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Occupational Licensing and Minorities: A Reply to Klein, Powell, and Vorotnikov

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[LINK TO ABSTRACT](#)

Introduction

In May 2009, the *Journal of Law and Economics (JLE)* published our article titled “Effects of Occupational Licensing Laws on Minorities: Evidence from the Progressive Era.” In that article, we investigated the impact of the adoption of state-level occupational licensing regulation on the participation of minority workers in a range of skilled and semi-skilled occupations. Specifically, we took advantage of quasi-experimental variation, afforded by the fact that different states adopted occupational licensing regulation at different times, to identify the effect these laws had on the prevalence of female and black workers in eleven different occupations using a differences-in-differences framework. We found that the adoption of these laws did not reduce minority participation in most occupations. In fact, for many occupations, we found that the adoption of licensing laws was correlated with increases in minority participation. We argued in our article that the evidence presented was generally inconsistent with the received wisdom, which claims that licensing laws reduced minority participation in most occupations. Instead, the evidence is more supportive of an alternative hypothesis that posits

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that licensing may help minorities, particularly in occupations for which information about worker quality is difficult to determine.

In “Was Occupational Licensing Good for Minorities? A Critique of Marc Law and Mindy Marks,” Daniel Klein, Benjamin Powell, and Evgeny Vorotnikov (henceforth KPV) take issue with our findings. They argue that our study is “rife with problems” (KPV 2012, 228). Among other things, they believe that: (i) the data we use (individual-level census returns) are inappropriate for addressing the question because of imperfections in how licensing laws were enforced; (ii) there is sample selection bias in the set of occupations we have chosen to analyze; (iii) several of our findings are based on small numbers of minority workers; (iv) we have conflated licensing and certification in some instances; (v) there is measurement error in how we measure the timing and restrictiveness of occupational licensing; and (vi) the omission of controls for interstate migration and craft unionization bias our results in favor of our findings. KPV therefore conclude: “[I]here is no reason to abandon the conventional view that licensure generally harms minorities” (2012, 229).

We welcome this opportunity to join KPV in a discussion of our study. However, after reviewing their arguments, we remain convinced that our study properly identifies the effect of licensing on minority participation during the period under investigation. While we concede that the interpretation of our evidence may be slightly altered for some occupations (where, we agree, that we have lumped together licensing and certification), and that the nature of the data make it difficult to identify the precise channel(s) through which licensing impacts minority participation, KPV’s criticisms do not definitively show that our methodology is biased in favor of our findings. In fact, nowhere in their critique do KPV show that any of our original regression findings are altered by the inclusion of new variables or changing variable definitions. Indeed, many of KPV’s concerns about measurement error bias our empirical strategy *against* finding a positive effect of licensing on minority participation, thereby strengthening our original claims.

Before proceeding with our rebuttal, we feel we should make a few disclosures of a more personal nature. First, as empirical economists, we did not have strong priors regarding the impact that licensing laws should have on minority participation in various occupations. Different hypotheses have different predictions regarding the direction of impact. An important task of empirical scholarship in economics is to determine the effect that policy has on economic outcomes as objectively as possible. This was our only goal in conducting this study. Our preference is simply to allow the data to speak and to base our conclusions on the data. Second, while we are interested in knowing the direction of effect, we have no stake in the outcome of this debate with respect to current policy. We therefore take issue with KPV’s claim that we are in favor of licensing

today (see KPV 2012, 222 n. 10, which claims that we give policy advice today based on our findings). Nowhere in our paper (nor in our other writings) do we ever argue that licensing regimes should be extended. In our conclusion (Law and Marks 2009, 364) we did suggest that licensing may help minority workers signal quality in some cases, but this hardly amounts to an argument in favor of current licensing regimes. We therefore ask KPV not to make inferences regarding our views about policy from our paper. Third, the fact that licensing may not have been very harmful on net for minority participation during the Progressive Era does not mean that it has not been harmful at other times and in other places or along other dimensions or that some minority workers were not harmed.³ Our study only addresses the impact of licensing within certain occupations over a given period. We can say nothing out-of-sample, and are well aware of other studies that have found harmful effects on minorities in other occupations and time periods (see, for instance, Federman, Harrington, and Krynski 2006).

Finally, the reader may be interested to know something of the background to this article. In March 2012 KPV submitted a critique of our 2009 article to the *JLE*. By request of the editor of the *JLE*, KPV also sent us a copy of their critique (cited hereafter as Vorotnikov et al. 2012). The following month we responded to KPV's critique and also sent a copy to the *JLE* (this reply is cited hereafter as Law and Marks 2012). The *JLE* subsequently rejected KPV's critique (which, we understand, was a revised version of the critique they initially sent to us). Since then, KPV have sent their critique to *Econ Journal Watch* (*EJW*). The present article is a revised version of our original response to KPV. We note that KPV have amended their critique to incorporate some of our replies. Accordingly, in addition to replying to each of the criticisms leveled in the *EJW* version of their critique, we will include a discussion of some of KPV's original criticisms.

KPV have organized their critique of our paper in four sections. The first section details problems that KPV have with our data. The second section outlines problems that KPV have with our analysis of the data. The third concerns alternative qualitative and historical evidence KPV believe we have ignored. The last section deals with the theoretical debate over licensing. We organize our response along the same lines.

3. Indeed, in an earlier article on the emergence of Progressive Era occupational regulation, one of us (along with Sukkoo Kim) wrote: "We hope that future scholars will take up the task to determine if and when licensing regulations became a tool to advance the narrow interests of professionals at the expense of the general public" (Law and Kim 2005, 754).

KPV's problems with the data

KPV make two claims about the why the data we use are inappropriate for investigating the effect of licensing on minority participation. The first is that census-reported practitioners in a licensed state are not necessarily licensed. Imperfections in how licensing laws are enforced may therefore bias our results because our data possibly include female and black practitioners who self-declare to be within an occupation, even though they are practicing without a license. KPV therefore, in the earlier version of their critique, accuse us of “assuming that licensing restrictions were perfectly enforced” (Vorotnikov et al. 2012, 3) and argue that our preferred interpretation of our results requires this assumption. Relatedly, they also present qualitative historical evidence suggesting that enforcement of licensing standards was weak during the period under investigation (especially for blacks) and that unlicensed practitioners often practiced in racially segregated markets. KPV's second claim is that the occupational licensing data we use are flawed. Specifically, they are concerned that the dates of initial licensing laws are measured with error. As evidence of this, KPV examine data on the occupational licensing laws for lawyers from other sources and show that the our primary data source on the timing of licensing laws contains errors with respect to the dates of initial licensing laws for the legal profession.

Is imperfect enforcement a source of bias?

Let us start with the first issue. It is true that the census does not identify whether or not an individual has a license. To our knowledge, there are no systematic data on the licensing status of individuals across a broad range of occupations during this period. Occupational status from individual census returns is self-declared. Accordingly, a person who declares herself a plumber will be coded as a plumber, regardless of whether or not the individual possesses a license to practice plumbing. We do not dispute this fact. The question for us is whether this makes it impossible to correctly identify the effect of licensing on minority participation in various occupations using census data. In other words, do imperfections in the enforcement of licensing laws render our empirical strategy invalid?

We believe that they do not. For one thing, if imperfect enforcement is to systematically bias our results, it is probably classical measurement error that biases our estimates toward zero (i.e., attenuation bias). KPV present no *systematic* evidence to suggest that imperfections in enforcement would be a source of non-

classical measurement error. The evidence they present—on plumbers in Maryland in the 1950s—is hardly fatal since it represents only a single occupation in a single year whereas our analysis covers eleven occupations in 48 states over seven census years.

Second, while KPV claim that we do not mention the fact that a practitioner in a licensed state is not necessarily licensed, the empirical strategy we used already acknowledges the possibility of imperfections in regulatory enforcement due to grandfathering. As a robustness check, we re-estimated all of our key regressions restricting the sample to young workers (see Tables 4 and 5 in Law and Marks 2009, 361-362). Licensing laws, when applied, are more likely to be binding (and hence, enforced) for young workers (who are new to a profession) than to older workers (who are incumbents and already have established themselves). The fact that we find similar results for young black and young female workers as for the whole sample suggests that imperfections in enforcement are not an important source of bias.

Finally, it is standard practice in the literature on the labor market effects of state-level public policy to use micro data from government sources like as the U.S. Census or the Current Population Survey. In all of these data sources, important variables like occupation and earnings are self-reported. Even when it is known that the state-level public policy is not perfectly enforced, researchers estimate models using a similar research design as ours. For instance, state-level minimum wages are not perfectly enforced. Enforcement may be uneven across different demographic groups, and yet economic analysis of the effects of minimum wages on labor market outcomes use empirical specifications that are, in spirit, much like ours.⁴ If we were to take KPVs criticism on this score seriously, we would also have to question the validity of this entire literature.

Accordingly, we maintain that our data set and empirical strategy allow us to correctly identify the effect of licensing on minority participation. Indeed, since our key empirical question is how state-level licensing affects minority labor force participation in a variety of occupations, the empirical model is the correct one, even if it is harder or impossible for minorities to gain a license. We agree, however, that the approach may not allow us to determine the precise causal mechanism. This is a common problem for quasi-experimental approaches to causal inference. Minority participation in some occupations may have increased as a result of licensing because of the signaling value of having a license, which is our interpretation of the evidence. On the other hand, minority participation may increase because licensing raises prices and creates a black market in which minority

4. See, for instance, Neumark and Wascher (2001) and Acemoglu and Pischke (2003).

workers are disproportionately represented. Our research design does not allow us to cleanly distinguish between these two alternatives.

That said, there are good reasons to be skeptical of the second interpretation. If licensing creates an illicit market that benefits minorities, why is it that we only observe a positive effect of licensing on minority participation in some occupations but not for others? *A priori*, it is not obvious why licensing should facilitate an illicit market for female engineers, plumbers, and pharmacists as well as for black teachers and doctors, but not for black barbers or beauticians. In fact, our results show that black participation in the barbering profession was reduced by occupational regulation of barbers, which raises the question of why black barbers were unable to operate underground but black teachers and doctors were? Our interpretation, we believe, is more consistent with the pattern of positive and negative results (i.e. licensing increased minority participation in professions where practitioner quality was harder to ascertain and where statistical discrimination more likely, and licensing reduced participation in occupations where quality was not hard to ascertain and where minority workers were a viable competitive threat), but we agree that the evidence in favor of this interpretation is not definitive.

Finally, KPV argue that our interpretation hinges on perfect enforcement. We disagree with this claim. In order for licensing to reduce statistical discrimination it must be the case that *some* potential customers value the information provided by a license and that *some* minorities be able to obtain one. No assumption of perfect enforcement is required.

Are the data on when occupational licensing laws were adopted flawed?

KPV's evidence for this claim is based on their analysis of a *single* occupation. They present no evidence showing that the data on when other occupations became regulated are systematically misreported. In fact, we excluded lawyers—the one profession for which they show that the data on licensing are in error—precisely because the quality of the data was weak (information on the timing of licensing was reported as unknown for 19 states in the Council of State Governments' 1952 study). This significantly lessens the force of the critique.

Nonetheless, we recognize that data on the timing of licensing laws for the occupations that we do examine may be recorded with error in the Council of State Governments' publication. Once again, the key question for us is whether this source of measurement error systematically biases our findings. In order for measurement error in the timing of licensing laws to bias our results, it would have to be correlated with trends in minority representation within an occupation over time. There are no reasons to believe that this is the case, nor do KPV present

any data to show this. Hence, we remain skeptical that this is an important source of systematic bias. Indeed, random measurement error in the reported timing of licensing, which is likely to be the relevant kind of measurement error in this context, will cause attenuation bias, making it *harder* to find that licensing has a statistically significant effect.

Nevertheless, our study has two features that help us deal with possible measurement error over the timing of licensing laws. First, we excluded from analysis any state for which the year in which licensing was adopted was unknown. Second, because we use census data (which are reported every decade), our empirical strategy allows for mis-measurement within a census decade.

KPV's problems with our analysis

In Table 5 (KPV 2012, 229), KPV list six additional problems with our empirical analysis.⁵ The first concerns which occupations were included for analysis. The second is that we have in some cases conflated licensure with certification. The third is that we do not include measures of how restrictive licensing regimes were. The fourth is that we include one occupation (teachers) where most employment was by the public sector. The fifth is that it is hard to interpret the results for occupations like nursing, where most workers were women. The sixth is that there is omitted variable bias because we do not control for the presence of craft unions or trends in interstate migration.

Occupations covered in our study

KPV have three complaints about the sample of occupations that we analyze in our study. First, KPV believe that our requirement that at least one percent of an occupation had to be either female or black to be included in our analysis biases our overall findings because we excluded occupations for which there are very small numbers of minority workers. They argue that are the requirement that at least one percent be back or female “could well have selected certain occupations out of the study *because* licensing was so strongly discriminatory.” (KPV 2012, 218). Second, in the earlier version of their critique, KPV argue that there are other occupations that we could have included but we did not. Using our criteria for selection, KPV argued that dentists, insurance brokers, and real estate agents should also have

5. Interestingly, KPV only have problems with the occupations for which we find that licensing had a positive effect on minority participation. They seem untroubled by our analysis of barbers, where we find a negative effect using the same empirical methodology.

been included (Vorotnikov et al. 2012, 9). The implicit suggestion is that we have cherry-picked those occupations we included in order to bias our results in favor of finding a positive impact of licensing on minority participation. For reasons that are undisclosed, KPV have chosen to omit this particular criticism in their updated critique, perhaps because we dealt with it successfully in our original reply (Law and Marks 2012, 10). Third, KPV argue that because minority participation rates in many of the occupations that we studied were very low and close to the one-percent threshold, our results are spurious (KPV 2012, 219).

There is an inherent contradiction in KPV's first and third criticisms. On the one hand they claim that our one-percent requirement results in too many occupations being excluded. On the other hand, they say that our one-percent requirement results in too many occupations being included. KPV seem to want to have their cake and eat it too. While we agree that it is hard to know what the appropriate participation cutoff should be, and we acknowledge there is a tradeoff, the fact of the matter is that some cutoff must be chosen. For many of the occupations for which we have data, minority participation was very low during this period, rendering statistical analysis problematic. KPV provide no remedy, nor do they definitively show that our choice of a one-percent cutoff systematically biases our results in favor of finding that licensing increases minority participation.

Now to KPV's second concern, which, as noted, is not mentioned in the current version of their critique. First, let us state most emphatically that we *did not* cherry-pick which occupations to examine. Our only goal was to find a sample of occupations that represented a broad range of skills and for which there was enough minority participation to conduct an empirical analysis. As we write in the introduction to this reply, we had no strong priors about whether minority participation should be harmed or helped by occupational regulation, nor do we have any stake in the outcome of this debate. While eleven occupations do not cover all licensed occupations, they still constitute a non-trivial share of the non-agricultural share of the labor force during this time (six percent—see Law and Marks 2009, 352). Indeed, there are very few studies of the effects of licensing on minorities that examine more than one or two occupations at a time. Accordingly, our coverage is broader than most.

Let us now deal with the three occupations that KPV claimed that we overlooked. We excluded dentists because over the six census years for which we have data on dental licensing laws (1880 through 1940, excluding 1890, for which there are no IPUMS data), there were only 32 black and 37 female dentists (out of a total of 2,055 dentists). For what it is worth, when we do estimate the effect of licensing on black and female participation in dentistry using identical regressions as for the other occupations, the effects of licensing are not statistically significant.

OCCUPATIONAL LICENSING AND MINORITIES

Insurance brokers and real estate agents are two occupations that we could have included in the original paper but did not. Our failure to include these occupations was an oversight on our part, and we thank KPV for pointing this out. Accordingly, we estimated the effects of licensing of these two occupations on minority participation using the same empirical model that we used in our original study. Regression results are shown in Table 1. The coefficient of interest is the interaction between the minority and licensing indicator variables. For insurance agents, the adoption of licensing did not have a statistically significant effect on either female or black participation, while for real estate agents, licensing had a positive and statistically significant effect on female and black participation. Hence, if we had included these occupations in our original study we would have found more evidence in favor of the view that licensing did not generally harm minority participation and in some cases even helped.

TABLE 1. Effect of occupational licensing on minority workers, by occupation

| | Insurance Agents: Black | Insurance Agents: Female | Real Estate Agents: Black | Real Estate Agents: Female |
|--|----------------------------|-----------------------------|------------------------------|-------------------------------|
| Licensing indicator | -.048 (.039) | -.047 (.039) | -.012 (.042) | -.018 (.041) |
| Black x Licensing | -.053 (.011) | | .376** (.054) | |
| Black | -.520** (.078) | | -.712** (.071) | |
| Female x Licensing | | -.034 (.070) | | .204** (.041) |
| Female | | -.550** (.052) | | -.473 (.039) |
| Sample size | 963,836 | 963,836 | 1,874,558 | 1,874,558 |
| State-years | 294 | 294 | 240 | 240 |
| Years included | 1870-1940 | 1870-1940 | 1910-1950 | 1910-1950 |
| Minority representation | 240 (4.99%) | 240 (4.99%) | 108 (2.25%) | 379 (7.88%) |
| <small>Note. Each column contains a separate regression. State and year fixed effects and individual- and household-level controls (age, gender, race, literacy, urban residence, domestic, married, widowed, children, two families, three families, at school) are included when available. Robust standard errors, clustered at state level, are in parentheses. * and ** denote statistical significance at the 5- and 1-percent levels, respectively. IL, MI, NC, and UT were excluded from the insurance agent sample due to missing licensing data. Minority representation has percentage of the occupation that is minority (black or female) in parenthesis as well as number of workers. Information on the introduction of state licensing laws is from the Council of State Governments (1952).</small> | | | | |

Licensing vs. certification

KPV claim that there are four occupations we study for which we have conflated licensure and certification: registered and practical nurses, teachers, and engineers. We agree with KPV that for some occupations we have conflated certification with licensure. Because the relevant table from the Council of State

Governments study was titled “Occupations Licensed by State and Dates of Licensing Statutes,” we assumed that these referred to the dates at which states adopted mandatory licensure. However, our overall conclusion—that licensing aids minority representation in some cases—is not altered by this fact for three of the four occupations.

First, consider teachers. KPV write that “in no states were people required to have a license to teach in a *private* school” (2012, 219, emphasis added). KPV furnish no evidence, however, to show that licensing was not in place for *public* school teachers. Later on, KPV (2012, 221) mention that in 1919-20, 93 percent of teachers worked in public schools. Accordingly, it is possible that in fact the vast majority of teachers were subject to licensure.

TABLE 2: Effect of occupational licensing on minority workers in nursing

| | Registered Nurses: Black | Practical Nurses: Black | Registered Nurses: Female | Practical Nurses: Female |
|---|--------------------------|-------------------------|---------------------------|--------------------------|
| Licensing indicator | .006 (.021) | -.055 (.040) | .072 (.060) | -.153 (.095) |
| Black x Licensing | .281* (.103) | .012 (.081) | | |
| Black | -.424** (.033) | .270** (.056) | -.291** (.076) | .274** (.044) |
| Female x Licensing | | | -.050 (.076) | .108 (.081) |
| Female | 1.510** (.040) | 1.109** (.056) | 1.655** (.049) | 1.077** (.069) |
| Sample size | 1,196,523 | 1,165,341 | 1,196,523 | 1,165,341 |
| State-years | 96 | 82 | 96 | 82 |
| Years included | 1950-1960 | 1960-1970 | 1950-1960 | 1960-1970 |
| “Minority” representation | 505 (5.03%) | 860 (19.78%) | 10,036 (97.79%) | 4,165 (95.81%) |
| <p>Note. Each column contains a separate regression. State and year fixed effects and individual- and household-level controls (age, age squared, gender, race, education, size of metro area, domestic, married, widowed, number of children) are included. Sample is restricted to those in the labor force. Robust standard errors, clustered at state level, are in parentheses. * and ** denote statistical significance at the 5- and 1- percent levels, respectively. AR, CA and MT had licensing in place at the beginning of the sample and were excluded from the registered nursing analysis. AK, AR, CO, CT, FL, ID, LA, NV, NY and RI had licensing in place at the beginning of the sample and were excluded from the practical nursing analysis. “Minority” representation has percentage of the occupation that is minority (black or female) in parenthesis as well as number of workers. Information on the timing of licensure is from Monheit (1982).</p> | | | | |

Second, let us address the two nursing professions. We have more recently collected data on the timing of mandatory licensure for the two nursing professions. Specifically, we have data on registered nurses from 1950 to 1960 and practical nurses from 1960 to 1970. During this period, 21 states adopted mandatory licensing for registered nurses and 20 states adopted mandatory licensing for practical nurses. We can therefore estimate the effect of licensure on the participation on female and black registered and practical nurses during the period when licensing became mandatory. Regression results are displayed in

Table 2.⁶ For women, mandatory licensing had no statistically significant impact on participation in either practical or registered nursing. For blacks, the adoption of mandatory licensing raised black participation in registered nursing but had no statistically significant effect on black participation in practical nursing. Hence, it would seem that when nursing licensure became mandatory, it still did not have significantly negative effects on participation, and likely increased the representation of blacks among registered nurses. Accordingly, the results from these regressions are consistent with our original findings.

Do we measure the restrictiveness of licensing?

KPV accuse us of including no measures of the restrictiveness of occupational licensing laws. Because the restrictiveness of licensing laws varied by state, KPV believe that failure to control for differences in the strictness of laws biases our results.

We have several replies to this argument. The first is that the claim that we do not have any measure of restrictiveness is patently false. We do. A whole section of our article (Section 6) studies the effects of specific licensing requirements on the prevalence of females and blacks among teachers and physicians. In fact, as we wrote in this section of our article, the motivation for collecting data on particular licensing requirements is precisely to reduce the problem of measurement error (Law and Marks 2009, 362).

KPV then argue that, for the two occupations that we study in greater depth, we have *selectively* chosen which requirements to include in our regression analysis. In discussing our analysis of physicians, KPV note that the two specific licensing requirements we included in our regressions—whether a four-year medical degree was required, or whether there were pre-medical education requirements—are both educational requirements. Since licensing requirements vary along many dimensions, they wonder why we chose to focus on these two requirements. The suggestion is that had we chosen other requirements, the results might be quite different.

Licensing requirements obviously do vary along many dimensions. However, educational requirements have been shown to be among the most prevalent (see Kleiner and Krueger 2009), so the focus on educational requirements would seem well justified, especially since teachers and doctors are among the more highly

6. Because the information provided by the decennial censuses changes over time, these regressions are not identical to those estimated in our original study. However, efforts were made to make them as similar as possible. For instance, in the analysis conducted using earlier census periods, we excluded military, retired, and housewives. The latter two are no longer reported categories. Hence, for this analysis, we excluded anyone who was not in the labor force.

skilled occupations in our sample. Additionally, if we are to use variation in licensing requirements to measure licensing, we need licensing requirements that varied sufficiently during the period under analysis. The other licensing requirements for which we could find data (for instance, whether or not a physician had to pass a licensing exam) did not vary significantly during this time. Hence, we could not use variation in these requirements to identify the effects of licensing.

For the remaining nine occupations in our sample, it is true that we do not have more direct measures of the restrictiveness of a state's licensing regime. The relevant question is therefore whether variation in restrictiveness is correlated with minority participation in such a way that states with low restrictiveness are those where minority participation is growing fastest. KPV provide no systematic evidence showing that variation in restrictiveness was correlated with minority participation. We have no reason to believe that the failure to control directly for restrictiveness induces anything other than classical measurement error, which should make it *harder* for us to find that licensing had any effect on minority participation.

Some occupations (teachers) are mostly employed by the public sector

KPV object to the inclusion of teachers in our analysis, largely because most teachers are public sector workers. We are not quite sure why this should matter. Our objective in conducting this study was merely to determine the effect of occupational regulation on minority participation. While we agree that the factors influencing employers' or customers' decisions may vary by sector, it is not clear to us that the net impact of licensing should be systematically different for the public sector. Licensing could potentially serve as either an entry barrier that facilitates discrimination against minorities or as signal of quality that reduces statistical discrimination, even in environments where the employer is the government and other (perhaps political) considerations are present. KPV have not clearly articulated why our overall findings are biased as a result of including public sector workers. Accordingly the inclusion of teachers among the occupations we analyze seems warranted.

Nurses and teachers?

KPV argue that it is not obvious how to interpret our findings regarding the effect of occupational licensing on teachers and nurses on women, two occupations that were (and still are) predominantly female. We agree and were up front

about this in our article (Law and Marks 2009, 363). We have nothing more to add on this issue.

Craft/trade unions and interstate migration as omitted variables?

KPV's final complaint in this section concerns omitted variable bias. The first omitted variable they are concerned about is the extent of craft or trade unionization in some occupations. During this period, some of the occupations we analyze (for instance, plumbers) were unionized in some states. In the current version of their critique, KPV argue that if craft unionization and licensing were *substitute* mechanisms for reducing minority participation in certain occupations, then failure to control for the presence of craft unions will bias our results toward finding that licensing helped minority participation.

While we agree that this would be the direction of bias if it were true that unionization and licensing were substitutes, we think it worth pointing out that this is precisely the opposite of what KPV wrote about craft unions in the earlier version of their critique (Vorotnikov et al. 2012, 15-17). In that earlier version, KPV noted that unions often used licensure as an additional tool for reducing minority participation in a given occupation.⁷ Hence, the suggestion from the earlier version of their critique was that craft unions and licensure were *complementary* mechanisms for reducing minority and female participation. But in that case, if failing to control for unionization biases our estimates, it probably biases our estimates of the impact of licensing *against* our actual findings. Let us take KPV's original argument seriously. Suppose unionization is positively correlated with the adoption of licensure, perhaps because unions lobby for licensure as an additional entry barrier that facilitates racial discrimination. In our view, this seems plausible since unions are well positioned to solve the collective action problem that must be overcome to obtain licensing legislation in the first place. In that case, if we fail to control for unionization, we will *over-estimate* the *negative* impact that licensure has on minority participation. In other words, our regressions are biased *against* finding either no effect or a positive effect. We are unsure why KPV have chosen to reverse their original position on the relationship between licensure and craft unionization. One possibility is that they quickly discovered that their original

7. In fact, when describing the qualitative evidence about this relationship in their original critique, KPV wrote that unions "worked hand-in-glove with licensing requirements to keep blacks out." They also argued that unions "had a legacy of discrimination" and "often lobbied for licensure" (Vorotnikov et al. 2012, 16-17). This evidence is omitted in the current version of their critique. To be fair, in the earlier version of their critique, KPV also mentioned the possibility that unionization and licensure may have been substitute mechanisms, but the bulk of their discussion seemed to suggest the opposite.

position on this relationship was not helpful for their critique, which we pointed out in our original reply (Law and Marks 2012, 17). However, we can never uncover KPV's true reasons.

In fact, it remains mostly a conjecture as to whether unionization was even correlated with licensure during this period. Unfortunately, there are no reliable state-level data on unionization prior to the mid 1960s (see Hirsh, Macpherson, and Vroman 2001). However, some other evidence we presented in our article suggests that predictors of unionization are not correlated with the adoption of licensure. In our effort to show that the adoption of licensing constituted a valid quasi-experiment, we estimated a series of regressions to determine whether states that adopted licensing differed systematically from states that did not (Law and Marks 2009, 356). Among other things, we found that the growth rate of an occupation (a predictor of unionization) was uncorrelated with the adoption of licensing, which, indirectly, suggests that licensing was uncorrelated with unionization.

In the original version of their critique, KPV claimed that our results are biased because we failed to control for changes in the minority population (Vorotnikov et al. 2012, 17). This claim, as we pointed out in our original reply (Law and Marks 2012, 18) is false, because in the same series of regressions of the correlates of adoption, we also included measures of the minority population, finding no correlation between changes in the minority population (either within the occupation or within the labor force) and the adoption of licensure (Law and Marks 2009, 356). If changes in the minority population are uncorrelated with the adoption of licensure, not including this variable will not alter our results. KPV had therefore failed to identify an omitted variable that we need worry about and appeared oblivious to the efforts we had undertaken to show that the adoption of licensing during this period constitutes a valid quasi-experiment. In their updated critique, KPV now claim that we fail to control for interstate migration of minorities, but acknowledge that we included black and female shares of the labor force in our analysis of the adoption of licensing. We do not believe this to be a serious problem because black and female shares of the labor force subsume black and female interstate migrants. For this to be a source of bias, one would have to think that the effects of licensure on minority participation should be different for minorities born in state versus those born in other states. KPV present no evidence to suggest that this should be the case.

Falsification tests?

KPV conduct two different empirical exercises to explore if our positive findings are “simply spurious correlations” (KPV 2012, 224). The authors do not state what underlying factor(s) they suspect might be driving these spurious cor-

relations between state-level licensure and minority occupation representation. As noted earlier, we ruled out many potential factors including urbanization, and the share of the labor force that is minority when we tested for the validity of the quasi-experiment (see Law and Marks 2009, 356). Unfortunately, in their text, KPV provide little detail about the specifics of these falsification tests. Indeed, KPV's description is limited to two brief paragraphs. With only a week left before our submission deadline, KPV sent us their code. We have used this to make inferences about what exactly they did.

KPV's first falsification test involved estimating "how licensing regulations that were introduced in engineering, nursing, and pharmaceutical professions affected minorities in the plumbing profession and vice versa giving us a total of 16 regressions" (KPV 2012, 224). The results of this exercise are displayed in Table 4, Panel A of their critique (224). KPV argue that if a regression of the effects of, say, engineering regulation, on participation of women in the plumbing profession generates a positive and statistically significant effect, this shows that our methodology is biased. According to KPV, "false positives" of this sort are found in 6 of 16 cases.

We have several problems with KPV's first falsification test. First, it is unclear why KPV only estimated these regressions using females as the dependent variable. Second, we are uncertain why KPV have estimated 16 regressions, not 20. If there are five occupations (engineering, registered nursing, practical nursing, pharmacy, and plumbing), then participation in each occupation can be estimated using data on regulation for four other occupations. Practical nurses are omitted from the first column of Panel A. Why practical nursing is not included is unclear. Perhaps it was because KPV were not able to replicate our results for black practical nurses. In any event, KPV's reasons for omitting this occupation are left unexplained.

Third, the validity of KPV's first falsification test also requires that there be no correlation in the timing of regulation among these seemingly unrelated occupations. We strongly suspect that this is not the case. State-level factors such as population growth or the political party in the state house or of the state governor could result in multiple professions becoming regulated at the same time. If regulation of, say, engineers is correlated with the regulation of pharmacists, and if the regulation of pharmacists increases minority labor force participation in pharmacy, then regulation of engineers will serve as a weak proxy for the regulation of pharmacists.

Finally, it is unclear what numbers are presented in KPV's Table 4, Panel A. Take the 0.4 that is shown in the second row of the panel. One interpretation (which seems to be suggested by the note below Panel A) is that 0.4 is the correlation between registered nursing regulation and engineering regulation. If this

is the case, then our previous point is made for us: it would not be surprising if a regression of the effects of regulation of one profession has a statistically significant effect on participation in another occupation if the regulations are in fact correlated with each other. Hence, we reject the claim that “false positives” in these regressions prove that our methodology generates spurious results.

For their second falsification test, KPV “estimated a series of 50 regressions for each profession with *randomly* generated regulations” as the key independent variable (KPV 2012, 225, emphasis added). It appears that the analysis was conducted using female participation in each profession as the dependent variable. Once again we do not know why KPV did not do the same for blacks. In any event, according to KPV, a positive and statistically significant relationship between these randomly generated regulations and female participation is found more than 10 percent of the time for four professions (see KPV’s Table 4, Panel B).

We have several problems with this falsification test. First, if the authors are truly using randomly generated regulations, then basic statistical reasoning suggests that with a sufficient number of runs, they should find an effect in no more than 10 percent of the cases. (Note, by the way, that they are using a 10 percent standard whereas we held ourselves to a five percent standard). One reason why KPV may find positive and statistically significant effects more than 10 percent of the time is that 50 runs is insufficient. It is possible that with a larger number of runs, the percentage of statistically significant cases would converge to 10 percent. In fact, KPV do not report bootstrapped standard errors so we cannot rule out the possibility that the results are in fact significant only 10 percent of the time.

It is also possible that while KPV state that they *randomly* generated regulations, they did not really do so. From looking at their code, it appears that for each state they randomly assigned a treatment year. This strategy induces correlation across time that is not accounted for in their estimation strategy, which could result in spurious correlations being found in more than 10 percent of cases (Bertrand et al. 2004).

Finally, regardless of what KPV have done, the numbers in Table 4, Panel B (2012, 224) suggest that the average correlation between KPV’s randomly generated regulations and the true regulations was between .20 and .27, which, in turn, implies that they are hardly random. Since KPV’s randomly generated regulations appear to be correlated with the true regulations (which affect participation), it is not surprising that they find significant results in more than 10 percent of the cases.

In summary, insufficient detail is provided about these tests for KPV to claim that they serve as a “damaging” (KPV 2012, 211) critique of our empirical approach. Additionally, from what we can infer about what KPV have done (again, we emphasize that KPV’s description of these tests is vague at best, and we only received their code one week prior to the deadline), we have multiple reasons to

be skeptical about whether these are in fact valid falsification tests. We contend that the implicit falsification tests we conducted on older workers who were grandfathered by occupational regulation and thus not affected by its adoption furnish a cleaner test for spurious correlation (see Law and Marks 2009, 360-361 n. 10). The results of these tests suggest that licensure was not correlated with other factors that independently affect minority labor force participation.

Evidence KPV claim we have ignored

KPV argue that there is much textual evidence that we have ignored that shows the discriminatory nature of licensing during the early twentieth century. We disagree. The fact that we may not have cited some of the particular sources quoted by KPV does not mean that we are unaware of that discrimination was widespread during this period and that licensing may have been viewed by some as a mechanism for reducing competition from minority workers. Indeed, in the introduction to our article, we wrote:

Licensing laws may reduce the prevalence of minorities, either because minorities find it more costly to meet licensing requirements, or because licensing represents a deliberate effort to exclude minorities. While in the first instance a decline in minority representation is an unintended consequence of licensing, in the second, licensing allows regulatory authorities and incumbent practitioners to indulge in their taste for discrimination. (Law and Marks 2009, 352)

Indeed, KPV gloss over the fact that we do find econometric evidence that licensing may have harmed minority participation in some occupations. For instance, we find that the representation of blacks among barbers was significantly reduced by barber licensing laws. We argue that licensing was likely to harm blacks in barbering because the possession of a barbering license provides very little information about quality (reputation mechanisms should be sufficient) and because barbering was an occupation for which black workers were a potential competitive threat to white workers.⁸ In a footnote (359 n. 8), we also mentioned textual evidence that suggests that licensing of barbers was adopted with discriminatory intent.

8. We also found that licensing of beauticians had a negative and statistically significant effect on black participation among beauticians if we exclude southern states from the regression. See Law and Marks (2009, 359 n. 9).

However, the fact that licensing was harmful to minorities in *some* occupations hardly establishes that licensing was harmful to minorities in *all* occupations. Additionally, our findings do not preclude discrimination against some minorities, even in those occupations where the net effect on minority participation was positive. After all, our estimates represent an *average treatment effect*. One of the advantages of an econometric/statistical approach is that it brings more systematic evidence to bear on an issue that can be gleaned from a survey of qualitative sources. Sometimes econometric findings are consistent with the qualitative evidence. Sometimes they are not. When they are not, the inclination is to put more weight on the econometric evidence, precisely because it is a more systematic approach. We are hardly unique in having this preference.

Another reason we place more emphasis on our statistical evidence is that it allows us to separate the intentions of the actors from the outcomes. Much of the qualitative evidence that KPV cite suggests that incumbent practitioners *desired* licensing in order to discriminate against black workers. KPV then prematurely jump to the conclusion that licensing must have had this effect. We do not dispute that there were white workers in early twentieth century America who wanted licensing to reduce competition from blacks. However, it does not necessarily follow that licensing, when adopted, actually *succeeded* in reducing competition from blacks. It is only by conducting an econometric analysis of licensure across a broad range of occupations that we can determine how licensure actually affected minorities.

In fact, it is this systematic, econometric approach that allows us ultimately to paint a far more historically nuanced portrait of the effects of licensure than KPV have acknowledged: namely, that the effects of licensing on minority participation during this time period depended on the extent to which minority workers were a competitive threat, as well as the degree to which there was concern about worker quality. In a few instances, licensing was indeed harmful to minorities, but in other instances, it was helpful. The advantage of our approach is that it allows us to compare objectively the actual effects of licensing across a wide range of occupations. We suspect it would be far more difficult to reach this conclusion by surveying what was written at the time about what individual actors or organizations wanted from licensing.

Theoretical debate about licensing

KPV's final complaint is that we ignore the critical debate about licensing and that we "act as though the quality-assurance rationale is something that the critical literature has neglected" (KPV 2012, 226). We have very little to say in

reply, largely because we do not believe that we have ignored the critical literature, nor do we claim to have invented the quality-assurance rationale for licensing. In our introduction we identified the two main theories for licensure (i.e., entry barriers vs. quality assurance) and cited the relevant sources. Our contribution here is empirical (i.e., to distinguish between the two main hypotheses that have been advanced to explain licensing), not theoretical. The only theoretical twist we may have added to the issue is to link the quality-assurance argument for licensing with a statistical discrimination story (the usual quality-assurance story does not tell us how licensing might affect *minority* workers, whereas combining quality assurance with statistical discrimination generates different predictions for minorities). However, we do believe that we have added to the empirical literature since, unlike most earlier studies, we are able to control for omitted time-invariant state-level factors that affect minority participation, and our study covers a broader range of occupations.

KPV seem to believe that we are uninformed about the literature on market solutions to asymmetric information problems. Obviously, we are aware of this literature. Indeed, because of the availability of these market-based mechanisms, we do not expect licensing to provide a quality assurance role in all instances (e.g., barbers). Accordingly, licensing is unlikely to be helpful (and may even be harmful) for minorities in environments where consumers can easily discover worker quality, either because quality is not an issue or because alternative quality-assurance mechanisms are available. The fact that we did not embark on a lengthy discussion of these alternative mechanisms does not mean that we are unaware of them.

Our problem with KPV's position is that it seems to be based on two non-trivial assumptions: the first is that if market solutions to asymmetric information problems *could* work, they *will* work, and therefore licensing is redundant. The second is that because licensure has shortcomings, it will necessarily fail. We do not dispute that there are plenty of good examples to support both assumptions in some cases. However, this is hardly a sufficient reason to assume that both assumptions are necessarily true in all instances. The fact that there may be potential market solutions does not mean that they will always work effectively, nor that licensing has nothing to add. Additionally, the fact that licensing may be an imperfect institution does not mean that its effects will be uniformly negative. Ultimately, it is an empirical issue. Our goal is simply to determine what the actual effects are without the imposition of strong priors.

Conclusion

No econometric study is perfect and few are definitive. As empirical social scientists, we must live with the fact that to address many questions, we must resort to observational data, which are recorded with error. Additionally, we have to accept that there are factors that may be relevant for our analysis but for which we cannot control. Finally, there are all the other human errors that arise, intentionally or otherwise, simply because none of us is infallible. In dismissing a particular empirical study, it is therefore not sufficient to show that a study is imperfect. Instead, one must go farther and demonstrate that the study is systematically biased in favor of what it finds.

KPV have leveled several criticisms against our study of the effects of occupational licensing laws on minorities during the Progressive Era. In our reply we have taken each of these criticisms seriously. While we acknowledge that there are some precise causal mechanisms that cannot easily be identified, and that in some instances we have erred in interpretation (certification vs. licensure), our view is that KPV have not successfully argued that our study is so biased that we make no contribution to the literature on how licensing laws affected minorities. They have failed to show that our use of census data, the uneven enforcement of licensing laws, measurement error in the timing and stringency of licensing laws, or the problem of omitted variables systematically bias our estimates in favor of finding positive effects. In each of these instances, we have shown that the problems that KPV have identified would either bias our estimates toward zero or in the opposite direction of what we do find. When we do estimate new regressions in light of KPV's suggestions, the results are consistent with our original findings. Specifically, we find that licensure increased representation of blacks among real estate agents and registered nurses, and increased representation of women in the real estate profession. Finally, KPV's falsification tests do not clearly demonstrate that our methodology is biased in favor of finding a positive effect on minority participation. Accordingly, KPV have not made a persuasive case that our study should be disregarded.

KPV also believe that we ignore the qualitative evidence in favor of discriminatory intent. While we dispute the claim that when it comes to our scholarly judgment, the "textual evidence of discriminatory intent...count[s] for very little" (KPV 2012, 226), our position is that it is impossible to know how licensing affected outcomes without a systematic, statistical analysis of the data. It is easy enough to find anecdotal evidence in favor of one position or another. The value added by modern empirical economics is that it goes beyond the anecdotes to

bring systematic data to bear on important economic questions. Accordingly, we are prepared to plead guilty to the charge of basing our conclusions on the statistical evidence. Would not any empirical economist plead the same way?

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