I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14899

Managing Dual Practice of Health Workers: Evidence from Indonesia

Paula González
Gabriel Montes-Rojas
Sarmistha Pal

NOVEMBER 2021

DISCUSSION PAPER SERIES

IZA DP No. 14899

Managing Dual Practice of Health Workers: Evidence from Indonesia

Paula González
Universidad Pablo de Olavide

Gabriel Montes-Rojas
Universidad de Buenos Aires

Sarmistha Pal
IZA and University of Surrey

NOVEMBER 2021

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Managing Dual Practice of Health Workers: Evidence from Indonesia*

Managing dual practice of health workers has often proved to be challenging, especially in emerging countries characterized by weak monitoring and low motivation. This paper exploits an exogenous variation in the initiation of private practice among heads of local public facilities (known as puskesmas) providing primary health care after the introduction of a 1997 health regulation in Indonesia. This regulation required health professionals to apply for a license for private practice at least three years after their graduation. Exploiting the exogenous variation in private practice after the 1997 regulation, we provide estimates of causal effects of dual practice on provision of public health services, distinguishing between the effects when private practice is located at or away from the public hospital. The estimates suggest that dual practitioners (relative to those engaged in public service only) work significantly less hours per week while they see significantly more patients in their public facilities. These adverse effects of dual practice are most pronounced when private practice is held away from the puskesmas. These results have important bearings on human resource management of universal health care provision.

JEL Classification: I10, I18, J2, J44, J45, O1

Keywords: dual practice of health professionals, Indonesia, Ministry of Health regulation 916, weak monitoring, difference-in-differences

Corresponding author:
Paula González
Department of Economics
Universidad Pablo de Olavide
Seville
Spain
E-mail: pgonzalez@upo.es

* We wish to thank Menno Pradhan and Erlend Berg for the initial idea of the paper and Rand Corporation for access to the data; we also thank Universidad Pablo de Olavide, City University of London and also the University of Surrey where most of this research has been carried out. We are grateful to Shibdas Bandypadhyay, Peter Carney, Clement Imbert, Mireia Jofre-Bonet, Inés Macho-Stadler, Sandip Mitra, Dilip Mukherjee, participants at the Mid-west International Economic Development Conference in Minneapolis, NEUDC Meeting in Brown University, Health Economics Study Group Meeting in Lancaster, Royal Economic Society Meeting in Brighton and also those at the Indian Statistical Institute Kolkata for their constructive comments and feedback on earlier versions of the paper. P. González acknowledges financial support from Agencia Estatal de Investigación (Ministerio de Ciencia e Innovación), project PID2019-104452RB-I00, Fondo Europeo de Desarrollo Regional y Consejería de Transformación Económica, Industria, Conocimiento y Universidades, Junta de Andalucía, projects P18-RT-2135 and CV20-35470.

1 Introduction

With a steep increase in health care spending across the globe as medical technology continues to evolve, achieving value for money is key to the success and sustainability of any public health system. Frequent absence of public health professionals and the poor delivery of public health services have often been a concern among policy makers in many emerging economies (Chaudhury et al., 2006). The presence of weak disciple environment and growing opportunities for private practice (PP) among public health professionals (Ferrinho et al., 2004) can particularly challenge the public healthcare provision, especially when health workers are poorly motivated.

Dual practice among physicians is common all over the world. The policy of allowing public employees to work as private providers was intended to create an incentive for physicians to stay in their public posts. This could serve to reduce waiting times for treatment or lead to improvements in access to health services, especially for poor and rural inhabitants of developing countries (García-Prado and González, 2011). Data on health workforce practices in many countries confirm dual practice by doctors (Berman and Cuizon, 2004; Ferrinho et al., 2004, García-Prado and González, 2011; González, 2014), but there is little empirical evidence on its impact. Theoretical studies mainly focus on analyzing physicians' incentives as dual providers (González, 2004; Barros and Olivella, 2005; González, 2005; Biglaiser and Ma, 2007; Brekke and Sargard, 2007), but results are rather ambiguous when it comes to identifying the impact of dual practice on the efficiency and quality of public health care provision.[1] More importantly, the related empirical literature is rather disperse and descriptive in nature, although it seems to suggest that health workers engaged in dual practice are often unproductive, inefficient and corrupt (Berman and Cuizon, 2004, Ferrinho et al. 1998, 2004).[2]

This paper makes an important attempt to empirically identify the causal effect of dual practice on public health provision with a view to derive implications for managing dual practitioners and, as far as we are aware, it is the first paper to do so. We use a novel identification strategy that relies on a 1997 health regulation 916 in Indonesia requiring a license for private practice after three

[1]González (2004), Biglaiser and Ma (2007), Brekke and Sargard (2007) and González and Macho-Stadler (2013) also investigate on the optimality of allowing dual practice and explore different policy options to deal with this phenomenon.

[2]See Eggleston and Bir (2006), García-Prado and González (2011), Socha and Bech (2011) and González (2014) for reviews on dual practice.

years of experience since graduation at the public health facilities. Introduction of this regulation (which cannot be influenced by individual health workers) had exogenously affected the initiation of private practice (PP) among eligible health workers around the cut-off experience of 3 years. Thus, exploiting the exogenous variation in private practice among eligible and non-eligibles after the introduction of the 1997 regulation, we identify the local treatment effects of dual practice: in particular, we document how dual practitioners may devote less time in the public health facilities, but see more patients. We also compare the behaviour of dual practitioners practising privately at or away from the public health facilities.

Indonesia is an important case in point. The government of Indonesia is committed to the provision of quality health care and over the last decades has introduced a series of regulations to ensure quality health service provision to all Indonesians (see Appendix 2, Table A2.1). Given the relative low pay of civil servants that includes government-employed physicians and other health professionals, allowing them to practise privately was considered to be essential to recruit and retain health professionals, especially in remote rural areas.[3] As such, private practice by public health professionals remains common in Indonesia (Eggleston and Bir, 2006). Division of work hours between state and private practice is governed by general regulations for civil servants and specific local regulations. These generally require civil servants to spend 8 hours a day, six days per week, in their state employment and conduct private practice only after the close of the official public work day, (Meliala, Hort and Trisnantoro, 2013). The latter may have been facilitated by the morning only clinical service in many puskesmas in the country that runs between 8 am and 2 pm.

Our results suggest that the regulation was effective to increase (decrease) dual practice of eligible (ineligible) public health professionals, after controlling for all other factors that may also influence dual practice. Instrumental variables (IV) estimates (exploiting the 1997 regulation) of selected public health provision measures suggest that dual practice is associated with significantly higher number of patients seen per week, but lower number of hours worked per week in the puskesmas, thus implying each dual practitioner tends to spend less time per patient in a typical week. There is also some indication that dual practitioners are more likely to refer public patients

[3]Income from government salary is a relatively small proportion of total income for specialists in Indonesia. In particular, 65.6% of income for surgeons and 81.2% of obstetricians are from the private sectors, as reported by Meliala, Hort and Trisnantoro (2013).

to private practices in our sample, which may be one motivation for seeing more public patients. These adverse effects of dual practice are most pronounced when heads are doctors and also when private practice is held away from the puskesmas.[4] More precisely, when the private practice is located away from the puskesmas physicians work less hours on average (11-16 vs 5-15 in the total sample), but see proportionally less patients (33-65 patients more if PP is located away, vs 65-70 patients more in the total sample). Thus, doctors whose private practice is located away from the puskesmas spend less time per public patient on average in a typical week compared to those practising privately at the puskesmas. These results highlight the adverse effects of dual practice in weak discipline work environments when public physicians lack motivation (because, for instance, they are poorly paid in their public job) and/or face large opportunity costs of working at public facilities (because the private alternative is much more attractive). Evidently, the adverse effects are more pronounced when the private practice is located away from the public health clinic. In doing so, we establish the robustness of our results in various ways. More importantly, we rule out the possibility that our results are not driven by higher ability of dual practitioners. This is because our weighting design focuses on eligible health workers with close to three years of experience. So long as a key driver of medical efficiency is experience, sample health workers that we focus on are likely to be of comparable efficiency, thus ruling out the competing explanation that more able health workers are more likely to have private practice, treat more patients and yet have shorter visits. Results are robust to cluster wild bootstrap, impact threshold analysis of omitted confounding factors, alternative samples as well as alternative specifications.

Our results contribute to the existing literature in two ways. First, we show that the causal impact of allowing dual practice on physicians' heath provision is fairly large. Despite the voices that claim a worsening in public health services due to dual practice (Ferrinho et al., 2004), all the empirical studies that document unfavorable effects of dual practice provide anecdotal evidence and fail to account for causality. Secondly, we contribute to the policy debate about the desirability of allowing minimal private practice within public facilities (Berman and Cuizon, 2004, Jan et al., 2005; Mainiero and Woodfield, 2011). This kind of policy, that has been implemented in several countries and has been debated in others,[5] is oriented toward offering doctors incentives

[4]In over 80% cases puskesmas heads practice privately away from the puskesmas. But in about 18% cases heads are found to run private practices at the puskesmas, especially in rural areas. All puskesmas heads practice privately when the private practice is located at the puskesmas and run after the close of the puskesmas in the afternoon.

[5]In Austria, France, Germany, Ireland and Italy public doctors are encouraged to develop their private practice in

to enable their private practice from public hospitals, while specifying the maximum amount of private work that can be provided within public facilities (García-Prado and González, 2007). Our results support the implementation of this policy to some extent, since the causal effects of dual practice are worse when the private practice is located away from the puskesmas.

As indicated above, the existing literature on dual practice is rather diverse encompassing the economics of dual job holding, governance of public service delivery as well as work place culture and social motivation of workers, especially in developing countries. There is also an emerging development literature that highlights the governance issues in the provision of public health services in developing countries: health workers are often absent from work (Chaudhury et al., 2006; Muralidharan et al., 2011). Doctors often spend just a few minutes with patients, providing lower quality care, and simultaneously over- and under-treating patients (see Das, Hammer and Leonard, 2008, and Das and Hammer, 2014, for reviews). Further Das et al. (2016) had studied the patient experience in public and private clinics in rural India and had reported that dual practitioners spent more time with private patients, completed more items on the checklist, and were also more likely to offer a correct treatment in their private practices, relative to their public practices. We complement these studies and link the dual practice of public health workers to their likely absence from public health services in weak monitoring environments. Our work also links to the literature on the role of social motivation on public service delivery. Workers in the public sector care about the usefulness of their job for society (Frank and Lewis 2004). Besley and Ghatak (2005) further argued that social motivation is an important driver of efficiency in organizational behaviour. But this may break down in weak monitoring environments.

Dual practice of health professionals undoubtedly poses a huge challenge to attain efficiency and quality of public health care provision, especially at the primary healthcare level. Achieving efficiency and quality of healthcare is a policy priority in most countries, as the share of healthcare costs out of GDP is increasing rapidly. These results from Indonesia therefore have important implications for an efficient management of public health professionals in other emerging economies characterized by weak monitoring and low motivation of health workers as well.

The paper is organized as follows. Section 2 describes the data and explains the identification strategy and the econometric model. Section 3 presents and analyzes the results. Finally, Section

public facilities. In other countries, like Portugal or Spain, some experiences have been implemented at the hospital level (Barros and Olivella, 2001; Camps-Cervera et al., 2006).

4 concludes.

2 Data, Identification and empirical strategy

2.1 Data

We use 1993, 1997, 2000 and 2007 rounds of the Indonesian Family Life Survey (IFLS), available for over 300 communities in Indonesia drawn from 13 provinces. This yields a puskesmas-level data over 1993-2007. The number of puskesmas per round is about 900 in our sample, but varies somewhat across the years. We use information collected from the heads of puskesmas or his/her deputies, who could be a doctor or other type of health professionals e.g., nurses, paramedics. These heads deliver both clinical and other administrative duties at the puskesmas. As such, our data is not a panel at the level of the heads of puskesmas, but pooled individual cross-sections at the puskesmas-level over 1993-2007. Since the personal information including their private practice (PP) and public service provision are only available for the heads of puskesmas, our analysis focuses on the dual practice of the heads of the puskesmas.

Table 1 summarizes the descriptive statistics for the selected variables for the full sample of all heads and separately for pre- and post-1997 subsamples. The bottom panel shows the same for doctor heads. Considering the top panel of all heads, about 75% of puskesmas heads practise privately though the proportion declined little in the post-1997 years, especially after 2000. On average, heads work 20 hours a week seeing about 84 patients and they spend about 15 minutes per patient. The likelihood of private practice is however slightly higher (78% as opposed to 75%) among doctor heads and they work less hours though see more patients on an average relative to all heads (see panel a above). Note, however, that hours worked per week had declined somewhat while number of patients seen per week increased in the post-1997 years. Second, average experience defined by the number of years since graduation of health workers was about 9.6 years before 1997, but rose to an average of approximately 13 years after 1997, thus indicating that the pool of heads were more experienced after the 1997 regulation.

[INSERT TABLE 1 ABOUT HERE]

Our research question is to investigate the effects of dual practice on public provision and, more specifically, on the effort that dual practitioners are putting at their public post. In particular, we are interested in testing whether public provision of care could be compromised if doctors are dual providers, as suggested in the literature. For that purpose, we consider two outcome variables relating to our hypotheses of interest. Our first outcome variable is the number of hours worked per week (hours_wk) by the puskesmas head. Second, we consider the number of patients seen per week (patients_wk) by the puskesmas head. Given the presence of some outliers in hours_wk and patients_wk, we winsorise these variables at 10% level. We provide estimates for both winsorised and non-winsorised outcomes later.

2.2 The 1997 Regulation and the Identification Strategy

As indicated above, the key to our identification strategy is the 1997 Ministry of Health regulation 916. The major change introduced by the 1997 regulation is that it necessitated health practitioners to get a license for practising privately and they can only get this licence after 3 years of compulsory service at puskesmas. The licensing requirement for PP was absent before the introduction of the 1997 regulation when health workers could practise privately anytime during the three years of compulsory service initiated after graduation. One important issue is that the regulation did not apply to those who were already graduated by 1997, and therefore it should not have affected puskesmas heads in 1997.

Key to our analysis is the variable Exp, experience, that refers to the number of years since graduation. The regulation should have affected puskesmas heads with $Exp < 3$, as those were the ones who previously had no restrictions but were now affected. We define the treatment group as those with $Exp < 3$ (i.e. $Exp = 1, 2$) and the control group as those with $Exp \geq 3$.

Table 2 provides summary statistics by the main categories defined in the next subsection, pre- and post 1997 regulation.

[INSERT TABLE 2 ABOUT HERE]

Heads of a puskesmas may be doctors (about 60%) or others including nurses, paramedics (40%). Non-doctor heads are more common in rural areas - 56% of rural (as against only 30% of urban) heads are non-doctors.

Heads of puskesmas are found to practise privately at the puskesmas (18%) or away from the puskesmas (82%). While all heads work privately when the private practice is located at the puskesmas which also includes some with experience less than 3 years, only some heads practice privately when the private clinic is located away from the puskesmas. The likelihood of private practice among this group of dual practitioners came down from 70% to 67% after the 1997 regulation. Given that there is a large heterogeneity in non-doctors (e.g., nurse, paramedics etc.), our baseline estimates pertain to the subsample of doctor heads. We also show the estimates of the full sample for comparison with the sample of doctor heads.

Figure 1(a) shows the mean value of PP (i.e. the proportion of puskesmas heads practising private practice) by each level of Exp for all puskesmas heads. It compares the value pre-1997 (1993 and 1997) and post-1997 (2000 and 2007) regulation. For clarity of exposition this exercise was implemented for individuals with $Exp \leq 10$ only. Figure 1(b) reports these statistics for the subsample of heads being non-doctors while Figure 1(c) shows those for doctor heads. The height of each bar in each panel represents the percentage of heads at a given level of experience practising privately.

[INSERT FIGURE 1 ABOUT HERE]

Overall, these figures show that there is a big drop in the proportion of heads doing private practice in the post-regulation period when $Exp < 3$only. The regulation is fully effective for non-doctor heads, none of whom with $Exp < 3$ practise privately after 1997. It seems from Figures 1(a) and 1(c) that although lower than pre-regulation years, some heads with less than 3 years of experience were still practising privately after the 1997 regulation and this happened solely among doctor heads. These heads are labelled as defiers using the usual causal inference terminology. This, however, pertains to less than 10% of all sample heads after 1997, who tend to practise privately at the puskesmas and mostly in rural areas. Chomitz et al. (1997) reported that the Ministry of Health was supposed to assign doctors to puskesmas randomly, but exempted those going to remote rural areas, where it was needed the most. This is confirmed in our data sample that some doctor heads with $Exp < 3$ continued to practise privately in rural areas even after the 1997 regulation because of dearth of supply of trained doctors and private practices in rural areas largely took place at the puskesmas.

In the context of dual practice in Indonesia, it is also important to explore if the heads of the puskesmas face weak monitoring. By virtue of their position, puskesmas heads are in charge of running the puskesmas and hence are unlikely to be directly monitored at their work place. Further, the 2000 and 2007 IFLS data provide information on whether the heads of puskesmas had authority to hire and fire staff (note that this information is not available for the 1993 and 1997 rounds of the survey). We find that a significantly higher proportion of dual practitioner heads (33% as opposed to 29% of those without PP) have the authority to hire and fire subordinate staff at the puskesmas and the mean difference is statistically significant. The latter may make it easier for these heads to use their subordinate staff to cover their responsibility at the puskesmas during periods of absence on account of private practice. We can explore two further ways a puskesmas head can facilitate their dual practice. First, the average tenure (i.e., number of years spent in the current puskesmas) of the head is higher in the post-1997 years, especially for the dual practitioners (see Table 1). Longer tenure in the same puskesmas may help the heads to establish their influence and authority over their subordinate staff. The second factor that may facilitate heads' dual practice is the availability of the support staff who can cover the head's responsibility during his/her absence from the facility. We find that the number of supporting doctors and nurses is significantly higher in puskesmas where the head is practicing privately (relative to those where the head is not doing so) and this holds both before and after 1997.

2.3 Econometric model

The basic empirical model of healthcare provision that we want to estimate is as follows:

$$y_{it} = \alpha_0 + \alpha_1 PP_{it} + \alpha_2 X_{it} + u_{it}, \tag{1}$$

where i refers to the head of the puskesmas and t to year. We have four independent cross-sections (not panel data at the level of the puskesmas heads) for 1993, 1997, 2000 and 2007. y refers to selected outcome variables, namely, hours worked per week ($hours\_wk$) and number of patients seen per week ($patients\_wk$) in the puskesmas. PP is a dummy variable indicating if a public health professional, i.e., the head of the puskesmas i holds a PP in year t.

The set of variables X contains the key explanatory variables including individual characteristics of the health worker that may also influence y. There are about 500 different dialects spoken in Indonesia's multicultural society and, as such, the knowledge of local language is an essential quality of a physician to be able to converse with his/her patients, which is an essential prerequisite for practicing privately in the community. So we construct a dummy variable ($llang$) that takes a value of 1 if the head knows the local language and 0 otherwise. Inclusion of this binary variable *llang* would thus allow us to account for the differential effect of the physician's knowledge of local language, if any, on public healthcare provision. We also include a binary variable *headdoc* to indicate if the head is a doctor (surely the dummy drops when we consider the cases of doctor heads) and we do this to account for the differential behaviour, if any, of doctors from other health professionals who may also head a puskesmas. The set of control variables X also includes a dummy variable for urban puskesmas (*urban*), a dummy variable for access to cemented road (*proad*), a dummy variable for whether the puskesma only works in the morning, and a dummy variable for whether there are more than 1 doctor or nurse in the puskesma ($doctors\&nurses > 1$). There are pronounced rural-urban differences in the placement of health professionals and also their private practice (see Chomitz et al., 1997). As such, the urban dummy *urban* would account for the differential effect of urban (relative to rural) regions on the outcome variables, if any. Finally we include a set of dummy variables generated by the interaction of district (Kaputapen) and year. We would argue that these district-level time trends may influence our estimates. This is because after the decentralisation at the turn of the millennium, the centre of administrative power moved from Jakarta to the district head quarters and hence district authorities (as opposed to the federal government) assumed power to regulate the puskesmas in the districts.

Note however that the key explanatory variable PP is likely to be endogenous: dual practice affects public health provision of the puskesmas heads, but the latter may also influence their dual practice. As indicated above, we make use of the 1997 Ministry of Health regulation 916 to study the effect on dual practice of heads. This allows us to exploit the exogenous variation in PP likelihood between eligible and ineligible puskesmas heads before and after the introduction of the regulation using a difference-in-differences (d-in-d) strategy as explained below. In particular, define

$$Exp3 = 1[Exp \geq 3]$$

as a dummy variable that takes a value of 1 if the individual has at least 3 years since graduation and 0 otherwise. The regulation should have only affected those that do not satisfy the criterion ($Exp3 = 0$) for applying for a license for private practice vis-à-vis those that were not affected ($Exp3 = 1$). This consideration yields a d-in-d framework for determining the private practice likelihood:

$$PP_{it} = \beta_0 + \beta_1 Exp3_{it} + \beta_2 Post97_{it} + \beta_3(Exp3_{it} \times Post97_{it}) + \beta_4 X_{it} + \upsilon_{it}, \tag{2}$$

where $Post97$ is a binary variable that identifies pre- and post-1997 reform observations, and it takes values 0 for years 1993 and 1997, and 1 for 2000 and 2007.

We use the same control variables X as in eq. (1) above; both first stage and second stage regressions include $Exp3$ and $Post97_{it}$. If the regulation had an empirical impact, then we should expect $\beta_3 > 0$. As such we compare the public health provision indices of eligible puskesmas heads (relative to non-eligible ones) after 1997 (relative to before 1997). In turn, this d-in-d model corresponds to a first-stage where $Exp3_{it} \times Post97_{it}$ is used as an IV for PP. Because the 1997 regulation was beyond the influence of the individual heads, we take this IV to be exogenously given in our analysis.

One important issue is that the d-in-d instrument may be weak to evaluate its impact on the full sample. The IV is expected to have an impact when considering puskesmas heads with Exp close to $Exp = 3$, but a weak effect when considering the entire distribution of Exp. As noted in the empirical results, the first-stage effects are larger when we weight observations more when they are closer to the $Exp = 3$ cut-off. In turn, this means that we are identifying a *local average treatment effect*, that is valid for a particular set of individuals, those who were indeed affected by the regulation. The advantage of this empirical strategy is that we are focusing on heads with comparable experience and therefore earnings too. The latter in turn helps us to eliminate competing explanation that more efficient heads can see more patients in shorter time. We attribute the observed effects on hours spent per week ($hours\_wk$) or patients seen per week ($patients\_wk$) solely to dual practice of doctors. This plays an important role in the second-stage results too and for the analysis of weak IV, where the first-stage effects statistical significance greatly increase when weights are used.

Following Fajnzylber et al. (2011) we therefore adopt a weighted least-squares (WLS) d-in-d design where larger weights are attached to observations closer to the proposed cut-off based on Exp to exploit the local nature of the d-in-d IV. In particular, for $Exp3$, this WLS method consider a Gaussian kernel where the center is located at $(Exp - 2.5)$. This is because $Exp = 2.5$ is just in between those affected by the regulation ($Exp \leq 2$) and those that were not affected by it $Exp \geq 3$. To implement this method we consider three different bandwidths, corresponding to values $\sigma = 1, 2, 3$, which are the standard deviations in the Gaussian kernel indexed $Exp - 2.5$. These correspond to Weight1, Weight2 and Weight3 in the empirical results, respectively, that we report below.

3 Results

3.1 First-stage results - Effect of the 1997 regulation on private practice likelihood

In order to explore the validity of our identification strategy we estimate eq. (2) for different specifications. The main first-stage d-in-d estimates with $Exp3 \times Post97$ are reported in Table 3. These report the interaction coefficient β_3 controlling for joint Kabutepen district and year fixed effects without and with additional control variables. Upper panel of Table 3 shows the baseline results pertaining to heads who are doctors. For comparison, we also show the corresponding estimates for all heads in the bottom panel of Table 3. For each case we first show the unweighted estimates (column (1)) followed by weighted estimates in columns (2)-(4) respectively using Weight1, Weight2 and Weight3. We show estimates with (bottom panel) and without (top panel) additional controls.

[INSERT TABLE 3 ABOUT HERE]

The results show that the regulation produced an increase PP of about 19% for unweighted estimates of doctors (with/without other controls), and this is statistically significant at 10% level. The corresponding unweighted estimates for all heads (see bottom panel) indicate between 14%-18% increase in the likelihood of PP and these are weakly significant at 10% level.

Evidently, the effect of the regulation on PP likelihood is stronger when we consider the weighted estimates summarised in columns (2)-(4) of Table 3 using Weight1, Weight2 and Weight3, respectively. In case of these WLS estimates, the size of the effects are larger (of about 30% increase) and their statistical significance varies from 5% to 1%. We then compare these estimates with those for all heads (see bottom panel). In this case the average size of the effect of the regulation is around 27% and they are all significant at 5% level. Overall, the effects of the regulation appear stronger for the subsample of puskesmas heads being a doctor and for the weighted sample closer to the level of cut-off value in between $Exp = 2$ and $Exp = 3$, the group which became affected by the regulation.

Next, in Table 4 we consider different placebo tests of the validity of the d-in-d estimates discussed above. As before we compare the case of doctor heads with all heads. We implement similar estimation strategies as before but using either $Exp4 = 1[Exp \geq 4]$ and $Exp5 = 1[Exp \geq 5]$ as alternative experience cut-off levels, and for the pre-treatment (1993 and 1997) and post-treatment (2000 and 2007) subsamples using $Exp3$ as before. For the alternative cut-off values of experience all regression coefficient estimates appear as statistically insignificant. For the 1993 and 1997 only subsample, there is weak negative effect only for the unweighted estimates in column (1), which is of the opposite sign of that in Table 3. The interaction coefficients though still negative, remain statistically insignificant for the weighted estimates in columns (2)-(4) irrespective of whether we include other controls or not. Table 4A shows the corresponding estimates for all sample heads generally confirms this for $Exp4$ and $Exp5$. As before for the 1993 and 1997 only subsample, we find a weak negative effect while for the 2000 and 2007 subsample only there is a weak positive effect for some. We therefore can establish the pre-treatment parallel trend hypothesis only for the weighted estimates that focus on the observations close to the cut of $Exp = 3$. For the post-treatment sample (2000 and 2007) almost all of the regression coefficients are statistically insignificant except for some positive values significant at 10%.

[INSERT TABLE 4 ABOUT HERE]

[INSERT TABLE 4A ABOUT HERE]

Overall, these first stage results indicate that there is strong support for the identification strategy, especially for the sample of doctor heads. In particular, the implementation of the 1997 regulation 916 seems to have produced a local effect on those that did not satisfy the eligibility criteria ($Exp < 3$) vis-à-vis those that did satisfy it ($Exp \geq 3$). This effect is not observed for the 1993 and 1997 pooled samples nor for the 2000 and 2007 ones, suggesting that the effect is specific to the implementation of the regulation (comparing before and after the regulation) and not of this particular level of Exp. Moreover, only for Exp at about 3 years there seems to be an effect on PP, with and without other controls. We therefore conclude that this effect is specific of the change in regulation, and it is therefore exogenous to the unobserved characteristics of the puskesmas heads, who could not influence the regulation.

3.2 Second-stage results - Effect of Dual Practice on Selected Public Health Provision Measures

At the second-stage, we evaluate the effect of dual practice on outcomes of public health provision, namely, hours worked per week ($hours\_wk$) and number of patients seen in a week ($n\_patients\_wk$) by the puskesmas heads. To this end, we use the first-stage procedure explained above to instrument the private practice (PP) dummy by ($exp5 \times post97$).

Ordinary least squares (OLS) regression results (see Table A1 in the Appendix) indicate biased private practice effects. In general, most estimated coefficient estimates of PP are negative for both outcome variables, namely, hours worked or patients seen per week by the heads of puskesmas in our sample. We argue that both the size and significance of these OLS non-IV estimates suffer from the simultaneity bias between the likelihood of private practice and the resultant public health provision indices by the dual practitioner, thus justifying the use of 2SLS estimates that we discuss below.

3.2.1 Hours worked per week

Table 5 shows the second-stage IV estimates of doctor heads for eq. (1), where the outcome of interest is $hours\_wk$, i.e., hours worked per week for doctor heads. Given the presence of outliers, we also considered the winsorized variable at 10% level. The Table shows the estimates for both the winsorized and non-winsorized hours per week variables with and without controls. For each set of estimates we show the unweighted (column (1)) as well as weighted (columns (2)-(4)) estimates respectively using Weight1, Weight2, Weight3. The corresponding estimates for all heads are summarised in Table 5A.

[INSERT TABLE 5 ABOUT HERE]

[INSERT TABLE 5A ABOUT HERE]

Focusing on the doctor heads, we find that the estimated coefficient of PP IV is negative in all columns, but statistically significant only for the weighted cases. We get similar results irrespective of whether we winsorize or not and also irrespective of the weight chosen. On average, a dual practitioner doctor works less than a non-dual practitioner doctor by 5-15 hours a week depending on the weights. As expected, the size of the private practice effect is slightly larger for the non-winsorized hours per week variable: it varies between 10-20 hours a week. These effects are statistically significant at 1%-10% level. Given the smaller than 50 clusters at our disposal, we also test the validity of these reported clustered standard errors using wild cluster bootstraps, which provides asymptotic refinement. These asymptotic wild cluster bootstrapped standard errors still produce t-statistics which are statistically significant at 1%-10% levels in this sample.

We use the Cragg-Donald test statistics for testing the weak identification in our sample. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10 percent, 25 percent, etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10 percent maximal IV size is 16.38 and that for 15 percent maximal IV size is 8.96 for our model. The Cragg-Donald test statistic for all the weighted cases are larger than 16.38. We can therefore reject the null hypothesis that the maximum bias due to weak instruments is 10 percent for the weighted estimates. Taken together, Cragg-Donald test statistics support the strength of our IV for the weighted estimates.

Table 5A shows the corresponding estimates for all heads in our sample. While most estimated coefficients of PP IV is negative and the size of the effects is significantly less compared to doctors, but none of them are statistically significant. The latter may simply reflect the heterogeneity of non-doctor heads in our sample, which may blur the effects observed for doctors.

3.2.2 Patients seen per week

Table 6 shows the second-stage IV estimates for eq. (1), where the outcome of interest is $patients\_wk$, i.e., patients seen per week among doctor heads. Given the presence of outliers, we also winsorize the variable at 10% level. The Table shows the estimates for both the winsorized and non-winsorized patients per week with and without controls. For each set of estimates we show the unweighted (column (1)) as well as weighted (columns (2)-(4)) estimates for all heads and also when the head is a doctor.

As before, the unweighted estimates (winsorised or not) of patients seen per week do not work very well. We, however, obtain consistent estimates when considering the weighted estimates, especially when using the winsorised outcome variable. The weighted estimates are generally positive and significant with and without other controls. On average, dual practitioner sees about 65-70 more patients per week, after controlling for all other factors. Although the size of the effect seems large at first sight, given a 6 days a week, it comes to about 11-12 extra patients on average per dual practitioner per day, which seems feasible. As before, significance of the IV is retained even when we use wild cluster bootstrap method to provide asymptotic refinement of the clustered standard errors.

As with hours worked per week, the Cragg-Donald tests are statistically significant for the weighted estimates. We can therefore reject the null hypothesis that the maximum bias due to weak instruments is 10 percent for most weighted estimates. Test statistic in column (4) indicates that we can reject the null hypothesis that the maximum bias due to weak instruments is 15 percent. This supports the strength of the IV used for the weighted estimates.

We also compare the estimates for doctor heads with those for all heads, as summarised in Table 6A. In this case, we get positive and statistically significant effects for weighted estimates with controls while using winsorised variables. However the estimated coefficients of PP IV tend to be negative for the non-winsorised variables, which may reflect the outlier issue in the sample. As with Table 6, Cragg-Donald test statistics are large enough for the weighted estimates to highlight the strength of our IV.

[INSERT TABLE 6 ABOUT HERE]

[INSERT TABLE 6A ABOUT HERE]

3.2.3 Tests for omitted confounding factors

Regression coefficients cannot be interpreted as causal if the relationship can be attributed to an alternate mechanism. One may control for the alternate cause through an experiment (e.g., with random assignment to treatment and control) or by measuring a corresponding confounding variable and including it in the model. Unfortunately, there are some circumstances under which it is not possible to measure or control for the potentially confounding variable which are unobservable. In this case, it is helpful to assess the robustness of a statistical inference to the inclusion of a potentially confounding variable. Frank (2000) and Frank, Maroulis, Duong, and Kelcey (2013) had proposed a method for testing the robustness of causal inference by identifying the impact threshold of a confounding variable or ITCV in short.

We ran the ITCV analysis for the key explanatory variable, namely, $Exp3 \times Post97$, which could be potentially confounding especially if the treatment is driven by some unobserved factors that may cause a spurious association between the outcome and the IV. For example, the IV may be influenced by the unobserved ability of the puskesmas head. All our regressions include district fixed effects that account for time-invariant unobserved district level factors. But this may fail to account for the time-varying unobserved characteristic of the puskesmas heads, which could bias the estimates of our outcomes, namely, hours spent per week or patients seen per week by these heads. To eliminate the possibility, we obtain the impact threshold of a confounding variable for our baseline estimates of doctor heads.

The threshold defines the point at which evidence from a study would make one indifferent to the adoption of the reform. If the evidence were more in favour of a reform or regulation, one would choose it; if the evidence were less, one would not choose it. As such, the threshold represents the effect size where the benefits of the adoption of the regulation outweigh its costs. The more the estimate exceeds the threshold, the more robust the inference is with respect to that threshold. The ITCV analysis thus enables us to determine how strong the effect of a hypothetical confounding variable would have to be to overturn current inferences. Accordingly, we calculate the percentage of observations that have to be biased in order to invalidate our key inference pertaining to the IV at the second stage.

For the subsample of doctor heads, we reported the coefficient estimates and the standard errors of the estimates of hours worked per week in Table 5. Focusing on the winsorised hours per week regression with controls, we obtain the following impact thresholds depending on the weights (we do not consider the unweighted estimates because the IV remained insignificant in these cases): (i) the estimated effect is insignificant for Weight 1 and hence the ITCV is not pertinent. (ii) For weight=Weight 2: to invalidate the inference 39.04% (648) cases would have to be replaced with cases for which there is an effect of 0. (iii) For weight=Weight 3, to invalidate the inference 36.19% (609) cases would have to be replaced with cases for which there is an effect of 0. The corresponding estimates of patients seen per week are shown in Table 6; focusing on the estimates of winsorised outcome, we obtain the following impact thresholds: (i) For weight=Weight1, to invalidate the inference 25.99% (257) cases would have to be replaced with cases for which there is an effect of 0. (ii) For weight=Weight2, to invalidate the inference 9.95% (158) cases would have to be replaced with cases for which there is an effect of 0. (iii) For weight=Weight3, the coefficient estimate is statistically insignificant; to sustain the inference, 29.87% of the cases with 0 effect would have to be replaced with cases at the threshold of inference. Since the ITCV estimate exceeds the threshold by a significant margin, it validates the robustness of our current inference.

3.3 Case of private practice being away from the puskesmas

Note that about 18 percent private practice in our sample is located in the puskesmas while the majority of private practice takes place away from the puskesmas. Placement of a private practice at the puskesmas is not at the discretion of the puskesmas head. According to the 2007 IFLS data, only 22.37% puskesmas had put forward their suggestions to the district health unit about service provision. But almost all (87%) decisions were made by the district health unit and district planning board and in about 13% cases the central health ministry was also involved. As such, puskesmas heads are unlikely to influence the location of the private practice at the puskesmas so that we can treat it to be exogenously given.

[INSERT TABLE 7 ABOUT HERE]

Table 7 compares means of selected characteristics between these two types of private practices in our sample. It highlights that the private practice located at the puskesmas behave differently from the one located away from the puskesmas. In particular, all heads practise privately when the private practice is located at the puskesmas, while only about 70% heads practises privately if it is located away from the puskesmas. Heads tend to work significantly more (3.4 hours per week), and see significantly less patients (nearly 22 less) if the private practice is located at the puskesmas, which suggests that the time spent per patient is high if private practice is located at the puskesmas. Possibly this kind of policy has the advantage of facilitating supervision, reducing opportunistic behaviour, and easing the enforcement of restrictions (as shown in Mainiero and Woodfield, 2008 for resident moonlighting in radiology). The latter may disappear when the private practice is located away from the puskesmas when a dual practitioner may have more freedom to lure away puskesmas patients to private clinics without being compromised.

[INSERT TABLE 8 ABOUT HERE]

Table 8 shows the first stage estimates of private practice likelihood when it is located away from the puskesmas. Given the small sample, we were unable to obtain the corresponding estimates when private practice is located at the puskesmas and hence we focus on the estimaets when the private practice is located away from the puskesmas. As before, there is confirmation of the validity of our IV $Exp3 \times Post97$ to determine the likelihood of private practice in this subsample. We can, therefore, proceed to the second stage.

[INSERT TABLE 9 ABOUT HERE]

Table 9 summarises the second stage estimates for (winsorised) hours worked per week (panel a) and patients seen per week (panel b) for dual practitioners practising privately away from the puskesmas. Evidently, the private practice effect is now negative and statistically significant for determining hours per week for both doctor heads and all heads in our sample, irrespective of the choice of weights. In particular, the size of the private practice effect varies between 11-16 hours a week and these effects are all statistically significant at least at 1% level. Moving on to winsorised patients seen per week, the private practice effects are positive and statistically significant when using Weight1 and Weight2; the size of the effect varies between 33-65 patients per week, i.e., between 5 to 11 additional patients per day. This may seem large, but may be a reflection of the relatively small sample at our disposal; however, these results are robust as their significance holds even after doing wild cluster bootstrapping. Cragg-Donald test statistics are all larger than their critical values, thus allowing us to reject the null hypothesis for all weighted cases; these, in turn, highlight the strength of our IV. Overall, these estimates confirm that the private practice effects are worse when it is located away from the puskesmas.

3.4 Estimates of log-linear models

In order to test the robustness our linear models, we also consider the estimates of the natural logarithms of the outcome variables for the second stage estimates. In this case the estimated coefficient of PP IV can be interpreted as the percentage change in hours worked per week or that in patients seen per week for a dual practitioner (relative to a non-dual practitioner). These estimates are shown in Appendix tables A2.4-A2.5 respectively for doctor heads and all heads. Both sets of estimates confirm the validity of the linear estimates shown earlier. In particular, the private practice effect is negative for log hours worked per week and positive for log patients seen per week. As before the private practice effects are more pronounced for doctor heads than all heads.

Tables A2.6 and A2.7 then show the estimates for doctor heads and all heads when the private practice is located away from the puskesmas. Evidently, we obtain stronger effects than the full sample and this happens for both doctor heads and all heads in this case. As before, these estimates highlight the worse private practice effects when the private practice is located away from the puskesmas.

3.5 Discussion

Taken together, results from Table 5 and Table 6 show a negative causal effect of private practice on hours worked per week in the puskesmas, and positive effect on number of patients seen per week. Our result that dual physicians work fewer hours in the public sector is not surprising. There are several papers in the literature that have warned that many public doctors work fewer hours than contracted in order to attend their private offices. Ensor and Duran-Moreno (2002) show that U.K. consultants spend time in private clinics that they should be devoting to their public duties. Mossialos, Allin, and Davaki (2005) suggest that the majority of doctors working at primary care centers run by IKA (one of Greece's largest social security organizations, covering the majority of the working population) work fewer than their contracted hours. Absenteeism in public facilities is even more common in developing countries (Chaudhury et al. 2006). However, as far as we know, none of these works have identified the causal impact of dual practice as we do.

We also consider the link between number of public patients seen by the dual practitioner and the likelihood of referring public patients to private practice from the puskesmas concerned as a measure of motivation of dual providers. IFLS data ask heads of puskesmas if they refer public patients to (a) other puskesmas; (b) hospitals; (c) private practice. We use this information to compare the likelihood of referring a puskesmas patient to private practice among dual practitioners and others. A simple t-test of mean comparisons suggests that the likelihood of private referral of puskesmas patients to private practice is about 7% for a puskesmas run by a dual practitioner head; the corresponding mean drops to 5% if the head of the puskesmas is not a dual practitioner and the mean difference is statistically significant too (t-statistic for mean comparison is 2.1636). These mean comparisons reflect one possible reason as to why dual practitioners may be more interested to see more public patients, even when they work less per week. Further, we find some difference in these mean comparisons for doctor and non-doctor heads among dual practitioners. In particular, the likelihood of referral of public patients to private practice is higher among dual practitioner non-doctor (9%) than doctor (6%) heads in our sample. As argued earlier, this referral practice is strengthened by their positions as the heads of puskesmas that allow them to use subordinate staff to cover their absence from the public jobs.

The finding of a higher likelihood of dual practitioners to refer public patients to private clinics lends support to the theoretical predictions that show that dual providers can be more motivated to use their public jobs to facilitate their private (rather than public) practice (González, 2004; Barros and Olivella, 2005; González, 2005; Biglaiser and Ma, 2007; Brekke and Sorgard, 2007). If one agrees that physicians might use their public performance to refer some public patients to their private practice, as our data shows, then they may be interested in giving consultation to a large number of patients to lure them and raise the amount of patients that end up being treated privately. This mechanism is present in the theoretical papers by González (2004), where physicians have an incentive to provide excessive effort in the public sector to raise their prestige and create additional demand.

Finally, there may be other unobserved factors that correlate with both the instrument and the outcomes. For example, ability of health workers may be an obvious unobserved factor, which could be consistent with our results that more able health workers are more likely to have private practice, treat more patients, and have shorter visits. We can rule out this competing explanation that our results are not driven by higher ability of dual practitioners. One obvious defence in support of our results is that our weighting design focuses on eligible health workers with close to three years of experience. So long as a key driver of medical efficiency is experience, sample health workers that we focus on are of comparable efficiency, thus ruling out the possibility that our central result is an artifact of greater efficiency of dual practitioners in our sample.

One possible limitation of our study is that it relies on assuming that the change in the regulation is the only cause of the change in the distribution of private practice likelihood on a close interval around the threshold level of experience $Exp = 3$. We, however, minimise this bias by controlling for a set of observed and unobserved (time-invariant and time-varying) characteristics. There may still remain some bias related to the unobserved characteristics of the puskesmas heads that we could not address because of data shortcomings; we have a panel at the level of the puskesmas, but not at the level of the heads of puskesmas.

Finally, the results obtained this way are only valid in a local sense for observations with values of experience close to the observed cut-off where the identification is valid.

4 Conclusion

The present paper examines the effects of dual practice on public healthcare provisions in a weak monitoring environment, using the hours worked per week and the number of patients seen per week by heads of puskesmas in Indonesia as proxies for health services provision by public professionals.

Our key results support that dual practice is likely to be associated with a significant reduction in the number of hours of work per week in the public facility; but dual practitioners might end up seeing more patients during the week, ceteris paribus. This implies that time spent per patient at the puskesmas is reduced and the quality of the public services could be compromised for dual practitioners. Finally, the adverse effects of dual practice are most pronounced when heads are doctors and also when private practice is held away from the puskesmas. These results are robust to the choice of weights. These findings are consistent with the theoretical predictions in Biglaiser and Ma (2007) and González (2005) where dual practitioners have incentives to shirk in their public posts and divert patients to private facilities.

Over the last years, universal health coverage (UHC) has become an agreed goal of global health policy and planning initiatives.[6] While the importance of human resources in UHC has been recognized, this cannot be done objectively without a good understanding of the extent and impact of health workers' dual practice that remains largely unexplored in the literature. Especially in low- and middle-income countries, the achievement of UHC may be hampered by unregulated dual practice as the private sector grows rapidly and costs of UHC grows rapidly. Failure to understand why, how, and to what extent public health workers engage in dual practice may compromise attempts to regulate it and undermine progress towards UHC. As far as we are aware, ours is the first study to identify the causal impact of dual practice on public health provisions by doctors and non-doctors in Indonesia using a novel identification strategy. While this is a study of Indonesia, we believe our results have important implications for the management of public health provisions in other emerging economies characterized by weak monitoring and low motivation of health workers (Chaudhury et al. 2006). Certainly, in order to evaluate the performance of countries in regard to dual practice and be able to make informed country-specific recommendations, further empirical work is needed. Still, we believe that this study can enrich the discussion on human resource management of dual practice and thereby contribute to the development of UHC.

[6]https://www.who.int/bulletin/volumes/94/2/14-151894/en/

References

[1] Barros, P. and P. Olivella. 2005. Waiting lists and patients selection. *Journal of Economics & Management Strategy* 14(3): 623-646.

[2] Barros, P. and Olivella P. 2001. Managed health care: incentive theory and empirical evidence. In: Asociación Española de Economía de la Salud, editor. Coordinación e incentivos en sanidad: XXI Jornadas de Economía de la Salud. Oviedo: Asociación Española de Economía de la Salud.

[3] Berman, P., Cuizon, D., 2004. Multiple public-private job-holding of health care providers in developing countries: an exploration of theory and evidence. Department for International Development Health Systems Resource Centre (DFID HSRC).

[4] Besley, T., & Ghatak, M. 2005. Competition and incentives with motivated agents. *American Economic Review* 95, 616-636.

[5] Biglaiser G, Ma, C.A., 2007. Moonlighting: public service and private practice. *RAND Journal of Economics* 38, 1113–1133.

[6] Eggleston, K. and Bir, A. 2006. Physician dual practice. *Health Policy* 78, 157–66.

[7] Brekke, K., Sœrgard, L., 2007. Public versus private health care in a national health service. *Health Economics* 16, 579–601.

[8] Camps-Cervera, V., López-Casasnovas, G., Puyol-González, A. 2006. Private services in public health organizations. Barcelona: Fundació Victor Grifols i Lucas (in Spanish).

[9] Chaudhury, N., Hammer, J., Kremer, M., Muralidharan, K., Halsey, F., 2006. Missing in action. *Journal of Economic Perspectives* 20, 91-116.

[10] Chomitz, K., Setiadi, G., Azwar, A., Ismail, N., Widiyarti. (1997). What do doctors want? Developing incentives for doctors to serve in Indonesia. Policy Working Paper no. 1888. , Washington DC.

[11] Das, J. and J. Hammer, 2014. Quality of primary care in low-income countries: Facts and Economics. *Annual Review of Economics* 6: 525-553.

[12] Das, J., J. Hammer and K. Leonard, 2008. The Quality of Medical Advice in Low-Income Countries. *Journal of Economic Perspectives* 22(2): 93-114.

[13] Das, J., A. Holla, A. Mohpal and K. Muralidharan, 2016. Quality and Accountability in Health Care Delivery: Audit-Study Evidence from Primary Care in India. *American Economic Review* 106(12): 3765–3799.

[14] Ensor, T. and A. Duran-Moreno. 2002. Corruption as a challenge to effective regulation in the health sector, in Regulating Entrepreneurial Behaviour in European Health Care Systems. Saltman, Busse and Mossialos eds: Open University Press.

[15] Fajnzylber, P., Maloney, W.F., Montes-Rojas, G.V., 2011. Does formality improve micro-firm performance? Evidence from the Brazilian SIMPLES program. *Journal of Development Economics* 94, 262-276.

[16] Ferrinho, P., Wim Van Lerberghe, W.V., Julien, M.R., Fresta, E., Gomes, A., Dias, F., Goncalves, A., Backstrom, B., 1998. How and why public sector doctors engage in private practice in Portuguese-speaking African countries. *Health Policy and Planning* 13, 332-338.

[17] Ferrinho, P., Van Lerberghe, W., Fronteira, I., Hipólito, F., Biscaia, A. (2004). Dual practice in the health care sector: review of evidence. *Human Resources for Health* 2.

[18] Frank, K. A. 2000. Impact of a confounding variable on a regression coefficient. Sociological Methods & Research 29, 147-194.

[19] Frank, K., S. Maroulis, M. Duong, M. Kelcey. 2013. ?What Would It Take to Change an Inference? Using Rubin's Causal Model to Interpret the Robustness of Causal Inferences?, Educational Evaluation and Policy Analysis 35 (4), 437-460.

[20] Frank, S.A., Lewis, G.B., 2004. Government employees. Working hard or hardly working? *American Review of Public Administration* 34 (1), 36–51.

[21] García-Prado, A., González, P., 2007. Policy and Regulatory Responses to Dual Practice in the Health Sector. *Health Policy* 84: 142-152.

[22] García-Prado, A., González, P., 2011. Whom do physicians work for? An Analysis of Dual Practice in the Health Sector. *Journal of Health Politics, Policy and Law* 36 (2), 265-294.

[23] González, P. 2004. Should Physicians' Dual Practice Be Limited? An Incentive Approach. *Health Economics* 13(6), 505-524.

[24] González , P. 2005. On a policy of transferring public patients to private practice. *Health Economics* 14, 513–27.

[25] González, P. and Macho-Stadler, I. 2013. A Theoretical Approach to Dual Practice Regulations in the Health Sector. *Journal of Health Economics* 32 (1), 66-87.

[26] González, P. 2014. Physicians' Simultaneous Practice in the Public and Private Sectors. In: Anthony J. Culyer (ed.), Encyclopedia of Health Economics, Vol 3. San Diego: Elsevier, 83-90.

[27] Jan, S., Bian, Y., Jumpa, M., Meng, Q., Nyazema, N., Prakongsai, P., & Mills, A. (2005). Dual job holding by public sector health professionals in highly resource-constrained settings: problem or solution? *Bulletin of the World Health Organization* 83, 771-776.

[28] Mainiero, M.B., Woodfield, C.A. (2008) Resident moonlighting in radiology. *Journal of the American College of Radiology* 5: 766-769.

[29] Meliala, A. Hort, K. and Trisnantoro, L. 2013. Addressing the unequal geographic distribution of specialist doctors in Indonesia: The role of the private sector and effectiveness of current regulations. *Social Science and Medicine* 82, 30-34.

[30] Mossialos, E., S. Allin, and K. Davaki. 2005. Analysing the Greek Health System: A Tale of Fragmentation and Inertia. *Health Economics* 14:151-168.

[31] Muralidharan, K., N. Chaudhury, J. Hammer, M. Kremer, and H. Rogers. 2011. Is there a Doctor in the House? Absent Medical Providers in India. Working paper.

[32] Socha, K., Bech, M., 2011. Physician dual practice: A review of literature. *Health Policy* 102, 1–7.

Table 1. Descriptive statistics at the puskesmas level 1993-2007

Variables	All Obs.	All Mean	All Std. Dev.	All 1993-1997 (pre-1997) Obs.	All 1993-1997 (pre-1997) Mean	All 1993-1997 (pre-1997) Std. Dev.	All 2000-2007 (post-1997) Obs.	All 2000-2007 (post-1997) Mean	All 2000-2007 (post-1997) Std. Dev.
Panel a: Full sample									
If practice privately	3411	0.74934	0.433456	1520	0.776316	0.41685	1891	0.727657	0.445283
Hours worked/wk	3234	20.45625	19.05246	1364	29.30132	12.46039	1870	14.00455	20.39464
Patients seen/wk	3038	84.21988	92.1034	1318	79.24507	77.73932	1720	88.03198	101.6049
Experience (in years)	2849	11.83959	7.260543	1380	5.39	2.87	1469	5.89	2.98
Speaks local language	3293	0.947464	0.223139	1402	0.947932	0.222244	1891	0.947118	0.223857
Head is a doctor	3893	0.591575	0.491606	2002	0.548951	0.497722	1891	0.6367	0.481077
Morning only puskesmas	3804	0.936383	0.244102	1913	0.944589	0.228839	1891	0.9280804	0.2584
Urban region	3896	0.598819	0.4902	2005	0.582544	0.493263	1891	0.616076	0.486468
Panel b: Head doctors									
If practice privately	1768	0.781109	0.413612	574	0.80662	0.395293	1194	0.768844	0.421749
Hours worked/wk	1741	17.64805	15.35998	556	28.83993	11.85656	1185	12.39684	13.94291
Patients seen/wk	1668	100.1097	105.6492	558	92.45699	87.79371	1110	103.9568	113.4123
Experience (in years)	1706	12.65358	7.123966	541	10.78743	6.610395	1165	13.52017	7.190031
Speaks local language	1768	0.923077	0.266545	574	0.905923	0.29219	1194	0.931323	0.25301
Head is a doctor	1768	1	0	574	1	0	1194	1	0
Morning only puskesmas	1768	0.914593	0.279566	574	0.914634	0.279669	1194	0.914573	0.279633
Urban region	1768	0.710407	0.453702	574	0.66899	0.470987	1194	0.730318	0.44398

Source: Authors' calculation using four rounds of IFLS data 1993-2007.

Table 2: Mean comparisons of selected variables below/above 3 years of experience

Post-97 .	Experience>=3yrs	Experience<3yrs	t-stat
If holds a private practice, all	0.73	0.66	0.7562
If holds a private practice, urban	0.71	0.62	0.7394
If holds a private practice, rural	0.76	0.71	0.5171
Hours worked/wk	13.03	13.2	0.1078
Patients seen/wk	75.9	88.7	1.0411
Holds private practice in the puskesmas	0.18	0.30	1.7286*
Pre-97 .	**Experience>=3yrs**	**Experience<3yrs**	**T-stat**
If holds a private practice, all	0.78	0.74	0.8826
If holds a private practice, urban	0.75	0.67	1.0913
If holds a private practice, rural	0.83	0.79	0.5718
Hours worked/wk	27.6	25.9	1.1516
Patients seen/wk	72.4	68.9	0.3470
Holds private practice in the pusk	0.18	0.37	-3.7761***

Note: Significance level: *** p<0.01, ** p<0.05, * p<0.1

Table 3. OLS estimates of likelihood of private practices

CONTROLS	(1) Unweighted	(2) Weight1	(3) Weight2	(4) Weight3
		Doctor heads		
NO	0.1949*	0.3901***	0.3119***	0.2755**
	(0.106)	(0.130)	(0.114)	(0.109)
YES	0.1888*	0.4021***	0.3092***	0.2662**
	(0.106)	(0.130)	(0.113)	(0.109)
Observations	1,767	1,019	1,681	1,703
		All heads		
NO	0.1404*	0.2872**	0.2659**	0.2714**
	(0.102)	(0.126)	(0.126)	(0.122)
YES	0.1773*	0.2858**	0.2596**	0.2663**
	(0.104)	(0.122)	(0.124)	(0.122)
Observations	2,790	1,362	2,172	2,237

Notes: The dependent variable in all models is PP (private practice). The coefficients reported correspond to the coefficient of Exp>=3 x Post97. *** p<0.01, ** p<0.05, * p<0.1. Clustered robust standard errors in parenthesis by Kabutapen district and year. All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are llang, urban, doctors&nurses>1, headdoc, access to pucca road and morning only puskesmas (for all heads). Naturally the head is a doctor dummy is dropped for doctor heads (see top panel). Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centered at Exp-2.5.

Table 4. Placebo tests for first-stage – doctor heads

	(1)	(2)	(3)	(4)
CONTROLS	Unweighted	Weight1	Weight2	Weight3
	All years - Exp>=4 x Post97			
NO	-0.0504	-0.0535	-0.0342	-0.0489
	(0.092)	(0.136)	(0.114)	(0.107)
YES	-0.0477	-0.0386	-0.0228	-0.0429
	(0.090)	(0.129)	(0.109)	(0.104)
	All years - Exp>=5 x Post97			
NO	-0.0406	-0.1408	-0.1239	-0.1028
	(0.072)	(0.106)	(0.098)	(0.093)
YES	-0.0367	-0.1137	-0.1189	-0.0969
	(0.072)	(0.105)	(0.095)	(0.091)
	1993 and 1997 only - Exp>=3 x 1[year=1997]			
NO	-0.0975	-0.0195	-0.1100	-0.1162
	(0.104)	(0.125)	(0.111)	(0.108)
YES	-0.0975	-0.0404	-0.1153	-0.1204
	(0.103)	(0.122)	(0.108)	(0.105)
	2000 and 2007 only - Exp>=3 x 1[year=2007]			
NO	0.2140	0.3349	0.2458	0.2696
	(0.162)	(0.202)	(0.183)	(0.166)
YES	0.1829	0.3429	0.1950	0.2333
	(0.164)	(0.216)	(0.207)	(0.179)

Notes: The dependent variable in all models is PP (private practice). The coefficients reported correspond to the coefficient of Exp>=4 x Post97 and Exp>=5 x Post97. *** p<0.01, ** p<0.05, * p<0.1. Clustered robust standard errors in parenthesis by Kabutapen district and year. All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp minus 0.5.

Table 4A. Placebo tests for first-stage – all heads

	(1)	(2)	(3)	(4)
CONTROLS	Unweighted	Weight1	Weight2	Weight3
	All years - Exp>=4 x Post97			
NO	0.0152	0.0886	0.1130	0.0885
	(0.079)	(0.121)	(0.109)	(0.103)
YES	0.0386	0.1008	0.1178	0.0919
	(0.079)	(0.115)	(0.106)	(0.101)
	All years - Exp>=5 x Post97			
NO	0.0113	-0.0211	0.0288	0.0269
	(0.061)	(0.097)	(0.084)	(0.079)
YES	0.0266	-0.0101	0.0362	0.0317
	(0.060)	(0.090)	(0.081)	(0.077)
	1993 and 1997 only - Exp>=3 x 1[year=1997]			
NO	-0.2706***	-0.1242	-0.2007*	-0.2280**
	(0.090)	(0.119)	(0.103)	(0.098)
YES	-0.1921**	-0.0808	-0.1601	-0.1872*
	(0.090)	(0.113)	(0.100)	(0.095)
	2000 and 2007 only - Exp>=3 x 1[year=2007]			
NO	0.1859	0.3658*	0.2677	0.2919*
	(0.145)	(0.204)	(0.187)	(0.169)
YES	0.1848	0.3714*	0.2463	0.2805
	(0.148)	(0.207)	(0.200)	(0.175)

Notes: The dependent variable in all models is PP (private practice). The coefficients reported correspond to the coefficient of Exp>=4 x Post97 and Exp>=5 x Post97. *** p<0.01, ** p<0.05, * p<0.1. Clustered robust standard errors in parenthesis by Kabutapen district and year. All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are llang, urban, doctors&nurses>1, headdoc, access to pucca road and morning only puskesmas Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp minus 0.5.

Table 5. IV estimates of hours worked per week – doctor heads

		(1)	(2)	(3)	(4)
Controls/Winsorized		Unweighted	Weight1	Weight2	Weight3
No/ Yes	PP IV	-18.6019	-5.2732*	-10.109***	-12.954***
		(15.149)	(3.043)	(3.436)	(4.460)
Cragg-Donald test	Wald F-stat	2.593	44.993	36.470	23.502
Yes/ Yes	PP IV	-20.0764	-5.1506*	-11.276***	-14.679***
		(16.788)	(2.990)	(3.636)	(4.952)
Cragg-Donald test	Wald F-stat	2.309	46.385	34.470	21.046
No/ No	PP IV	-15.4904	-10.7228**	-16.838***	-17.963***
		(21.659)	(4.348)	(4.967)	(6.278)
Cragg-Donald test	Wald F-stat	2.156	44.671	35.877	22.848
Yes/No	PP IV	-17.0456	-9.435**	-17.417***	-19.457***
		(23.446)	(4.198)	(5.187)	(6.869)
Cragg-Donald test	Wald F-stat	2.405	42.464	37.001	24.568
Mean of dep. var.		17.6481	18.4982	17.6828	17.5805

Note: The dependent variable in all models is hours worked per week and we estimate it for the subsample of doctor heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 5A. IV estimates of hours worked per week - all heads

		(1)	(2)	(3)	(4)
Controls/Winsorized		Unweighted	Weight1	Weight2	Weight3
No/ Yes	PP IV	-7.0324	0.1799	-0.3968	-1.9829
		(14.684)	(3.909)	(3.600)	(3.771)
Cragg-Donald test	Wald F-stat	1.423	30.308	30.648	26.566
Yes/ Yes	PP IV	-9.0329	-3.7075	-3.0416	-3.5947
		(11.701)	(3.952)	(3.666)	(3.796)
Cragg-Donald test	Wald F-stat	2.471	29.297	29.506	26.181
No/ No	PP IV	-1.7580	-3.4746	-2.6317	-3.5121
		(33.172)	(6.080)	(7.964)	(8.657)
Cragg-Donald test	Wald F-stat	1.422	30.308	30.647	26.566
Yes/No	PP IV	-6.5913	-7.3237	-7.4747	-6.8924
		(25.572)	(6.195)	(8.151)	(8.734)
Cragg-Donald test	Wald F-stat	1.246	29.446	28.565	24.404
Mean of dep. var.	Non-winsor	20.4563	20.7803	19.9687	19.9181

Note: The dependent variable in all models is hours worked per week and we consider the sample of all heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 6. IV estimates of patients seen per week – doctor heads

		(1)	(2)	(3)	(4)
Controls/Winsorized		Unweighted	Weight1	Weight2	Weight3
No/ Yes	PP IV	20.3400	69.5349**	75.0656**	62.3877
		(123.826)	(27.305)	(33.308)	(42.874)
Cragg-Donald test	Wald F-stat	1.604	39.963	25.889	14.273
Yes/ Yes	PP IV	14.2048	64.4053**	69.7929**	60.1492
		(136.588)	(25.807)	(33.299)	(45.431)
Cragg-Donald test	Wald F-stat	1.293	41.577	24.585	12.346
No/ No	PP IV	-43.1786	67.760*	31.067	-21.055
		(203.541)	(38.858)	(44.988)	(60.346)
Cragg-Donald test	Wald F-stat	1.60350	39.96306	25.88916	14.2734
Yes/No	PP IV	-55.6115	66.4020*	31.7647	-18.8520
		(227.325)	(36.706)	(45.197)	(63.833)
Cragg-Donald test	Wald F-stat	0.85524	24.44859	18.62331	14.17104
Mean of dep. var.	Non-winsor	100.1097	96.8288	100.4195	100.3390

Note: The dependent variable in all models is patients seen per week. We estimate it for the subsample of doctor heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 6A. IV estimates of patients seen per week – all heads

		(1)	(2)	(3)	(4)
Controls/Winsorized		Unweighted	Weight1	Weight2	Weight3
No/ Yes	PP IV	-125.6346	28.7440	-5.3927	-27.0568
		(177.497)	(28.870)	(29.373)	(33.831)
Cragg-Donald test	Wald F-stat	1.084	25.972	20.974	16.158
Yes/ Yes	PP IV	-44.8571	64.6193**	37.1179	11.9096
		(114.057)	(31.088)	(31.160)	(33.397)
Cragg-Donald test	Wald F-stat	1.511	12.957	18.899	15.021
No/ No	PP IV	-285.7343	5.207	-85.166*	-139.532**
		(342.572)	(41.291)	(47.006)	(60.381)
Cragg-Donald test	Wald F-stat	1.0838	25.9725	20.9735	16.1580
Yes/No	PP IV	-154.2977	56.9300	-22.7316	-81.0591
		(211.064)	(42.692)	(44.204)	(53.794)
Cragg-Donald test	Wald F-stat	0.8552	24.4485	18.6233	14.1710
		84.2199	84.9529	89.9362	89.2844

Note: The dependent variable in all models is patients seen per week. We estimate it for the subsample of all heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Mean comparisons of selected characteristics between private practices located at and away from the puskesmas

Variables	PP at puskesmas	PP away from puskesmas	T-statistic
	All heads		
Practices privately	1.0	0.70	11.9849***
Hours worked per week	24.0	20.6	4.3041***
Patients seen per week	68.6	90.5	-3.8002***
Head is a doctor	0.61	0.79	-6.7575***
Experience (years)	7.35	8.16	-3.3159***
Tenure at the puskesmas (years)	7.43	6.00	4.0730***
If speaks local language	0.93	0.92	0.4335
Doctors & nurses>1	0.55	0.62	-1.9097*
Morning only puskesmas	0.94	0.93	0.9049
Access to pucca road	0.75	0.87	-5.5556***
If urban	0.32	0.62	-9.8418***
	Dual practitioner heads		
Practices privately	-	-	-
Hours worked per week	21.6	17.8	3.9599***
Patients seen per week	69.0	89.3	-4.4027***
Head is a doctor	0.47	0.74	-11.9677***
Experience (years)	10.0	13.6	-8.9375***
Tenure at the puskesmas (years)	8.6	7.6	2.9322***
If speaks local language	0.9494	0.9471	0.2093
Doctors & nurses>1	0.47	0.56	-3.7145***
Morning only puskesmas	0.95	0.93	1.4640
Access to pucca road	0.80	0.90	-6.5241***
If urban	0.39	0.67	-11.9860***

Note: The table shows the mean comparisons of selected characteristics between private practice (PP) located at and away from the puskesmas. The upper panel considers all heads while the lower panel considers the dual practitioner heads. Columns (1)-(2) show the means for these two types of puskesmas and column (3) shows the corresponding t-statistics for comparison. significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Private practice likelihood estimates when the private practice is located away from the puskesmas

		(1)	(2)	(3)	(4)
Controls		Unweighted	Weight1	Weight2	Weight3
Doctor heads	Exp3xPost97	0.3003**	0.6450***	0.4777***	0.4222***
		(0.147)	(0.182)	(0.165)	(0.157)
	Observations	1,494	803	1,416	1,438
	R-squared	0.100	0.537	0.410	0.338
All heads	Exp3xPost97	0.2401	0.5449***	0.4008**	0.3771**
		(0.147)	(0.182)	(0.177)	(0.170)
	Observations	2,221	1,016	1,737	1,794
	R-squared	0.104	0.483	0.349	0.271

Notes: The dependent variable in all models is PP (private practice). The coefficients reported correspond to the coefficient of Exp>=3 x Post97. *** p<0.01, ** p<0.05, * p<0.1. Clustered robust standard errors in parenthesis by Kabutapen district and year. All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are llang, urban, doctors&nurses>1, headdoc, access to pucca road and morning only puskesmas (for all heads). Naturally the head is a doctor dummy is dropped for doctor heads (see top panel). Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centered at Exp-2.5.

Table 9. Effect of private practice on hours worked and patients seen, private practice is located away from the puskesmas

		(1)	(2)	(3)	(4)
Sample	VARIABLES	Unweighted	Weight1	Weight2	Weight3
Panel a		**Winsorised Hours worked per week**			
Doctor heads	PP IV	-17.9416	-11.481***	-15.089***	-16.277***
		(11.995)	(2.445)	(3.063)	(3.864)
Cragg-Donald test	Wald F-stat	3.683	66.678	50.509	33.141
All heads	PP IV	-17.7314	-9.3852***	-11.246***	-12.469***
		(15.251)	(2.629)	(3.250)	(3.895)
Cragg-Donald test	Wald F-stat	2.184	55.632	38.813	27.837
Panel b		**Winsorised Patients seen per week**			
Doctor heads	PP IV	-24.1233	35.6237*	65.1099**	53.0908
		(97.256)	(18.972)	(26.250)	(33.954)
Cragg-Donald test	Wald F-stat	2.284	55.644	33.164	18.584
All heads	PP IV	-70.7608	33.2675*	46.1121*	24.6149
		(120.534)	(19.030)	(25.686)	(30.677)
Cragg-Donald test	Wald F-stat	1.460	46.221	26.290	16.563

Note: The dependent variables are hours per week and patients seen per week in panel a and b respectively. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Table A1. OLS non-IV estimates of hours worked per week (winsorized), patients seen per week (winsorized) and referral

Controls	VARIABLES	(1) All Unweighted	(2) Head doc Unweighted	(3) All Weight1	(4) Head doc Weight1	(5) All Weight2	(6) Head doc Weight2	(7) All Weight3	(8) Head doc Weight3
				(a) Hours worked per week					
Yes	PP	-0.4368	-0.5402	0.6386	-1.1034*	0.9264**	-0.8113*	0.6317	-0.6691
		(0.317)	(0.406)	(0.590)	(0.650)	(0.431)	(0.467)	(0.412)	(0.447)
				(b) patients seen per week					
Yes	PP	-5.4161**	-3.4792	-1.6498	-4.3586	-3.8090	-6.6834*	-3.0256	-5.5011
		(2.701)	(3.915)	(3.892)	(4.984)	(2.908)	(3.774)	(2.855)	(3.749)

Notes: Panels (a) and (b) respectively show ols non-IV estimates of hours worked per week and patients seen per week. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1. All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, a dummy if the health worker speaks local language, if works in an urban puskesmas, if there is more than one doctors & nurses, if the head is a doctor (this dummy drops when head is a doctor), access to pucca road and morning only puskesmas (for columns (1), (3), (5) and (7) only). Columns (2), (4), (6) and (8) show the estimates of head doctors and hence the head is a doctor dummy is dropped here.

Appendix 2

Table A2.1. A historical perspective of government regulation in the health sector in Indonesia

Regulation	Description
Constitution 1945	Guarantees “the right to health” as a realization of general welfare
Presidential Instruction 1974	Mandated that all new medical graduates serve in under-served rural districts for 1-3 years
Presidential Regulation No. 37 1991	Regulated the recruitment of doctors as temporary employees.
Health Act 23 1992	Regulates health personnel training and education as conducted by government and private sector institutions.
Government Rule No. 23 1996	Regulates type of health personnel
Ministry of Health Regulation No. 916 1997	Regulates the licensing of Medical Practitioners
Ministry of Health Decree No. 1239 2000	Nurse’s Registration and Practice regulations
Ministry of Health Regulation No. 1540 2002	Regulates the placement of health doctors during the service period
Ministry of Education Act No. 20 2003	Develops standards for higher education for medical professionals
Medical Practitioner Act No. 29 2004	Regulates that every doctor and dentist has to ensure quality services and cost containment.
Social Security Law No. 40 2004	Mandates the nature of social security contributions and services
Local Government Authority Act No. 32 2004	Provides each local government the authority to recruit their own medical personnel as local government authority
Ministry of Health Regulation No. 1419 2005	Regulates the conduct of medical and dental practice

Source: USAID, 2009

Table A2.2. Variable definitions

Variable abbreviations	Definitions
PP	Binary variable indicating private practice of health professionals
Exp3	Binary variable indicating if experience>=3 years and 0 otherwise
Post97	Binary variable indicating the years after the introduction of the 1997 regulation and 0 otherwise
hours_wk	Hours worked per week by the head of the puskesmas
patients_wk	Number of patients seen per week by the head of the puskesmas
whours_wk	Winsorized hours worked per week by the head of the puskesmas
wpatients_wk	Winsorized number of patients seen per week by the head of the puskesmas
Referral	Binary variable indicating if any puskesmas patient is referred to a private clinic
Private practice in the puskesmasl	Binary variable indicating if private practice is held in the public hospital
Llang	Binary variable indicating if the health professional speaks the local language
Urban	Binary variable indicating if the puskesmas is in the urban region
Proad	Binary variable indicating if the puskesmas has access to pucca road
Morn_only	Binary variable indicating if the puskesmas holds clinical service only in the morning

Table A2.3. IV estimates of log outcomes (hours worked and patients seen per week) for doctor heads

Controls/Winsorized	VARIABLES	(1) Unweighted	(2) Weight1	(3) Weight2	(4) Weight3
	Winsorised log hours worked per week				
No/ Yes	PP IV	-1.1399	-0.3694**	-0.6326***	-0.7851***
		(0.900)	(0.183)	(0.205)	(0.261)
Cragg-Donald test	Wald F-stat	2.5929	44.9926	36.4701	23.5023
Yes/ Yes	PP IV	-1.2273	-0.3642**	-0.6956***	-0.8869***
		(0.998)	(0.180)	(0.217)	(0.291)
Cragg-Donald test	Wald F-stat	2.3107	46.4368	34.4925	21.0590
	Winsorised log patients seen per week				
No/ Yes	PP IV	0.6161	1.1829***	1.2822***	1.1985*
		(1.766)	(0.406)	(0.497)	(0.647)
Cragg-Donald test	Wald F-stat	1.6035	39.9630	25.8891	14.2734
Yes/ Yes	PP IV	0.5255	1.0594***	1.1398**	1.0994
		(1.938)	(0.380)	(0.490)	(0.677)
Cragg-Donald test	Wald F-stat	1.2942	41.6243	24.6019	12.3546

Note: The table shows the IV estimates of natural logarithm of hours worked and patients seen per week for doctor heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table A2.4. IV estimates of log outcomes for all heads

Controls/Winsorized	VARIABLES	(1) Unweighted	(2) Weight1	(3) Weight2	(4) Weight3
	Winsorised log hours worked per week				
No/ Yes	PP IV	-0.7723	-0.1522	-0.2136	-0.2787
		(0.978)	(0.225)	(0.207)	(0.215)
Cragg-Donald test	Wald F-stat	1.4225	30.3077	30.6475	26.5659
Yes/ Yes	PP IV	-0.7791	-0.3388	-0.3396	-0.3555
		(0.749)	(0.232)	(0.215)	(0.220)
Cragg-Donald test	Wald F-stat	2.4705	29.2971	29.5059	26.1807
	Winsorised log patients seen per week				
No/ Yes	PP IV	-1.6353	0.8182*	0.1496	-0.1646
		(2.459)	(0.448)	(0.436)	(0.490)
Cragg-Donald test	Wald F-stat	1.0837	25.9724	20.9735	16.1580
Yes/ Yes	PP IV	-0.6688	1.3493***	0.7608	0.3793
		(1.664)	(0.501)	(0.479)	(0.501)
Cragg-Donald test	Wald F-stat	1.5110	23.9566	18.8989	15.0207

Note: The table shows the IV estimates of natural logarithm of hours worked and patients seen per week for all heads. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

Table A2.5. IV estimates of log outcomes for doctor heads, private practice away

Controls/Winsorized	VARIABLES	(1) Unweighted	(2) Weight1	(3) Weight2	(4) Weight3
		Winsorised log hours worked per week			
No/ Yes	PP IV	-1.1462	-0.7199***	-0.8291***	-0.9022***
		(0.696)	(0.146)	(0.162)	(0.202)
Cragg-Donald test	Wald F-stat	4.0995	70.0047	59.8394	39.0409
Yes/ Yes	PP IV	-1.2243	-0.7636***	-0.9547***	-1.0355***
		(0.767)	(0.151)	(0.187)	(0.236)
Cragg-Donald test	Wald F-stat	3.6857	66.7746	50.5485	33.166
		Winsorised log patients seen per week			
No/ Yes	PP IV	0.0321	0.4027	0.7641**	0.6675
		(1.217)	(0.261)	(0.322)	(0.415)
Cragg-Donald test	Wald F-stat	2.7484	60.5968	42.0216	24.0683
Yes/ Yes	PP IV	0.0397	0.4991*	0.9921***	0.9043*
		(1.333)	(0.263)	(0.375)	(0.494)
Cragg-Donald test	Wald F-stat	2.2852	55.7275	33.1913	18.5991

Note: The table shows the IV estimates of natural logarithm of hours worked and patients seen per week for doctor heads when the private practice is away from the puskesmas. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

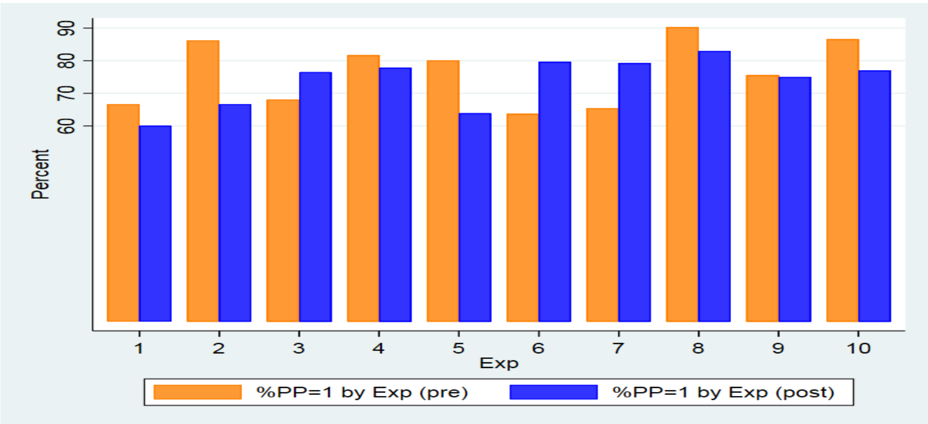
Table A2.6. IV estimates of log outcomes for all heads, private practice away

Controls/Winsorized	VARIABLES	(1) Unweighted	(2) Weight1	(3) Weight2	(4) Weight3
		Winsorised log hours worked per week			
No/ Yes	PP IV	-1.7945	-0.7061***	-0.8460***	-0.9139***
		(1.817)	(0.171)	(0.216)	(0.253)
Cragg-Donald test	Wald F-stat	1.1191	51.42	35.1395	25.8616
Yes/ Yes	PP IV	-1.8726	-0.6932***	-0.8608***	-0.9391***
		(2.033)	(0.165)	(0.218)	(0.264)
Cragg-Donald test	Wald F-stat	0.9684	54.1945	35.7645	24.9972
		Winsorised log patients seen per week			
No/ Yes	PP IV	-1.6510	-0.0603	-0.1737	-0.4208
		(2.603)	(0.280)	(0.372)	(0.475)
Cragg-Donald test	Wald F-stat	0.9148	45.2513	24.4974	15.5316
Yes/ Yes	PP IV	-1.5617	0.4555*	0.5730	0.2861
		(2.805)	(0.272)	(0.377)	(0.462)
Cragg-Donald test	Wald F-stat	0.7311	46.0876	24.5326	14.865

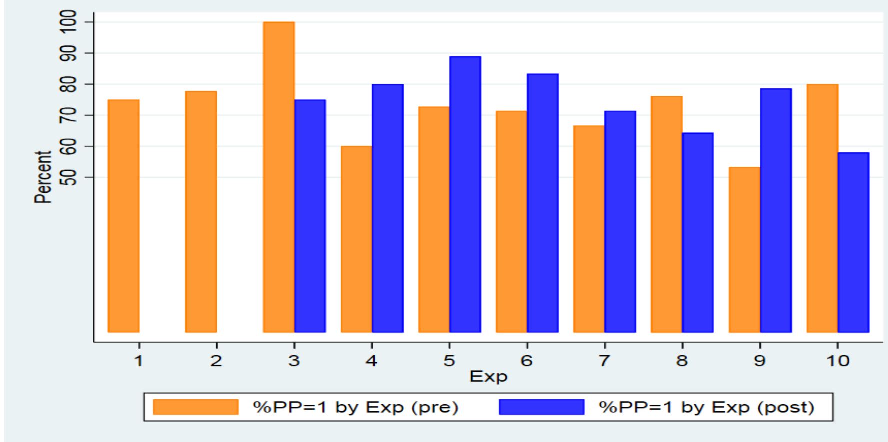
Note: The table shows the IV estimates of natural logarithm of hours worked and patients seen per week for all heads when the private practice is away from the puskesmas. The coefficients reported correspond to the coefficient IV estimates of private practice (PP). All specifications include fixed-effects by the interaction of Kabutapen district and year. Controls are Exp>=3, Post97, llang, urban, doctors&nurses>1, access to pucca road and morning only puskesmas. Weight1, Weight2 and Weight3 correspond to analytical weights for a Gaussian kernel with standard deviation of 1, 2 and 3, respectively, centred at the corresponding value of Exp= 3 minus 0.5. Cragg-Donald test statistic is a test for weak identification test. The test answers the question: can we reject the null hypothesis that the maximum relative bias due to weak instruments is 10%, 25% etc. A lower acceptable bias means that the instrument has to achieve a higher first stage F-statistic. Stock-Yogo weak ID test critical values for 10% maximal IV size is 16.38 and that for 15% maximal IV size is 8.96 for our model. Standard errors are shown in the parentheses; significance level: *** p<0.01, ** p<0.05, * p<0.1.

FIGURES

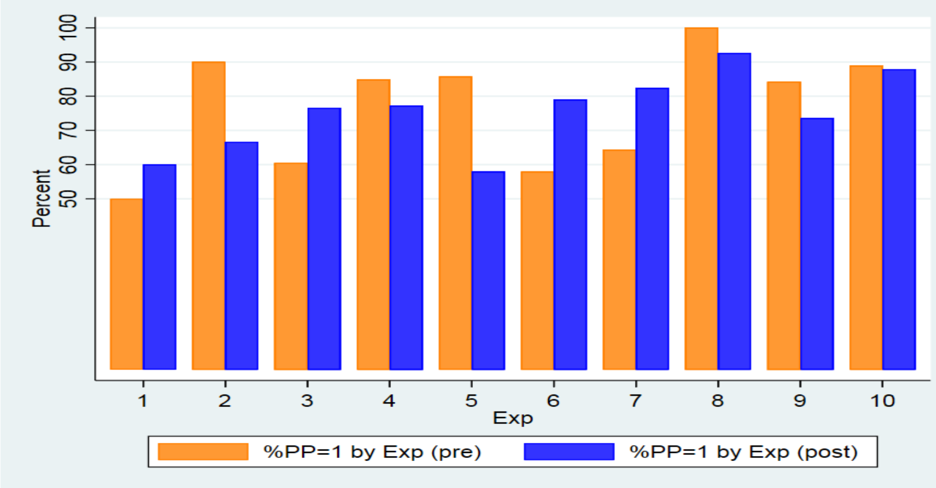
Figure 1: Enforcement of the regulation



(a)PP likelihood of all heads pre- and post 1997 regulation



(b)PP likelihood of non-doctor heads pre- and post 1997 regulation



(c)PP likelihood of doctor heads pre- and post- 1997 regulation

The figure shows the private practice likelihood by experience level pre- and post-regulation. Panel (a) shows the case of all heads; panel (b) for non-doctor heads and panel (c) for doctor heads in the sample.