

Reply

What's So Special about the Arabian Peninsula? A Reply to Groh and Rothschild*

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ABSTRACT

My 2008 article suggested that oil wealth, but not Islam, has impeded progress towards gender equality in the Middle East. Groh and Rothschild re-examine one part of my study, which reported a statistical correlation between oil rents and female labor force participation; they argue that the “deep cultural history” of the Arabian Peninsula offers a better explanation for the observed correlations. In this brief reply, I note that they do not accurately describe my conclusions and analysis; that other evidence in the article does not support their argument; and that they have not identified what makes the Arabian Peninsula so different from the rest of the Middle East — apart from its extraordinary oil wealth.

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While, for the most part, the countries of the Middle East and North Africa (MENA) have made admirable progress in closing gender gaps in education and health outcomes, these investments in human development have not yet translated into commensurately higher rates of female participation in economic and political life.

World Bank (2011, viii)

My 2008 article, “Oil, Islam, and Women” asks two questions: why has the Middle East region lagged far behind other regions in progress towards gender equality? And why do gender rights vary within the Middle East? Based on a combination of quantitative and qualitative evidence, it argues that oil wealth can help explain both puzzles, and that simple measures of Islam cannot.

My argument is based on three observations: that in highly-patriarchal societies, the entry of women into the formal labor force can trigger far-reaching changes in gender norms¹; that in many countries, women are first drawn into the labor force by jobs in low-wage, export-oriented manufacturing; and that the discovery of oil wealth tends to lead to the Dutch Disease, which makes these female-friendly industries uncompetitive. This implies that oil can crowd out economic opportunities for women, and lead to the persistence of patriarchal norms.

Groh and Rothschild (2012) state that they find my theory “eminently reasonable” (18), but argue that the empirical analysis “does not provide much evidence that oil is an important driver of female labor force participation rates at all; and they provide some mild evidence that Islam *is* (18).”

Groh and Rothschild take issue with a small but important part of my analysis: my claim that oil rents can affect the number of women in the nonagricultural labor force. They first critique the article’s cross-national regressions that show a link between oil rents and female labor force participation, pointing out that the correlation loses significance when they add to the model a dummy variable for the seven states on the oil-rich Arabian Peninsula.² This indicates, they suggest, that some unobserved

¹ Social theorists since at least Engels[1884]1978 have suggested that working outside the home has a transformative effect on the social status of women. For a review of recent research, see Iversen and Rosenbluth (2008).

² These seven states are Bahrain, Kuwait, United Arab Emirates, Saudi Arabia, Qatar, Oman, and Yemen.

characteristic of the Arabian Peninsula is leading to the appearance of a correlation between oil and female labor force participation.

They also criticize the article's first-differenced regressions with country fixed effects, which again show a correlation between oil rents and the number of women in the work force; they argue that the specification I use is not well suited to testing my theory.

I agree with Groh and Rothschild that the sources of gender inequalities in the Middle East are an open question, and that there is much we do not know. They also identify at least one important anomaly in the evidence for my theory: that neither oil rents nor female labor force participation appear to be correlated with real exchange rates.

Still, I do not think they have yet made a compelling case. Their 'Arabian Peninsula' dummy variable represents six of the seven Middle Eastern countries with exceptional oil wealth. It is not surprising that adding it to some of my regressions causes the coefficient on the 'oil rents' variable to lose statistical significance; the more difficult question is whether this is a meaningful exercise.

I argue below that it is probably not, for three reasons: their article inaccurately describes my conclusions and how I reach them; it overlooks the majority of my evidence; and it does not explain what makes the Arabian Peninsula countries so different from the rest of the Middle East — besides its exceptional petroleum wealth.

What Does My Article State?

Groh and Rothschild's description of my article is inaccurate in two important ways. First, they state that my conclusion is either:

- a. "that oil is *all* that matters,"³
- b. "that oil is what matters *the most*,"⁴

in explaining gender inequality in the Middle East. The article does not contain these statements or reach these conclusions. Rather, it argues that oil *helps* explain gender inequalities in the Middle East, and that the effect of oil rents is substantively important.

The article *does* argue that oil is a better explanation for the Middle East anomaly than a leading alternative — Islam — which is consistent with

³ Page 2.

⁴ Page 2. See similar statements on pages 3 and 14.

the paper’s statistical results. It does not claim, or attempt to demonstrate, that the substantive effects of oil are greater than the substantive effects of other variables in the model, such as income or population age structures. Both the case studies and Ross (2009) point out that cultural and historical factors have had powerful effects on gender outcomes in the Middle East.⁵

Groh and Rothschild also inaccurately describe the basis for my conclusions. They state

Ross’s central evidence that oil, not Islam, is the driver of persistent gender inequalities in the Middle East is a set of coefficients from a series of cross-country between regressions of Female Labor Force Participation Rates on Oil Rents using country-level variables time-averaged over the 1993–2002 period (3).

This is also untrue. If this was my central evidence — a single set of cross-national regressions — my article would indeed be empirically weak. Female labor force participation (FLFP) is just one of the article’s three dependent variables. My inferences about the role of oil rents are based on four sets of statistical regressions (three sets of cross-national between regressions, and one set of first-differences regressions with country and period fixed effects), and four country-level case studies.

The article never suggests that the cross-national regressions re-analyzed by Groh and Rothschild constitute the “central evidence.” To the contrary, the article emphasizes the unreliability of this set of estimations because the dependent variable — female labor force participation — tends to be measured in different ways in different countries. My 15 page article spends two brief paragraphs discussing these regression results, mostly to describe the robustness tests.

If my article had concluded that “oil is *all* that matters” or “that oil is what matters *the most*,” in explaining gender rights in the Middle East, and its central evidence was a single set of cross-national regressions, it would indeed be easy to refute.

⁵ I also restated my conclusions in Ross (2009). Groh and Rothschild may have misinterpreted the article’s argument that “women in the Middle East are underrepresented in the workforce and government because of oil — not Islam (107).” To say that A causes B does not logically imply that A is the only factor affecting B, or even the most important.

The Link Between Oil Rents and Female Political Representation

Groh and Rothschild's key innovation is to divide the Middle East's 18 countries into two subregions — the Arabian Peninsula and everything else — and demonstrate that doing so renders the cross-national correlation between oil rents and female labor force participation unstable (their Table 1 and Figures 1a and 1b). They suggest it is unobserved differences across these two subregions, not oil, that explains much of the variance in female labor force participation.

The cross-national association between oil rents and FLFP is only one of four pieces of evidence in the article that points to the role of oil in gender inequalities in the Middle East. The other three pieces are either overlooked or too quickly dismissed by Groh and Rothschild.

The first piece of evidence comes from the cross-national correlations between oil rents and my other dependent variables: the fraction of parliamentary seats held by women (*Female Seats*), and the fraction of cabinet seats held by women (*Female Ministers*). If the effects of oil rents on gender equality are really driven by the Arabian Peninsula states, then adding the *Arabian Peninsula* dummy to these models should also cause the *Oil Rents* measure to lose significance. In fact, it has the opposite effect.

I demonstrate this in Tables 1 and 2, where I modify my original regressions on *Female Seats* and *Female Ministers* in the same ways that Groh and Rothschild modify my regressions on *Female Labor Force Participation*. The results in Table 1, where the dependent variable is *Female Seats*, are striking: in all specifications, the *Oil Rents* variable remains large and statistically significant; and adding the dummies for *Arabian Peninsula* and *Rest of MENA*, along with interactions terms, causes the coefficient on *Oil Rents* to increase by about fifty percent (column 4). When all seven states on the Arabian Peninsula are excluded from the data, the *Oil Rents* coefficient grows by almost sixty percent from its baseline value, and it remains significant at the $p = 0.01$ level (column 7). The *Islam* variable only attains statistical significance when the dummy variable for “the rest of MENA” is dropped (columns 5 and 6).

The results are similar in Table 2, where the dependent variable is *Female Ministers*. In most specifications, the *Oil Rent* coefficient remains statistically significant, and including the *Arabian Peninsula* and *Rest of MENA* dummies and interactions terms causes the coefficient on *Oil Rents* to rise by more than sixty percent (column 4). When the seven Arabian

Table 1. Cross-national regressions on female representatives.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income (log)	0.320*** (0.0939)	0.328*** (0.0948)	0.321*** (0.0940)	0.336*** (0.0957)	0.297*** (0.0901)	0.310*** (0.0912)	0.342*** (0.0962)
Middle East	-0.193** (0.0796)	-0.205** (0.0792)					-0.152* (0.0856)
Islam	-0.139 (0.0893)	-0.128 (0.0906)	-0.142 (0.0903)	-0.121 (0.0922)	-0.219*** (0.0713)	-0.205*** (0.0721)	-0.120 (0.0916)
Oil Rents per capita	-0.218*** (0.0655)	-0.333*** (0.120)	-0.150* (0.0855)	-0.338*** (0.120)	-0.143* (0.0797)	-0.317*** (0.111)	-0.343*** (0.117)
MENA*Oil		0.141 (0.126)					
Rest of MENA			-0.458 (0.279)	-0.522** (0.232)			
Peninsula			-1.089*** (0.368)	-1.386*** (0.267)	-0.919*** (0.315)	-1.180*** (0.225)	
Rest of MENA*Oil				-0.260 (0.951)			
Peninsula*Oil				0.268** (0.121)		0.248** (0.112)	
Observations	161	161	161	161	161	161	154
R-squared	0.264	0.266	0.270	0.278	0.262	0.269	0.201

Robust standard errors in parentheses. Column 1 replicates Column 4 of Table 4 in Ross (2008). All variables are standardized. Each model includes a constant, which is not shown. Column 7 drops countries in the Arabian Peninsula.
***p<0.01, **p<0.05, *p<0.1

Table 2. Cross-national regressions on female ministers.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Income (log)	0.278*** (0.102)	0.284*** (0.104)	0.280*** (0.102)	0.293*** (0.106)	0.255** (0.0977)	0.263*** (0.100)	0.294*** (0.105)
Middle East	-0.174*** (0.0584)	-0.184*** (0.0609)					-0.160*** (0.0605)
Islam	-0.0116 (0.0814)	-0.00182 (0.0840)	-0.0145 (0.0820)	-0.000470 (0.0851)	-0.102* (0.0601)	-0.0928 (0.0614)	0.00539 (0.0845)
Oil Rents per capita	-0.138** (0.0620)	-0.223** (0.104)	-0.0932 (0.0781)	-0.227** (0.105)	-0.0831 (0.0711)	-0.188* (0.1000)	-0.218** (0.104)
MENA*Oil		0.102 (0.110)					
Rest of MENA			-0.483** (0.190)	-0.381* (0.228)			
Peninsula			-0.871** (0.347)	-1.156*** (0.371)	-0.686** (0.299)	-0.925*** (0.323)	
Rest of MENA*Oil				1.111* (0.664)			
Peninsula*Oil				0.199* (0.119)		0.161 (0.114)	
Observations	155	155	155	155	155	155	149
R-squared	0.121	0.122	0.123	0.128	0.114	0.117	0.100

Robust standard errors in parentheses. Column 1 replicates Column 4 of Table 5 in Ross (2008). All variables are standardized. Each model includes a constant, which is not shown. Column 7 drops countries in the Arabian Peninsula.
 ***p<0.01, **p<0.05, *p<0.1

Peninsula states are excluded, the *Oil Rents* coefficient again jumps by almost sixty percent from its value in the baseline regression, and it remains significant at the $p = 0.05$ level (column 7). Once again, the *Islam* variable is only significant when the dummy variable for “the rest of MENA” is dropped (column 5).

In short, Groh and Rothschild’s *Arabian Peninsula* dummy may or may not help explain why oil rents are correlated with female labor force participation, but it does not explain why oil rents are associated with other outcomes for women.

The second piece of evidence comes from a series of first-differenced regressions with country fixed effects; these models are designed to look at the relationship between changes in oil rents and changes in FLFP within countries over time, and can help mitigate the non-stationary properties of the key variables. The original article shows that under a variety of conditions, an increase in oil rents in one year is associated with a decrease in the number of women in the work force the next year.

Groh and Rothschild replicate my initial estimates and carry out some additional robustness tests: they employ White standard errors; switch from fixed to random effects; add a control for Islamic populations; and use a combination of regional dummies and interaction terms, to allow the effects of oil to differ in the Arabian Peninsula, the rest of the Middle East, and all other countries.

In general, they find the key results from the original model to be robust: in their specifications, increases in oil rents are *always* significantly associated with decreases in FLFP, either in the full sample of countries (their Table 2, columns 1–4; Table 4, columns 1 and 2; Table 5, columns 1 and 2); within the Middle East region (Table 4, column 3; Table 5, column 3); or when the Middle East is subdivided, within the oil-rich Arabian Peninsula (Table 4, columns 4 and 5; Table 5, columns 4–6). In some models, oil does not have a significant effect in the oil-poor subregion of the Middle East or outside the Middle East.⁶ Notably, oil rents are *always* associated with reduced FLFP in the Arabian Peninsula. These results seem to contradict Groh and Rothschild’s assertion that oil wealth cannot explain why the Arabian Peninsula states have low FLFP.

⁶ Note that my theory does not predict that oil will reduce the number of women in the labor force in all types of countries; it should only occur where women are unable to take jobs in the nontraded sectors — which are generally countries with strong pre-existing patriarchal norms.

Groh and Rothschild nonetheless argue that these results may not be valid for two reasons. The first is that the model is “poorly adapted to testing Ross’s basic hypothesis, so that even robustly significant Oil Rent coefficients should not be interpreted as providing support for Ross’s hypothesis (8).” My theory, they suggest, only makes predictions about the relationship between *levels* of oil rents and FLFP; the first-differences model, however, estimates the relationship between *changes* in these factors.

This is incorrect. My theory does not try to explain initial levels of FLFP, which vary from country to country for idiosyncratic reasons, but how a boom in oil rents will alter these levels. The first hypothesis in the original article is explicitly about how these variables change: it states that “A *rise* in the value of oil production will *reduce* female participation in the labor force (110, emphasis added).”⁷

Their second objection is that a hypothesized intervening variable (real exchange rates) appears to be uncorrelated with both oil rents and FLFP. This implies, according to Groh and Rothschild, that even if the first differences estimations are valid, my theory about why they are valid cannot be correct.⁸ This is certainly possible: among Groh and Rothschild’s many critiques, I find this the most significant challenge to my theory. Still, there are many reasons why oil rents might be uncorrelated with the ‘real exchange rate’ variable in Groh and Rothschild’s specification that have little to do with the validity of my theory.⁹

⁷ Of course, levels of oil wealth and FLFP are also informative, since they represent the accumulated changes over time from some baseline level. Since I assume that levels of both oil rents and FLFP were universally low in the past, the cross-national estimates can be considered a snapshot of the accumulated changes over the past 50 or 100 years.

⁸ Groh and Rothschild suggest that their critique of my theory is consistent with other studies that cast doubt on the existence of the Dutch Disease. To support this point, they state that “Magud and Sosa’s (2010) meta-analysis of this literature indicates inconsistent empirical evidence of robust correlations between natural resource shocks and currency appreciation (11).” In fact, Magud and Sosa report that 31 of 35 studies they reviewed found that natural resource or other capital inflow shocks resulted in exchange rate appreciation, which is the central symptom of the Dutch Disease. Magud and Sosa conclude, “the Dutch Disease does exist (Magud and Sosa, 2010, p. 27).”

⁹ For example, their use of a one-year lag on the explanatory variables may be inappropriate. A change in oil rents should lead to an immediate change in a country’s real exchange rate, not changes the following year. Perhaps if changes in oil rents one year were strongly correlated with changes in oil rents the next year — if they were serially correlated — their model would work, since changes in oil rents last year would predict changes in oil rents this year, which in turn would predict changes in the real exchange rate this year. Yet there is only a weak, and *negative* correlation from one year to the next in changes in oil rents, due to the volatility of

The third piece of additional evidence comes from case studies of Algeria, Morocco, and Tunisia, which show how oil wealth can plausibly explain gender outcomes at country level, through the Dutch Disease mechanism I suggest.

These three former French colonies are similar in many ways, and in the 1960s shared exceptionally low levels of female labor force participation; but beginning in the early 1960s, Algeria was ‘treated’ with oil, while Tunisia and Morocco remained oil-poor. During the 1970s, Tunisia and Morocco began to develop major, export-oriented textiles industries, which drew large numbers of women into the labor force; as the number of women in the work force rose, so did each country’s vibrant gender rights movements; and the rise of these movements helped bring about major improvements in gender rights.¹⁰ In oil-rich Algeria, labor costs in the country’s small, domestically oriented textile sector were two to three times higher than in neighboring Morocco. Despite the efforts of the Algerian government, this made it impossible for Algeria to develop a competitive, export-oriented manufacturing textiles sector. With relatively few women in the labor force, Algeria’s gender rights movement was smaller and less successful than its counterparts in Morocco and Tunisia.¹¹

What Explains the Arabian Peninsula Effect?

Let us take Groh and Rothschild’s cross-national analysis at face value. Why does adding an *Arabian Peninsula* dummy to the regression on female labor force participation cause the *Oil Rents* coefficient to drop in size and lose statistical significance?

There are at least two ways to interpret Groh and Rothschild’s results. One is that the *Arabian Peninsula* variable is acting as a dummy variable for the region’s oil-rich states. The Arabian Peninsula includes six of the Middle East’s seven countries with exceptional oil wealth.¹² In the period covered

global oil markets. The strength of the accumulated evidence on the Dutch Disease (see note 8) makes me skeptical about their results.

¹⁰ This was especially true in Morocco, which lacked the enlightened leadership of Tunisia’s President Bourguiba.

¹¹ For additional detail on these cases, see Ross (2012).

¹² These are Bahrain, Kuwait, Oman, Saudi Arabia, Qatar, and United Arab Emirates. On the Arabian Peninsula, only Yemen has less-than-extraordinary oil wealth. Libya is the only Middle Eastern country outside the Arabian Peninsula with exceptional petroleum wealth (meaning above \$1000 per capita).

by the cross-national regressions, the mean level of oil rents per capita in the Arabian Peninsula subregion is \$4486; in the “rest of the Middle East” subregion, it is \$310.¹³ If the Arabian Peninsula states have low FLFP rates because of their oil, the significance of the subregional dummy would be consistent with my original argument. The first-difference regressions are also consistent with this interpretation, since they indicate that oil rents can help explain changes in FLFP within the Arabian Peninsula.

Groh and Rothschild’s preferred interpretation, however, is that the Arabian Peninsula states have other, unobserved cultural or historical qualities that explain why so few women have joined the work force. This is a plausible hypothesis, but it is not clear what these qualities are. Scholars who divide the Middle East into smaller units rarely identify “Arabian Peninsula” and “all other states” as the relevant subregions.¹⁴ What makes the seven Arabian Peninsula states different from the rest of the Middle East, and why would these differences affect the number of women in each region’s work force?

Groh and Rothschild suggest four possibilities. The first is that the Arabian Peninsula states have a distinctive pattern of indigenous plough use, which could mean that “patriarchal norms are literally rooted” in the region’s soil (16). They base this idea on Alesina, Giuliano, and Nunn (2011a), who suggest that the use of the plough in centuries past is associated with reduced female participation in the labor force today. Where plough use was less prevalent, gender norms became more egalitarian.

Can the Alesina–Giuliano–Nunn argument help explain the paucity of gender rights in the Arabian Peninsula? The paper cited by Groh and Rothschild displays no country-level data, but an uncited companion paper by the same authors provides detailed historical data for all countries.¹⁵ It reveals that historic plough use was prevalent in most of the Middle East — including almost all of North Africa, the eastern Mediterranean, Yemen, and Oman — yet there was no indigenous plough use in Saudi Arabia and Kuwait. This would imply *greater* female labor force participation in Saudi Arabia and Kuwait than the rest of the Middle East. Data for Bahrain, Qatar, and the United Arab Emirates are missing.

¹³ Among the non-Middle Eastern states, it is \$112.

¹⁴ See, for example, Rauch and Kostyshak (2009).

¹⁵ Alesina *et al.* (2011b).

Groh and Rothschild's second possibility is that oil mattered but in a different way than my article suggests; they suggest that oil rents may have caused states on the Arabian Peninsula to "lock in" pre-existing low levels of female empowerment." To support this idea, they refer to Morrison's (2009) work on regime stability. Morrison suggests that non-tax revenues, including petroleum revenues, have a stabilizing force on political regimes: they tend to inhibit transitions from autocracy to democracy by allowing elites to increase social spending for the masses, and inhibit transitions from democracy to autocracy by facilitating a reduced tax burden of the elite.

Yet Morrison makes no mention of gender rights or the labor force, and it is hard to know how a theory of oil and gender rights derived from his work would differ from my own theory.¹⁶ Groh and Rothschild do not tell us why oil rents would "lock in" patriarchal norms on the Arabian Peninsula but not in the rest of the Middle East.

Groh and Rothschild's third explanation is the persistence of patriarchal kinship networks. They refer to Charrad (2009), who argues that kin-based networks existed in many Middle Eastern countries before they found oil. Her article identifies six oil-rich countries with these networks: three on the Arabian Peninsula (Saudi Arabia, Kuwait, and the United Arab Emirates), and outside three others it (in Libya, Iraq, and Algeria). Charrad suggests that kinship networks can help explain the region-wide "Middle East" effect on gender norms; Groh and Rothschild imply it could more narrowly account for the subregional "Arabian Peninsula" effect, although they do not offer evidence that these networks were more prevalent on the Arabian Peninsula than in the rest of the Middle East.

Finally, Groh and Rothschild suggest that Islamic beliefs may be more conservative on the Arabian Peninsula. They note that Saudi Arabia is home to Wahhabism, which they describe as "the most fundamental form of Islam (17)." They also speculate that "the five divergent schools of Islam in the Middle East are naturally sorted by region (17)," and suggest that their subregional dummy "is picking up some aspect of religious beliefs (17)."

While Groh and Rothschild are correct that Saudi Arabia is home to the Wahhabi interpretation of Islam, Wahhabism has a significant presence in just one other Arabian Peninsula state (United Arab Emirates); even within

¹⁶ I show in my original article that a country's regime type is not associated with female labor force participation. This implies that Morrison's study tells us little about this issue.

Saudi Arabia, Wahhabis constitute a minority of the population.¹⁷ It is not the dominant force on the Arabian Peninsula that Groh and Rothschild seem to imply.

In fact, Islamic traditions across the Arabian Peninsula are strikingly diverse: Saudi Arabia, Qatar, United Arab Emirates, and Kuwait are majority Sunni; Yemen is almost evenly split between Sunni and Shi'a; Bahrain is about two-thirds Shi'a; and Oman is about three-quarters Ibadi, a distinctive third branch of Islam.¹⁸

Even these broad traditions contain sharp divisions. Although the population of four states is predominantly Sunni, they represent three separate legal traditions (Maliki, Shafi'i, and Hanbali, the latter including the Wahhabi interpretation). Among the Shi'a, there are also striking cleavages: Yemen's Shi'a population is primarily Zaidi, while Bahrain's Shi'a are largely Jafari. Each of these sects and legal traditions have unique interpretations of the sources of religious and legal authority, and different cultural practices.

Even if people on the Arabian Peninsula share highly patriarchal values — and evidence is at best fragmentary¹⁹ — it would be wholly compatible with my argument, which is that gender norms are endogenous to the participation of women in the work force.²⁰

My argument is consistent with the findings of several more recent studies. Blaydes and Linzer (2008), find that in the Middle East, women are more likely to hold fundamentalist values when they lack economic opportunities.

¹⁷ While the Wahhabi interpretation dates back to the eighteenth century, the prevalence of Wahhabism in Saudi Arabia today may be partly attributable to oil. Both Gause (1994) and Herb (1999) suggest that the Al Saud clan — which brought the Wahhabi tradition to prominence — triumphed over rival tribes and clans in the mid-twentieth century, thanks in part to its access to oil revenues.

¹⁸ This section draws on Rahman (1979), Nasr (2006), and Pew Forum (2009). An excellent map of the religious sects, denominations, and traditions in the Gulf is available at <http://gulff2000.columbia.edu/images/maps/MidEastReligionCore.lg.jpg>.

¹⁹ Groh and Rothschild cite data from the World Values Survey in Saudi Arabia, and the Arab Barometer Survey in Yemen, which indicate popular support among respondents for a relatively strict interpretation of Islamic law.

²⁰ Recall that my theory does not try to explain the origins of gender norms, which obviously preceded the natural resource discoveries of the twentieth century and are deeply rooted in local traditions; rather, it seeks to explain variations in *progress toward* gender rights.

The article illustrates this process by describing the case of South Korea, where gender norms in the middle of the twentieth century were just as patriarchal — arguably even more patriarchal — than in the Middle East. It suggests that the development of Korea's export-oriented manufacturing sector in the 1960s and 1970s brought large numbers of women into the labor force, and ultimately led to far-reaching changes in gender norms and political institutions.

Do *et al.* (2011), show that in countries that export products that employ more female labor, women are more likely to participate in the labor force, and the gender gap tends to shrink. They identify “apparel and accessories” and “cut and sew apparel” as among the most female-intensive sectors. The least female-intensive sectors are all based on the extraction and processing of natural resources: “logging,” “coal mining,” “cement,” “sawmills,” and “nonmetallic mineral mining.”

Summary

Groh and Rothschild argue that one part of my argument is empirically weak, and that the “deep cultural history” of the Arabian Peninsula better explains the lagging status of Middle East women. They identify one pattern that is potentially inconsistent with my argument: that real exchange rates do not appear to be correlated with either oil rents or female labor force participation. In so doing, however, they overlook most of the evidence in article — both quantitative and qualitative — which conflicts with their argument, and they do not propose a convincing alternative explanation for the low numbers of women in the labor force on the Arabian Peninsula. The Arabian Peninsula states are exceptional, probably because their oil wealth is exceptional.

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