.0546** (0.027)
Observations	41,930	50,914	45,134
R^2 overall	0.005	0.03	0.001
No. of Individuals	10,180	11,220	10,856

Note: RLMS, rounds VIII–XVIII (1998–2009). Treatment effect estimated by a weighted fixed-effects regression. Included covariates are the same as in Table 8. Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form.

* $p < 0.01$, ** $p < 0.05$, *** $p < 0.01$.

education dummies was statistically different from zero at the conventional levels. However, having a degree from a higher-education institution appears to lead to a significantly lower probability of informal employment according to all other estimates (see, e.g., Table 8). For this reason, I attribute the lack of statistical significance to the relative inaccuracy of the instrument and not the absence of a real effect.

The Extensive Margin

So far, all estimates of the effect of the reform on informality have implicitly relied on individual transitions in and out of informal employment. However, an alternative route through which the reform might have affected informality is by changing the probability of choosing a formal job for those who were unemployed before the reform and found employment in the post-reform period.

In order to estimate the effect of the reform on the extensive margin, I restrict the sample to individuals who were unemployed in the year 2000 but found employment sometime during the post-reform period.

The top panel of Table 12 reports estimation results for the baseline fixed effect specification. I interpret these estimates as the predicted change induced by the reform in the probability of informal employment, other things constant, and conditional⁴² on finding employment in the post-reform period.

Table 12. Tax Reform Effect on the Extensive Margin.

	Informal Employee	Informal Irregular Activities	Any Informal Employment
A. Baseline			
<i>Post</i>	0.2740*** (0.093)	0.4429*** (0.058)	0.5704*** (0.114)
<i>Treat</i> × <i>Post</i>	-0.0146 (0.025)	-0.1433*** (0.023)	-0.1355*** (0.027)
Observations	21,224	24,924	22,899
No. of individuals	7,339	8,080	7,709
R^2 overall	0.027	0.016	0.054
B. Robustness tests			
Including <i>District</i> × <i>Year</i> interactions	-0.0111 (0.025)	-0.1467*** (0.023)	-0.1357*** (0.028)
Control group restricted ^a	-0.0121 (0.025)	-0.1387*** (0.023)	-0.1310*** (0.027)
Treatment group expanded ^b	-0.0049 (0.025)	-0.0969*** (0.023)	-0.1001*** (0.028)
Treatment defined using income from all sources	-0.0314 (0.029)	-0.0948*** (0.026)	-0.1242*** (0.031)
Treatment defined using years 2001–2004 ^c	-0.0284 (0.029)	-0.1362*** (0.027)	-0.1044*** (0.033)
<i>Treat</i> × <i>Trend</i> ^d	-0.0007 (0.004)	-0.0212*** (0.004)	-0.0197*** (0.005)

Note: RLMS, rounds VIII–XVIII (1998–2009). Sample restricted to those unemployed just before the reform and who were employed at least once in the post-reform period. The dependent variable is set to zero in round IX. Round VIII is excluded.

^aExcludes from control group individuals whose earnings were never reported in post-reform period.

^bThe treatment group includes individuals with yearly income between 3,168 and 4,800 rubles in any post-reform round.

^cExcludes individuals who receive treatment in the late post-reform years (see main text for details).

^dIncludes a post-reform time trend (2,000 = 1) instead of the post-reform dummy. All other covariates are the same as in Table 8. Arellano (1987) robust standard errors in parentheses allow for heteroscedasticity and auto-correlation of arbitrary form.

* $p < 0.01$, ** $p < 0.05$, *** $p < 0.01$.

The tax reform significantly reduced the probability of informal irregular activities for new jobs. Specifically, individuals in the treated group were over 14% less likely to choose this form of informal employment relative to the control group. The estimate for informal

employment in the main job is negative but not statistically significant. Finally, the effect on the overall informality indicator was similar to that on irregular activities.

In the bottom panel of the table I report some robustness tests (comparable to those in Table 9). In general, the results reinforce the message that the reform had a strong effect on the extensive margin for irregular activities, while for informal employees the effect was probably not significant.

CONCLUSIONS

The modern Russian economy is notorious for the high level of uncertainty regarding regulations, the pervasiveness of corruption and tax evasion, and the relative powerlessness of the State to enforce the law. The economic and social costs of these institutional failures are probably large. Unfortunately, there are negative feedback effects that make the emergence of better institutions unlikely. Russia seems to be trapped in a low-level equilibrium of high informality and poor public goods provision by the State.

In this context, the tax reform of 2001 appears as a very important experiment. The reform reduced average tax rates for the Personal Income Tax and the Social Tax and made the tax structure more regressive. Because individuals in the lower income bracket were for the most part not affected, it is possible to estimate the effects of the reform using a DID approach.

In this paper I study the effect of the reform on individual participation in the informal sector. I find evidence that the reform led to a reduction in the fraction of informal employees. The reform seems to have had an even stronger negative effect on the prevalence of informal irregular activities. These effects – which I estimate to be in the order of 2.5% and 4.0%, respectively – are robust to a number of different specifications and small alterations in the definition of treatment status. A semi-parametric estimator gives even larger estimates of these effects. It also shows that the reform had a long-lasting impact.

I also find that, predictably, the effects of the reform were relatively stronger in the top income brackets, where the reduction in marginal tax rates was more radical. Finally, at the extensive margin, the reform made it 14% less likely that someone entering the job market in the post-reform period would perform informal irregular activities.

These results are consistent with the emerging consensus that changes to the tax system are bound to lead to strong behavioral responses, although

not necessarily in the form of a reduced labor supply. In particular, several studies have shown that income reporting and tax avoidance are sensitive to changes in marginal tax rates. The Russian flat tax reform provides strong evidence that labor market informality should be added to the list of possible margins of adjustment for individuals.

NOTES

1. In many Latin American countries, the share of informal employment exceeds 50% of the urban labor force (Gasparini & Tornarolli, 2007). Existing estimates for Sub-Saharan Africa and Asia are even higher (Jütting, Parlevliet, & Xenogiani, 2008).

2. As I explain below, these estimates should be interpreted as lower bounds. I find no evidence that the tax reform affected informal main-job-entrepreneurs or informality in the second job. See Table 8.

3. The paper also contributes to the larger literature on ‘flat tax’ reforms (e.g., Keen, Kim, & Varsano, 2008).

4. These estimates put the size of hidden economy in the order of 40% of official Russian GDP.

5. The RLMS is conducted by the Higher School of Economics and the “Demoscope” team in Russia, together with Carolina Population Center, University of North Carolina at Chapel Hill.

6. In fact, 8.5% of those considered employed only work doing irregular activities.

7. The Rosstat labor force survey counts any form of work, including barter, as employment. It also asks employment-related questions regarding a reference week, while the RLMS asks about activities during the last month.

8. The INFSUP was designed and financed by the Centre for Labour Market Studies at the Higher School of Economics in Moscow and the Labor Markets in Emerging and Transition Economies Research Program at IZA in Germany. I thank Vladimir Gimpelson and Hartmut Lehmann for generously making these data available.

9. In rare opportunities, the INFSUP was administered on a later date than the regular questionnaire, although always by the same interviewer.

10. There are numerous reviews of the literature on the informal sector and informal employment. See, for example, Peattie (1987), Swaminathan (1991), and Jütting et al. (2008).

11. This classification is based on four items of the adult questionnaire: (1) “do you work at an enterprise or organization? We mean any organization or enterprise where more than one person works, no matter if it is private or state-owned. For example, any establishment, factory, firm, collective farm, state farm, farming industry, store, army, government service, or other organization.” Enterprise workers are considered entrepreneurs if they answer positively to both (2) “Are you personally an owner or co-owner of the enterprise where you work?” and (3) “In your opinion, are you doing entrepreneurial work at this job?” The distinction between entrepreneurs and employees for non-enterprise individuals is

based on: (4) “At this job are you ... (a) involved in an employer’s or individual labor activity or (b) work for a private individual?”

12. The question is: “Tell me, please: are you employed in this job officially, in other words, by labor book, labor agreement, or contract?” This item was not included in round X (2001).

13. Russian law does not require self-employed individuals to create a corporation or special legal entity. They are instead allowed to operate under a special and simpler registration procedure. However, the obligation to sign a written contract and register employees applies to all employers without exception.

14. The registration question for the second job is identical to that in the primary job. It was also not included in round X.

15. This item is available in every round.

16. In Table 9, I show that this distinction does not affect the main results. Similarly, one could distinguish between those whose only remunerated work are irregular activities and those for whom irregular activities are supplementary. This distinction does not affect the results either.

17. For example, Lehmann and Pignatti (2007) use a similar definition for Ukraine. For a discussion of the relative merits of alternative definitions, see Swaminathan (1991) and Portes and Schauffler (1993).

18. Because I analyze informality in each of the three possible jobs separately, some individuals are counted under more than one category.

19. Specifically, after the regular earnings item for the main job, the questionnaire asked: “what percent of that money do you think was officially registered, i.e. taxes were paid?” No similar question was included for the other jobs.

20. Tables with the detailed distributions of occupation and industry for informal workers can be found in the working paper version (Slonimczyk, 2011).

21. For the second job, some questions were not included in round XVIII. I then report information from the most recent round when the item was available. See the notes at the bottom of the table for data sources.

22. This information comes from round XVII (2008).

23. The informal entrepreneur category is overwhelmingly populated (95%) by self-employed individuals, so I do not present these statistics for them. However, roughly 50% of informal employees work at enterprises.

24. The median size of informal employees’ firms is only 10 workers. The median for the whole sample is 50.

25. The proportion of informal employees who had actually been on paid vacation during the previous 12 months was only 8.6%.

26. I reserve the terms “main job” and “secondary job” to refer to the adult questionnaire-based categories.

27. The most significant concern arises for individuals who, according to the adult questionnaire, performed both a second job and irregular activities.

28. This threshold is obtained as follows: $3,625 = (50,000/12) \times (1-0.13)$.

29. The maximum possible savings are given by $(4,800-3,168) \times 0.13 \approx 212$.

30. More precisely, the tax reform seems to have led to a reduction in informal employment relative to a counterfactual estimate based on the control group. It is clear from the figure that the effect of the reform was not strong enough to revert the ascending trend of informality.

31. This is one of the recommended approaches for DID studies (Bertrand, Duflo, & Mullainathan, 2004).

32. To save space I omit all other covariates.

33. The question is: “What is the total amount of money that you received in the last 30 days? Please include everything: wages, retirement pensions, premiums, profits, material aid, incidental earnings, and other receipts, including foreign currency, but convert the currency into rubles.”

34. In practice imposing common support means that the M-DID estimator only uses information on control individuals if their observable characteristics are close to those of one or more treated individuals. In contrast, regression estimates use all control group observations.

35. Specifically, I used an average of the 10 nearest neighbors. I also experimented using a kernel matching procedure but while the results were almost identical, processing times were much longer. Therefore I stick to nearest neighbor matching. The propensity score was estimated using a logit model that included all time-constant and time-varying controls (as in the OLS regression of Table 7). Matching was done on the index rather than the probability. Estimates were obtained using Leuven and Sianesi `psmatch2` module for Stata.

36. These detailed treatment groups correspond with the following tax brackets: 50,000–100,000, 100,000–150,000, 150,000–300,000, and 300,000+ (see also Table 6).

37. See notes to Table 10 for details.

38. The full set of results is available from the author upon request.

39. This approach was first suggested in Gorodnichenko et al. (2009). See also Duncan and Sabirianova-Peter (2009).

40. $h = 0.9(\sigma/\sqrt[3]{n})$ and σ is the smaller amount between the standard deviation of reported income and the inter-quartile range.

41. For brevity complete estimation results were not included. They are available from the author upon request.

42. Duncan and Sabirianova-Peter (2009) study the effect of the reform on the extensive margin of employment in general (independently of formal status). Using the same DID methodology, they find that the expected probability of finding a job in the post-reform period was significantly higher for individuals in the treated group. The estimated effect is between 0.09 and 0.14, depending on whether they use a male or female sample. However, these estimates require extrapolating earnings for individuals not employed throughout the post-reform period in order to assign them to treatment or control. This is not necessary for investigating the effect on informality, conditional on employment.

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APPENDIX

Table A1. Summary Statistics by Treatment.

	Control	Treated	All Employed
Female	0.61	0.52	0.54
Age	42.29	37.18	38.21
Secondary education comp	0.76	0.87	0.85
College education comp	0.12	0.23	0.21
Schooling (years)	11.07	12.16	11.94
Labor market experience	20.12	16.26	17.04
Married	0.47	0.59	0.57
Urban location	0.63	0.78	0.75
Russian national	0.63	0.73	0.71
Russian born	0.92	0.92	0.92
Size household	3.32	3.54	3.50
No. of female household members	1.77	1.86	1.84
No. of youth household members	0.72	0.84	0.81
No. of elderly household members	0.29	0.18	0.20
Observations	17,404	68,475	85,879
No. of individuals	3,545	11,487	15,032

Note: RLMS, rounds VIII–XVIII (1998–2009). An individual is considered treated if her after-tax monthly labor income from all sources is above 3,625 rubles in any post-reform round. The control group comprises the untreated individuals who were employed.