

# Learning the Ropes: Task Specific Experience and the Output of Idaho State Troopers

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## Abstract

We provide new estimates of the importance of task-specific and job-specific human capital, using a unique data set of all citations issued by state troopers in Idaho between 2005 and 2009, linked to human resource records of the Idaho State Police. We document that the relationship between trooper experience and the number of tickets issued in a given month follows a logarithmic growth pattern, similar to previously documented relationships between experience and output in other industries. We then show that when the Idaho Legislature amends laws that the troopers must enforce, there is a temporary reduction in the number of times troopers “use” that law. Task-specific and job-specific experience appear to be substitutes, in the sense that the reduction in citations is largest for officers with less experience, and law changes later in a given officer’s career have a smaller effect on their use of the law.

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## 1. Introduction

One of the central issues in labor economics is the relative contributions of specific and general human capital to overall labor productivity. The growing availability of detailed firm-worker matched data has allowed empirical economists to test many of the predominate theories about the transferability of worker productivity from one firm to another, spurring the refinement of the idea of specific human capital from a firm-based notion to one that is focused on the actual job, or set of tasks, that a worker performs at a given firm (Abowd and Kramarz 1998, Gibbons and Waldman 2004). A small number of empirical papers has highlighted the role of tasks in determining a worker's wage or output, rather than their occupation per se (Clement et al 2007, Autor and Handel 2009, Gathmann and Schonberg 2010, Ost 2012,), but the impact of task-specific experience on productivity remains a relatively unexplored area in labor economics (Acemoglu and Autor 2010).

The extent to which task-specific and more general firm-specific experience are substitutes or compliments in productivity is also unclear. There is some evidence that workers with more tenure are less willing to change the tasks associated with their current job, even when switching improves productivity of switchers (Ichniowski and Shaw 1995), but what happens workers have no choice about whether or not to adopt new workplace procedures?

In this paper, we build on the existing literature on experience and productivity using a unique administrative data set that allows us to differentiate between the effects of firm-specific and task-specific tenure on the output of state troopers employed by the Idaho State Police (ISP) between 2004 and 2009. Consistent with existing evidence on tenure and output, particularly Shaw and Lazear 2008, we show that as firm-specific tenure increases, the number of tickets issued by a trooper for a given citation follows a roughly log-normal path, with some decline for

substantively older workers. We then define task-specific experience as the length of time a trooper has been charged with issuing citations for a specific Idaho law. Essentially, we point out that enforcing each Idaho law constitutes a distinct task that the trooper must perform; identifying citizens who are driving too fast in a residential area is a different skill from identifying citizens who are driving under the influence, which is also different from identifying people with improper towing permits.

During our sample period, the Idaho State Legislature amended 81 out of the 287 criminal and traffic laws that the ISP troopers regularly enforce. As a result, at any given point in time we observe troopers with different levels of firm-specific experience performing different tasks with which they have varying levels of experience, based on when each law was last amended. For a given task, the level of task and firm-specific experience will vary across troopers, based on when the trooper was hired relative to the last time the law changed. Unlike existing empirical research on task-based experience, there is no scope for selection in the adoption of new tasks; troopers cannot choose to enforce old laws, they can only perform the new tasks poorly by issuing fewer tickets. It is also highly implausible that the experience of any given trooper at the ISP would influence whether or not the Idaho legislature changes a law.

A-priori, it is not obvious whether older or younger troopers will learn to enforce new laws faster. Many theories of human capital investment imply an inverse relationship between job tenure and likelihood of adopting new workplace tasks. For example, workers with longer time horizons will receive larger benefits from any investment in task or firm specific human capital, meaning that younger workers that recently underwent training may disproportionately benefit from technological innovations in the workplace (Bartel et al. 2007). Similarly, there is a general consensus in the literature that younger firms are more likely to adopt new techniques, in part

because these firms are more likely to learn about new ideas during more general market research (Ichinowski et al. 1995). At the same time, if learning about new job requirements takes away from time working then, to the extent that productivity falls with tenure, the opportunity cost of new training will be lower for older workers (Shaw and Lazear 2008). There is also some empirical evidence that more experienced workers may be better able to adapt to new environments; U.S. wages of foreign-born men are increasing in foreign job experience (Mincer and Ofek 1982). The ability of workers to adapt to new tasks may also be a function of the type of change- the adjustment costs associated with a task that increases a worker's output may be larger than the adjustment costs associated with a reduction in output (Hammermesh and Pfann 1996).

We find that, on average, job and task-specific tenure appear to be substitutes. When the effects of the two sources of experience are assumed to be independent, the impact of job specific experience seems to be more important than task specific experience; a trooper with 10% increase in experience will issue roughly 4% more citations. However, there appear to be important nonlinearities in this effect. Specifically, including interactions of job and task specific experience consistently yields a positive and statistically precise relationship between experience with a given law and citations, which declines as troopers gain overall experience. The substitutability of job and task-specific experience holds for both older and younger workers, although the first order impact of job-specific experience is positive for younger troopers and negative for older troopers. Notably, this seems to be primarily a pure adjustment cost, as we do not observe asymmetric responses to statute changes that increase or decrease the number of actions that are considered illegal, nor do we observe a differential response to laws that increase the penalty of offending.

In addition to complementing the existing research on task-based productivity, our results are consistent with recent research in empirical legal studies documenting the response of judges to changes in sentencing rules (Bushway et al. 2012, Schanzenbach & Tiller 2008). The finding that a trooper's experience, both over all and with a given law, affects the rate at which individuals are accused of crimes has important implications for criminal justice policy.

The paper proceeds as follows: in section two, we review the existing empirical evidence on task-specific experience and output. We then present an intuitive theoretical framework for thinking about the incentives faced by state troopers when enforcing the Idaho Criminal Statute. In section four, we describe our data, and provide some institutional background on the Idaho State Police. We present our analytic framework and reduced form results in section five, and we conclude with a brief discussion in section six.

## **2. Related Literature**

Clement et al. (2007) was the first empirical paper to explicitly focus on disentangling task-specific experience from either job-specific or general experience. Using a cross section of forecast analysts, they estimate how a given forecaster's ability to predict a firm's earnings after a restructuring varies with their time at a firm, the number of forecast revisions the analyst has previously done, and the number of specific, post-restructuring forecasts that the analyst has done for the firm. They find that analysts who have previously studied firms undergoing a major organizational change are much better at forecasting a given company's earnings after restructuring, but not any better at general forecasting tasks. While this is a purely cross-sectional analysis, the results of Clement et al. (2007) suggest that task-specific experience affects productivity, over and above general experience.

Gathmann and Schonberg (2010) build on Clement et al. (2007) by estimating the relationship between task-specific experience and productivity using German social security records, which contain both wages and job descriptions for German workers employed between 1975 and 2001. Identifying the specific tasks associated with each job in the social security data, and following workers across individual jobs, Gathmann and Schonberg (2010) estimate that a substantial amount of a persons' wage growth over their lifetime is due to task-specific experience. The importance of the task, rather than the job per-se, depends on the skill level of the individual; task-specific experience explains more of the lifetime wage growth for people with higher levels of education.

Finally, Ost (2012) disentangles job and task-specific experience using elementary school teachers in North Carolina who teach multiple grades. Using test scores as a measure of teacher skill, Ost (2012) finds that teachers with more experience teaching math (in general) had students with higher test scores, but experience with grade-specific math was also important. Depending on the fixed effect structure, Ost estimates that the impact of grade-specific experience on math scores is between  $\frac{1}{2}$  and as large as the impact of general experience on math scores. For reading scores, general experience was consistently more important than grade-specific experience.

Our analysis of the ISP builds on this literature in a number of ways. Like Gathmann and Schonberg (2010) and Ost (2012), we observe workers over time, allowing us to minimize bias due to unobserved time invariant differences in innate ability. Similar to Clement et al. (2007) and Ost (2012), we observe an individual worker's output directly, rather than wages. While wages are certainly correlated with productivity, there is considerable concern that wages might also be correlated with observable and unobservable individual attributes, such as educational attainment, unions, discrimination, pedigree and ability. Indeed, Shaw and Lazear (2008) show

that the relationship between pay and tenure is much flatter than the relationship between the actual output of factory workers and their experience. Finally, like Clement et al. (2007) and Gathmann and Schonberg (2010) we observe workers engaged in different tasks at the same time, allowing us to more credibly separate general from task-specific experience.

Understanding how workers adapt to changes in the particulars of their jobs is important for our understanding of human capital and productivity, but the fact that we are studying the behavior of state troopers means that our findings have particular relevance for law and economics as well. State troopers are expected to enforce state laws, and the issuance of citations is a byproduct of their job duties. As such, state troopers are one of many legal agents who “provide” justice in society.

Despite the fact that the probability and severity of punishment is typically expected to be based on the particular rule being broken, there is a large body of evidence in empirical legal studies documenting the importance of extra-legal factors, including the age or residence of the suspect, the race of the law enforcement officer, and even the timing of the event, in legal outcomes (Bushway and Piehl 2007, Makowsky and Stratmann 2009, Antonovics and Knight 2009, Fagan and Geller 2010, Danziger et al. 2012). Our results are also directly related to research on reporting bias, the probability that law enforcement officers report offenses to the Uniform Crime Reports, a potential issue raised in Maltz (1999) and documented in Vollard & Hamed (2012). We build on this literature by providing some insight into the correlates of a law enforcement officer’s decision to enforce the law.

## **2. Theoretical Foundation**

We present an intuitive model of task-specific and job specific experience to provide a framework for thinking about our reduced form model of citations and tenure. Suppose there are

three agents: civilians, troopers, and supervisors. Civilians have the choice to abide by the law or not. Troopers are charged with discerning illegal activity on the part of the civilians. If troopers either issue citations to civilians who have not actually violated a law, or fail to issue citations to civilians who do violate the law, they will be penalized by their supervisors. For the purposes of exposition, we will assume that citizens choose an action,  $a$ , that lies somewhere on a two dimensional line, and each law,  $v$ , in the Idaho statute makes a set of behaviors,  $R_v$ , illegal. If  $R_v < a$ , then the behavior  $a$  is illegal, and if  $R_v > a$ , then  $a$  is legal.

The trooper faces two sources of uncertainty. First, the trooper must be able to observe the actions taken by the citizen, in order to identify where  $a$  falls on the spectrum of legality. Ability to detect behavior is a job-specific skill that the trooper needs to enforce all laws. The second source of uncertainty facing a trooper has to do with the exact location of  $R_i$ ; even if they observe citizen behavior perfectly, do they know whether or not the behavior they observe “matches” the behavior outlawed by statute?

Because of these two sources of uncertainty, instead of  $a$ , the trooper observes  $a + \delta\beta^{EX_{iv}}$ , where  $\delta \sim N(0, \sigma)$  is a noise terms that represents the citizens’ ability to shade their own actions, and the trooper’s ability to match what the citizen is doing to the particular law. This term can be either positive (meaning that the trooper perceives more “illegality” than is true) or negative (the citizen’s actions appear “more legal” than they actually are). The third term,  $\beta^{EX}$  represents the trooper’s skill, with  $0 < \beta < 1$ , and  $EX_{iv} = t_i + \tau_v$ , a function of the trooper’s overall time working for the Idaho State Police ( $t_i$ ), and the amount of time that the trooper has enforced the particular version of law  $R_v$  ( $\tau_v$ ). Intuitively,  $\delta\beta^{t_i + \tau_v}$  can represent the trooper’s ability to collect the evidence required to make the citation valid in court, or experience with the amount of resistance a citizen will pose after being accused on a particular charge. As the trooper’s experience with

the particulars of a given law increases, they will be better able to accurately tell if a citizen's behavior exceeds legal bounds. By assuming that  $EX_{iv}$  is a linear function of  $t_v$  and  $\tau_i$ , we are assuming the empirically testable prediction that a more experienced trooper will be less affected by changes in the particulars of a given law.

There are four possible outcomes for the officer each time they interact with a citizen.

	$R_v - a - \delta\beta^{ti+tv} > 0$ (No Ticket)	$R_v - a - \delta\beta^{ti+tv} < 0$ (Issue Ticket)
$R_v - a > 0$ (Legal)	0	$-C_v$
$R_v - a < 0$ (Illegal)	$-EIS_v$	0

If the officer engages in the correct behavior, meaning that both  $R_v - a$  and  $R_v - a - \delta\beta^{ti+tv}$  have the same sign, then her payoff is normalized to zero. However, when  $\delta\beta^{ti+tv} < R_v - a < 0$  or  $\delta\beta^{ti+tv} > R_v - a > 0$ , the officer will issue an unjustified ticket or fail to punish a lawbreaker, respectively. Note that as  $t$  or  $\tau_i$  increases, both inequalities are less likely to be true, regardless of whether or not the draw of  $\delta$  is positive or negative. Both of these are costly outcomes from the officer's point of view. In the first case, citizens have the option of complaining to the officer's supervisor, meaning that the officer will incur a penalty  $C_v$ . If the officer "misses" a large number of citations, they may eventually be penalized by their supervisor.<sup>1</sup> We therefore describe the expected cost of not issuing a warranted ticket, or de-policing, as equal to  $EIS_v$ . Both  $C_v$  and  $EIS_v$  are assumed to vary across laws, with a misapplication of statutes with serious punishments being more costly to the officer.<sup>2</sup>

We argue that, for a given value of  $v$ ,  $C_v$  is larger than  $EIS_v$  for a number of institutional reasons. First, the Department of Justice places much higher weight on the frequency of citizen

<sup>1</sup> As we will discuss further, this is a less certain outcome than the penalty imposed by a citizen complaint.

<sup>2</sup> Intuitively, incorrectly ticketing someone for traveling 5 miles above the speed limit is less costly than incorrectly tickets someone traveling 50 miles over the speed limit. Issuing a larger ticket when one should not be issued should be more likely to result in a citizen complaint, and overlooking more egregious speeders should be more of a cause for disciplinary action than ignoring marginal speeders.

complaints than “de-policing” in their audits of law enforcement agencies (DOJ 2012). In 2007, approximately 39% of law enforcement agencies used some sort of computer-based “Early Intervention System” to monitor officer behavior (USDOJ 2007). Citizen complaints are a standard component of the officer performance metrics used by these systems, but tracking the number of arrests made by officers is only recommended for particularly comprehensive systems (Walker 2003). Second, as demonstrated by recent controversies in Arlington, VA, Logan, UT, and New York City, law enforcement agencies face significant public scrutiny when allegations of arrest “quotas” are raised, limiting supervisor’s ability to punish officers for issuing too few tickets, even if they suspect de-policing on the part of troopers.<sup>3</sup>

As experience with either observing citizen behavior ( $t_i$ ) or enforcing a given law, ( $\tau_v$ ), increases, troopers will be better able to detect actual rule breakers, and, given the institutionally-motivated asymmetry in the cost of mistakes, troopers will issue more citations as they gain experience.

### **3. Data and Institutional Background**

The largest law enforcement agency in Idaho is the Idaho State Police, which employed 282 commissioned officers and 225 full time staff in 2010. The roughly 150 troopers employed at any given time carry the largest burden in terms of enforcing the law, writing approximately 3,300 of the 4,600 citations issued each month. Turnover rates in the ISP are quite low relative to other state agencies; in 2010 there was a 7.2% turnover rate and only a 1.9% voluntary separation rate in the ISP, compared to a 12.8% turnover rate and 4.8% voluntary separation rate in the state as a whole (IHR 2012).

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<sup>3</sup>See <http://www.wjla.com/articles/2012/03/arlington-county-police-quotas-memo-retracted-73982.html>, [http://news.hjnews.com/opinion/article\\_37325c38-4df9-11e1-bc81-001871e3ce6c.html](http://news.hjnews.com/opinion/article_37325c38-4df9-11e1-bc81-001871e3ce6c.html) , [http://www.cbsnews.com/8301-201\\_162-57435418/judge-grants-class-action-status-to-suit-accusing-nypd-of-racial-bias-in-stop-and-frisk-policies/?tag=contentMain;contentBody](http://www.cbsnews.com/8301-201_162-57435418/judge-grants-class-action-status-to-suit-accusing-nypd-of-racial-bias-in-stop-and-frisk-policies/?tag=contentMain;contentBody)

For the purposes of this research, we are concerned about two major components of the ISP. First, a working knowledge of information dissemination about changes in laws is necessary to understand how an officer learns about changes in the penal code. Second, we want to understand the incentives (monetary or otherwise) that an ISP trooper faces when enforcing the law.

The productivity of officers (as measured by citations that the officer writes) results from familiarity with the law, the attentiveness of the officer both in learning and enforcing the law, and their effort. In this research we focus on the officer's familiarity with the law, especially with regards to laws that have changed since the officer has undergone initial, academy training.<sup>4</sup>

In order to join ISP at the rank of trooper, an individual must be admitted and complete training at the ISP academy and post-academy training. This training provides the baseline level of knowledge that an officer should have when enforcing the law. The majority of this training comes from, for example, gun training seminars that the agency will fund and make available at each staff office.

The ISP has three mechanisms for ensuring that officers are educated about changes in the law. First, when changes to the legal system that result in major shifts in the way that officers enforce the law occur, the ISP will hold in-service training sessions whereby specialists are sent to the field in order to carry out an educational program. In addition, officers are required to complete DVD training at their field station. For less complicated changes, the officers are informed in team meetings and in update emails, which contain information about changes in procedures that must be enacted but require little in the way of additional training. Finally, officers are equipped with Lexis-Nexis code for Idaho that is automatically and real-time

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<sup>4</sup> Before exiting the trooper academy, officers are required to pass a Peace Officer Standards and Training exam that requires the officer to know detailed information about the Federal and State Constitution and Statutes.

updated to include changes in the legal system, and carry a Post-Officer Standardized Training Book (which troopers refer to as their cheat sheet) that includes all federally mandated citations.

While accurate knowledge about the changes in the legal system is critical for our analysis, an equally important concern is the incentives that individual officers incur while enforcing the law. It is quite common to hear that officers face quotas and/or that their job depends upon raising revenues through fines. In order to overcome this public scrutiny, many states have begun publishing the composition of fines online.<sup>5</sup> In Idaho, fines for traffic citations do not end up directly funding the state police. In fact, for a standard speeding traffic infraction, approximately 40% of the fine goes to a state general fund, 20% goes toward court costs, 6% goes to a county justice fund, and 12% goes toward an ISTARs technology fund and emergency surcharge fee each.<sup>6</sup>

At the individual officer level, it is also unclear that issuing more tickets will lead to pay raises. The ISP, like most state departments in Idaho, pays their employees according to a pre-determined, fixed pay scale, with 19 grades. ISP troopers are paid as grade K state employees, and received pay raises as determined by state legislature.<sup>7</sup> In FY 2011, grade K state employees could earn an hourly wage of at least \$16.59, and no more than \$30.51, with the “policy” wage, which is in practice roughly the average wage for ISP employees, being \$24.41.

Through a research agreement, we obtained data on all citations issued by the ISP between January of 2002 and December of 2009. For each citation, we know the primary and secondary

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<sup>5</sup> See <http://www.isc.idaho.gov/rules/iir9.txt> for a breakdown of how a fine is distributed across agencies and programs in Idaho.

<sup>6</sup> ISTARs technology fund is the program that maintains the Idaho Statewide Trial Court Automated Records System.

<sup>7</sup> After 2007, troopers have the option of signing up for “Project Choice,” which would allow for pay raises based on performance, paid for by an additional three dollar vehicle registration fee, over and above their grade K wage. For the purposes of the current paper, we assume that, since all troopers were able to sign up for Project Choice, any impact of this program on citations issued should be constant across officers and across laws, and be subsumed in a time fixed effect. It is not obvious how much take-up of Project choice there is, as the actual pay of ISP employees is within 50 cents of the policy wage (IHR 2012).

state statute that a motorist was accused of violating, the date of the citation, where the citation occurred, and the current rank and badge number of the primary officer who issued the citation. We were able to link the officer identifying information with Human Resource records of the ISP, indicating the date that the officer was hired by the ISP, and the date that the officer achieved his or her current rank. For most officers who are currently state troopers, the date they were hired is the same as the date they became a trooper. Out of 153 officers in our sample, 39 were officially hired as commissioned officers before achieving the rank of troopers.

We then employed four research assistants to document the history of all of the 287 laws that the troopers in our data referenced more than ten times when citing citizens. We recorded every instance in which the legislatively recommended penalty increased (47 times) and every instance in which an amendment changed the scope of a law (44 times). Of the changes in scope, twenty-two increased the scope of the law by, for example, lowering the speed limit in business districts, and the other twelve changes reduced the scope of the law by, for example, amending a ban on loaded firearms in vehicles to allow for hunting rifles. All amendments to the Idaho statute go into effect on July 1<sup>st</sup> of each year.

After merging the citation database with information on the history of the Idaho statutes, we then calculated how many times each officer issued a citation for each individual law in each six-month period, beginning in January of 2002. We then calculated two measures of trooper experience, for each law, for each half-year interval: Starting at one, how many half-years of experience did the trooper have with the ISP? This is our measure of job-specific human capital, which is officer-time specific. We also calculate a measure of task-specific experience; in each period, how much experience each officer has with the current vintage of each particular law. If a law has not changed since the officer was hired, their task-specific and job-specific experience

measures are the same. When a law changes, the amount of task-specific experience an officer has with that law falls back to one.

*< Table 1 about here >*

We therefore have a total of 539,560 officer-law-half-year observations in our final data set. On average, each officer uses a given law 0.596 times, or just over once a year. Not surprisingly the distribution is highly skewed, with speeding in particular being used frequently by some officers. Just over 28% of the enforced laws have changed since a trooper was hired, and on average, troopers have 7.26 years of experience with the ISP, but 6 years of experience with a given law. The overall distribution of trooper experience is bimodal, as there are essentially two groups of troopers; those who are senior officers with over 10 years of experience, and junior troopers, who are likely to be promoted to higher ranks within the ISP. It is plausible that heterogeneity in unobserved characteristics across these two types of officers will affect their productivity and response to law changes, and so we will present some results for these groups separately.

*< Figure 1 about here >*

In Figure 1, we plot the average number of times an officer uses a given law, against their experience as a trooper and their experience with a given law. For ease of comparison, we focus just on junior officers, who we observe with the same amount of job and task specific experience. For the first two years on the force, troopers issue more tickets each period. After the end of the second year, the number of citations issued by each officer declines somewhat, and a trooper with 10 years of experience uses each law about 30% less frequently than a second-year trooper. We then compare the average use of each law relative to a troopers experience with that law. This curve is flatter, starting at a higher value, and peaking at a lower value, than

job-specific experience. The number of times that an officer uses a law does not decline as much as task-specific experience increases, implying that some of the reduction seen in general measure is the result of officers responding to legal changes.

#### 4. Analysis and Results

On average, the number of citations issued by police officers appears to be roughly lognormal in general tenure. Troopers experience a steep “learning curve” during the first two years of their career, and the average number of tickets is roughly stable afterward, with perhaps a slight decline over time. When we look at the response of troopers to changes in their tasks, we also see a clear learning curve, where citations increase for a year but then level off quickly, and even decline 18 months after the change.

Our baseline specification will impose a parametric structure on this learning curve, and simply allow for general and task-specific human capital to make separate contributions to a trooper  $i$ 's use of law  $v$  in semester  $t$  as follows:

$$\ln(Citations_{ivt} + 0.01) = \alpha_i + \tau_t + \delta_{vT} + \ln(Tenure_{it})\beta_G + \ln(LawTenure_{itv})\beta_{TG} + \varepsilon_{ivt}$$

where  $Tenure_{it}$  is the number of semesters that trooper  $i$  has worked for the ISP in semester  $t$ , and  $LawTenure_{itv}$  is the number of semesters that this same officer has used the version of law  $v$  that is in effect in semester  $t$ . If a law has not changed since the officer was hired, both of these variables are exactly the same. However, if a law is revised, the officer will have less experience enforcing that particular law than he has as a trooper more generally. Our officer fixed effects,  $\alpha_i$ , mean that we are looking at changes in the productivity of each individual officer, and our semester fixed effects  $\tau_t$  allow us to abstract from any general trends in citation behavior over time that are common to all officers and all laws. We also include a “statute vintage” fixed

effect,  $\delta_{vT}$ , that identifies each version of each law in the Idaho State Statute. These statute vintage effects net out any behavioral change on the part of Idaho citizens. For example, one might expect that increasing the penalty associated with say, drunk driving, might deter some citizens from driving drunk. Similarly, if the highway speed limit is lowered in business districts, then we might expect more speeding tickets to be issued. Our identification of general and task specific human capital is based on the assumption that these first order effects of the law change should be common to all police officers. Increasing the scope of a law should cause all officers to issue more tickets, and so we are identifying  $\beta_{TG}$  off of the change in the number of times an officer issues tickets for that statute within law vintage and the differential response of officers who were hired before and after the law change. In our primary specification, we will allow for arbitrary correlation in the unobserved determinate of law use at the law level ( $v$ ), although we will also show a subset of our results with standard errors clustered at the officer level ( $i$ ).

*< Table 2 about here >*

As shown in table 2, we find that, on average, a trooper with 10% more experience will issue 3% more tickets than his less experienced colleague. This result is robust to the inclusion of separate violation and semester fixed effects (columns 1 and 2) or statute by vintage fixed effects (column 3) which is our preferred specification. Note that this result is estimated to be statistically different from zero if we cluster our standard errors at the officer level (column 1) or at the law level (all other columns), and that our standard errors are roughly 15% smaller when we cluster at the officer level.

In columns 4 – 6 of table 2, we separately identify the impact of general and task-specific experience. When we do not allow for a general impact of the law change on citizen behavior (column 4), we cannot reject the null hypothesis that task-specific experience is unrelated to

productivity. However, once we absorb law by vintage effects, we estimate that a 10% increase in the amount of experience that a trooper has with a given law is associated with a 0.4% increase in the amount of times he uses it. Additionally, when we include officer by stature fixed effects in our analysis (columns 4 and 6), our estimates examine how officers choose to enforce different laws differently. In other words, our task specific experience effects are now identified entirely off of laws that change during our sample period, as opposed to laws that changed prior to our sample period. When we include these effects, we find our first evidence that officers might be averse to changes in laws, as the coefficient on task-specific experience is both negative and statistically significant.

In table 3 we exclude laws that changed in ways to reduce the scope of the law or increased deterrence. The relationship between general experience and productivity remains roughly constant across these samples, but we find that the average impact of task specific experience is driven by changes in the law that explicitly affect the amount of work a trooper has to do - change that affects the number of things that are illegal. When the scope of a law either increases (column 1-4) or decreases (columns 5-8), we estimate that a 10% increase in experience enforcing the current law is associated with a 1.5 – 2.8% increase in the number of times a trooper uses it. The fact that both general and task-specific effects are positive suggests that troopers are simply less likely to issue citations for laws they are less familiar with, rather than a principle agent problem where officers are behaving in a way that counteracts the intended impact of the legislature. If the Idaho legislature increases the penalty associated with a given crime (columns 9-12), we do not find evidence that this change affects trooper's behavior, and in fact receiving notification that the law has changed may increase the number of times a trooper uses it.

*< Table 3 about here >*

In table 4, we allow for the impact of a law change to vary with officer experience. We do this in two ways. First, (column 1) we simply include a continuous interaction between general and task-specific experience. This variable is consistently negative, and significantly different from zero, suggesting that as troopers become more experienced, their productivity is less dependent on their experience with the particulars of a given law. When we include officer by statute fixed effects (column 2) the interaction term becomes larger, indicating that officer productivity is less affected by their experience with the current vintage of a law as the officer gains experience.

Recall that there is a natural division in the troopers we observe, where a handful of troopers were hired in the 1980s, and have over 10 years of experience by the time our sample period begins. An implication of this is that the officers identifying the marginal impact of both general and task specific human capital varies across the ranges of experience. In the remaining columns of table 4, we divide our sample into “junior” and “senior” troopers, based on whether or not we observe this officer with more than 10 years of general experience. We find that both junior and senior officers – in our preferred specification (columns 6 and 10) - use a law approximately 3.5% more with a 10% increase in experience with that law, but that, after troopers have been on the job for more than 10 years, additional time on the job is associated with fewer citations overall. This is potentially due to negative selection, as senior troopers, by definition, have not been promoted or have chosen to move to a higher ranking position. Thus, the source of variation in enforcing different laws differently (inclusion of officer by statute fixed effects) appears to come from differences in the tenure of the officer.

*< Table 4 about here >*

A similar pattern arises when we focus systematically on different types of law changes; junior officers are driving the positive relationship between task-specific experience and productivity. As seen in table 5, junior officers increase productivity as they gain experience as an officer whereas senior officer's productivity decreases. When we focus our analysis on laws that changed more than once during our sample period and include officer by statute fixed effects (columns 3, 6, 9), the effect of experience on productivity for junior officers becomes both more pronounced and statistically significant. On the other hand, this narrowed analysis has almost no impact on senior officers. Thus, we find that younger officer's tend to enforce legal changes more aggressively with increased experience as more senior officer's decrease productivity as they gain experience and laws change. Moreover, this trend apparent for all types of changes in laws.

*< Table 5 about here >*

In table 6, we include an interaction variable for trooper experience with the trooper's experience with the current law. The results are quite telling, as junior officers have higher productivity with the current vintage of the law, but their productivity dissipates as they gain experience as an officer. Conversely, senior officers appear to decrease productivity with experience, but see small increases in productivity with the current vintage of the law.

When we focus our analysis on laws that change more than once during our sample period (columns 3, 6, 9), our results echo the results when we include the full sample. In fact, our point estimates become more precise and highlight the fact that increases in productivity occur when a junior officer gains experience, but their productivity appears to exhibit diminishing productivity.

*< Table 6 about here >*

State troopers issue more than seven times as many citations for violating traffic laws (Idaho Statute Sections 49) as they do for criminal offenses (Idaho Statute Sections 18 and 19)- 36,000 vs. 283,165 tickets in our sample. Since issuing traffic citations are a larger part of a trooper's daily routine, it is possible that they adjust to changes in traffic code in a different way than the criminal code. In table 7, we allow for the general and task-specific learning curves to vary for these two types of offenses.<sup>8</sup> Here, we find that general trooper experience appears to affect the number of tickets that troopers issue, regardless of the type of violation. However, changes in criminal law do not appear to have a statistically identifiable impact on a trooper's use of that law, relative to traffic laws. In fact, the sign of the imprecise coefficient suggests that troopers actually become less likely to issue citations for a criminal law as they become more familiar with it.

*< Table 8 about here >*

## **5. CONCLUSION**

More experienced workers are, for the most part, more productive workers. Based on our analysis of Idaho state troopers, we conclude that the return to task-specific experience is a decreasing function of job-specific experience. The substitutability of task-specific and job-specific experience is evident on a number of dimensions. For young troopers, task specific experience is at least, if not more, important for output as their general experience. Changes in the specifics of the laws do not impact the output of older troopers output as much, if at all. We also find that within trooper, as job experience increases the "learning curve" associated with a legislative change flattens. This is most evident for law changes that directly affect the type of behavior that is illegal, and we find no evidence of asymmetric responses, in the sense that law

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<sup>8</sup>We do observe troopers issuing citations for violations of other parts of the State Statute, especially health code violations. We exclude these types of violations from these regressions.

changes that decrease the scope of the law appear to reduce citations as much as changes which increase the number of actions that are illegal. Legislative changes that increase the recommended penalties also temporarily reduce officer enforcement, but this response is muted compared to changes that directly affect the legality of certain behaviors.

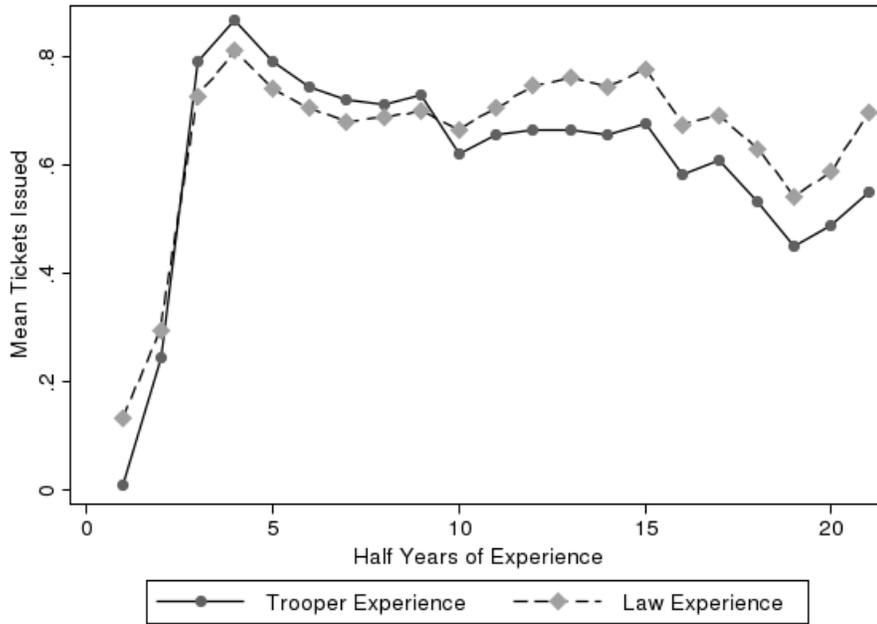
Our finding that task-specific experience and job-specific experience, as well as the magnitudes of the impacts of both types of experience on productivity, builds directly on a small literature in labor economics. The fact that we observe this relationship between tenure and output for law enforcement officers is of particular relevance for empirical law and economics. The sequential nature of decision making in the decentralized U.S. criminal justice system means that policy changes put in place by the legislature may not be implemented in practice. While persistent judicial discretion in the face of legislative changes has been documented at both the federal and state level, to the best of our knowledge, we present some of the first evidence of systematic patterns in the way officers choose to enforce the law. Unlike research on judges, which typically focuses on judicial beliefs about punishment, we find that police officers are less likely to enforce laws they are less familiar with, regardless of whether the law has become more or less punitive.

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Figure 1: Mean Tickets Issued per Month, by Job and Law-Specific experience



Note: Junior Officers Only

Table 1: Semi-Annual Experience and Citations Issued by Idaho State Troopers, 2002-2009

	Mean	Standard Deviation	Min	Max
<i>All Troopers (n=539,560)</i>				
Citations Issued	0.596	8.06	0	735
Trooper Experience	14.5	12.0	1	55
Statute Experience	12.0	10.6	1	55
Statute has more Deterrence?	0.164			
Statute has Larger Scope?	0.077			
Statute has Smaller Scope?	0.042			
<i>Junior Troopers (n=351,288)</i>				
Citations Issued	0.628	8.61	0	735
Trooper Experience	8.09	5.06	1	21
Statute Experience	7.20	4.83	1	21
Statute has more Deterrence?	0.164		0	1
Statute has Larger Scope?	0.077		0	1
Statute has Smaller Scope?	0.042		0	1
<i>Senior Troopers (n=188,272)</i>				
Citations Issued	0.535	6.91	0	475
Trooper Experience	26.6	12.0	7	55
Statute Experience	21.1	12.3	1	55
Statute has more Deterrence?	0.164			
Statute has Larger Scope?	0.077			
Statute has Smaller Scope?	0.042			

Note: "Junior" and "Senior" troopers are defined by their maximum trooper experience in our sample. All Senior troopers are observed with more than 10 years of experience (21 half-year periods)

Table 2: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Experience as Trooper)	0.283** [0.0304]	0.283** [0.0350]	0.283** [0.0350]	0.283** [0.0366]	0.242** [0.0370]	0.349** [0.0568]
Ln(Experience with Current Statute)					0.0393* [0.0166]	-0.063* [0.0298]
R <sup>2</sup>	0.362	0.362	0.367	0.056	0.367	0.560
N	536690	536690	536690	536690	536690	536690
Officer Fixed Effects	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x
Statute x Vintage Fixed Effects			x	x	x	x
Officer x Statute Fixed Effects				x		x
Cluster Level	Officer	Statute	Statute	Statute	Statute	Statute

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the indicated level of clustering. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes)

+ =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$

Table 3: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers by Type of Law Change, 2002-2009

	Increase Law Scope				Decrease Law Scope				Increase Penalties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Ln(Experience as Trooper)	0.277**	0.192**	0.223*	0.278**	0.266**	0.154**	0.180	0.256*	0.249**	0.224**	0.290**	0.311
	[0.0413]	[0.0536]	[0.101]	[0.0922]	[0.0379]	[0.0593]	[0.118]	[0.104]	[0.0346]	[0.0347]	[0.0544]	[0.0554]
Ln(Experience with Current Statute)	0.00872	0.0926*	0.0621	0.007	0.00574	0.117*	0.091	0.151	0.00385	0.0278	-0.035	-0.050
	[0.0175]	[0.0466]	[0.086]	[0.753]	[0.0211]	[0.0592]	[0.107]	[0.091]	[0.0133]	[0.0193]	[0.0327]	[0.0329]
R <sup>2</sup>	0.361	0.363	0.560	0.555	0.31	0.312	0.519	0.518	0.309	0.313	0.519	0.518
N	420750	420750	420750	430100	409530	409530	409530	417010	480590	480590	480590	488070
Officer Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Statute x Vintage Fixed Effects		x	x	x		x	x	x		x	x	x
Officer x Statute Fixed Effects			x	x			x	x			x	x

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the statute level. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes). Columns 4, 8, and 12 include laws that change more than once.

+ =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$

Table 4: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers by General Experience, 2002-2009

	All		Junior				Senior			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln(Experience as Trooper)	0.122** [0.0281]	0.060 [0.056]	0.343** [0.0315]	0.480** [0.087]	0.197** [0.0413]	0.187+ [0.099]	-0.302 [0.227]	-0.294** [0.066]	-0.300** [0.0433]	-0.341** [0.0755]
Ln(Experience with Current Statute)	0.287** [0.0415]	0.413** [0.080]	0.0286* [0.0109]	-0.105 [0.0573]	0.289** [0.0549]	0.339** [0.124]	0.019 [0.0149]	0.012 [0.043]	0.0243 [0.0678]	0.372* [0.180]
Ln(Experience as Trooper) x Ln(Experience with Current Statute)	-0.0738** [0.0113]	-0.111** [0.017]			-0.104** [0.0204]	-0.140** [0.033]			-0.00146 [0.0159]	-0.0688* [0.0322]
R <sup>2</sup>	0.367	0.561	0.379	0.569	0.379	0.569	0.360	0.565	0.360	0.565
N	536690	539560	348418	348418	348418	348418	188272	188272	188272	188272
Officer Fixed Effects	x	x	x	x	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x	x	x	x	x
Statute x Vintage Fixed Effects	x	x	x	x	x	x	x	x	x	x
Officer x Statute Fixed Effects		x		x		x		x		x

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the statute level. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes)

+ =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$

Table 5: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers by Experience and Type of Law Change, 2002-2009

	Increase Law Scope			Decrease Law Scope			Increase Penalties		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Junior Officers									
Ln(Experience as Trooper)	0.269** [0.0752]	0.378* [0.188]	0.452** [0.165]	0.227* [0.0895]	0.350 [0.219]	0.447* [0.192]	0.311** [0.0455]	0.377** [0.0941]	0.424** [0.095]
Ln(Experience with Current Statute)	0.103 [0.0648]	-0.004 [0.171]	-0.077 [0.146]	0.130 [0.0841]	0.009 [0.203]	-0.088 [0.174]	0.0215 [0.0230]	-0.043 [0.072]	-0.082 [0.072]
R <sup>2</sup>	0.376	0.567	0.565	0.330	0.531	0.531	0.329	0.530	0.530
N	273150	273150	279220	265866	265866	270722	311998	311998	316854
Panel B: Senior Officers									
Ln(Experience as Trooper)	-0.269** [0.0600]	-0.374** [0.076]	-0.346** [0.075]	-0.244** [0.0671]	-0.363** [0.088]	-0.356** [0.080]	-0.250** [0.0502]	-0.269** [0.0658]	-0.251** [0.070]
Ln(Experience with Current Statute)	-0.00754 [0.0409]	0.105* [0.052]	0.071 [0.055]	-0.0284 [0.0504]	0.102 [0.070]	0.093 [0.060]	-0.0028 [0.0265]	0.0182 [0.0429]	-0.006 [0.049]
R <sup>2</sup>	0.353	0.560	0.557	0.297	0.516	0.514	0.300	0.517	0.516
N	147600	147600	150880	143664	143664	146288	168592	168592	171216
Officer Fixed Effects	x	x	x	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x	x	x	x
Statute x Vintage Fixed Effects	x	x	x	x	x	x	x	x	x
Officer x Statute Fixed Effects		x	x		x	x		x	x

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the statute level. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes). Columns 3, 6 and 9 include laws that change more than once. + =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$

Table 6: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers by Experience and Type of Law Change, 2002-2009

	Increase Law Scope			Decrease Law Scope			Increase Penalties		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Junior Officers									
Ln(Experience as Trooper)	-0.005 [0.0683]	-0.106 [0.222]	0.208 [0.199]	-0.0648 [0.0906]	-0.156 [0.270]	-0.010 [0.248]	0.134** [0.0358]	-0.008 [0.103]	0.772 [0.113]
Ln(Experience with Current Statute)	0.556** [0.109]	0.686** [0.251]	0.538* [0.232]	0.597** [0.125]	0.710** [0.295]	0.545* [0.276]	0.328** [0.0606]	0.524** [0.132]	0.427** [0.142]
Ln(Experience as Trooper) x Ln(Experience with Current Statute)	-0.174** [0.0340]	-0.203** [0.041]	-0.181** [0.041]	-0.173** [0.0330]	-0.195 [0.040]	-0.174** [0.042]	-0.123** [0.0230]	-0.177** [0.033]	-0.157** [0.034]
R <sup>2</sup>	0.377	0.567	0.565	0.33	0.532	0.531	0.329	0.531	0.530
N	273150	273150	279220	265866	265866	270722	311998	311998	316854
Panel B: Senior Officers									
Ln(Experience as Trooper)	-0.280** [0.0589]	-0.442** [0.087]	-0.421** [0.861]	-0.264** [0.0657]	-0.434** [0.099]	-0.445** [0.096]	-0.267** [0.0488]	-0.349** [0.0765]	-0.334** [0.077]
Ln(Experience with Current Statute)	-0.0635 [0.171]	0.642** [0.190]	0.558** [0.180]	-0.144 [0.226]	0.684* [0.207]	0.637** [0.204]	-0.0621 [0.0908]	0.422** [0.154]	0.431 [0.156]
Ln(Experience as Trooper) x Ln(Experience with Current Statute)	0.0154 [0.0416]	-0.106** [0.0316]	-0.092** [0.030]	0.0313 [0.0547]	-0.115** [0.033]	-0.102** [0.033]	0.0163 [0.0210]	-0.075** [0.0270]	-0.081** [0.029]
R <sup>2</sup>	0.353	0.560	0.557	0.297	0.516	0.514	0.3	0.517	0.516
N	147600	147600	150880	143664	143664	146288	168592	168592	171216
Officer Fixed Effects	x	x	x	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x	x	x	x
Statute x Vintage Fixed Effects	x	x	x	x	x	x	x	x	x
Officer x Statute Fixed Effects		x	x		x	x		x	x

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the statute level. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes). Columns 3, 6, and 9 include laws that change more than once. + =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$

Table 7: Fixed Effects Estimates of General and Task-Specific Experience and Productivity for Idaho State Troopers, by type of Law, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Experience as Trooper)	0.260** [0.0310]	0.260** [0.0346]	0.261** [0.0347]	0.259** [0.572]	0.257** [0.0386]	0.322** [0.0719]
Ln(Experience as Trooper) x Traffic Law	0.0342** [0.00939]	0.0342** [0.00911]	0.0327** [0.00926]	0.0357 [0.073]	-0.0313 [0.0327]	0.0497 [0.108]
Ln(Experience with Current Statute)					-0.00384 [0.0165]	-0.0578** [0.0219]
Ln(Experience with Current Statute) x Traffic Law					0.0734* [0.0352]	-0.0162 [0.0542]
R <sup>2</sup>	0.365	0.365	0.37	0.563	0.37	0.563
N	529210	529210	529210	529210	529210	529210
Officer Fixed Effects	x	x	x	x	x	x
Semester Fixed Effects	x	x	x	x	x	x
Statute Fixed Effects	x	x	x	x	x	x
Statute x Vintage Fixed Effects			x	x	x	x
Officer x Statute Fixed Effects				x		x
Cluster Level	Officer	Statute	Statute	Statute	Statute	Statute

Standard errors in brackets allow for arbitrary correlation in Ln(Citations+0.01) at the indicated level of clustering. All regressions include controls for Ln(Region Population), the fraction of the population that are males between 18 and 24 years old, and Ln(Vehicle Crashes)  
 + =  $p < 0.10$ , \* =  $p < 0.05$ , \*\* =  $p < 0.01$