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woes and military failures. By late 1862 the Republicans may well have lost their majority in the House—a swing of fewer than 10,000 could have achieved such a result.

Given the potential benefits to the Confederacy of controlling the American waters, why did they not obtain a stronger navy? Was Union naval control inevitable? If not, how could the Confederacy have obtained a superior navy? The Confederacy faced daunting obstacles in obtaining a navy, but Northern difficulty in assembling and augmenting its fleet for blockade duties and its hesitance to build immediately an ironclad fleet provided the Confederacy with a fleeting opportunity to gain an initial and perhaps only temporary edge.

The Confederacy did not achieve naval superiority. The informal embargo on the export of raw cotton crippled Confederate finances early in the war. The government hobbled itself by relying upon commerce raiders and by hoping for European complicity in building warships. Immediately importing ship machinery and iron plating from Europe might have been a wiser course. Confederate naval efforts were characterized by “too little, too late.” What opportunities the Confederacy might have had went unrealized.

Thus, the Union Navy’s control of the American waters had three main effects: neutralizing the Confederacy’s price-setting power in the market for raw cotton; preventing Confederate control of those waters; and disrupting Southern internal trade. Although Northern naval superiority alone probably was not sufficient to have defeated the Confederacy, it appears to have been a necessary condition for the Northern victory.

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Comments on Burnette, Deutsch, and Summerhill

Nobody told me beforehand that my job was going to be easy, and it hasn’t been. The category “non-North American” is a somewhat bewildering one. Its breadth left me to cope with topics ranging from Scotch whiskey to Brazilian railways and from the Italian Mezzogiorno after unification to seventeenth-century London goldsmiths. Even before receiving my bundle of dissertations, I wondered about the need to create a set of rules or a scale incorporating “degrees of difficulty” as in mountaineering or “novelty” as in ice skating.

Looking over back issues of the *JEH*, it is striking how the rich, successful countries have tended to capture the lion’s share of the attention in the quest for the Gerschenkron Prize. I thought that an unsophisticated content analysis of short-listed dissertations since 1978 would not go amiss here. A breakdown by region shows a marked preference for Europe. Dissertations on European countries account for over two-thirds of all the dissertations. Africa and Asia have accounted for only four dissertations between them.

Europe	36
Central and Latin America	7
Asia	2
Africa	2
Comparative, world	5
Total	52

Great Britain, France, and Germany alone account for more than half of the total. The Pacific Rim hardly features. By this reckoning, Scandinavia simply does not exist; though Ireland, I am happy to say, has been represented three times, and with distinction.

My chronological breakdown is even cruder, because many short-listed dissertations

straddled my dividing dates. However, it will serve for present purposes. The coverage is no less skewed.

This century	12
1800–1899	21
1700–1799	8
Earlier	5

I wonder whether such patterns really represent the best allocation of research talent in economic history.

Again, the end product of good economic history is sharp insights and clever conclusions. But should one award extra points to researchers who had to cope with unfamiliar languages, master particularly difficult techniques, and delve into awkward archives? I am told that some years ago a brilliant dissertation was not shortlisted because the author had spent too long on it, giving him, as it were, an “unfair” advantage. One sees the logic of this, even when one disagrees.

In choosing which dissertations to discuss, then, should one pay *some* regard to the field, or should one reward excellence, pure and simple? How should the “importance” of a topic be judged? How relevant is policy relevance? I worried about these issues before confronting this year’s crop of submitted dissertations; but in the event, if they affected my judgement, it was only subliminally. I think the presentations you have just heard marginally shift the balance in desirable ways without sacrificing standards.

My summary sense of the dissertations submitted in 1995 was consolidation rather than remarkable innovation. They produced no high-tech gyrations or methodological breakthroughs, no drastic yet still convincing historical revisions. Most candidates paid lip service at least to some primary source research, and a few did much more than that, though I sensed a reluctance to use manuscript or archival sources. At a time when some historians are feeling like members of an endangered species at Economic History Association meetings, I am happy to admit that my preference would have been for more research based on such sources. All the eight dissertations I read were impressive pieces of work, and picking the three you have just heard summarized here was no easy task.

So on to the short list. Joyce Burnette’s thesis is about gender discrimination and occupational segregation in industrializing Britain. This is a well crafted and clever piece of work. Its target is the standard labor history view that customary constraints continued to influence where women might work during the British Industrial Revolution and so led to gender discrimination. Burnette seeks to show that, on the contrary, in much of the economy, labor markets worked well and that, by implication, the degree of discrimination against women has been exaggerated. She argues that, by and large, in the market for unskilled labor, occupational segregation stemmed from comparative advantage. In this market, moreover, segregation was rarely complete. Some women worked with knitting frames and broad looms; indeed, “it was rare but not unknown for a woman to be a coal hewer” (p. 124). Within the local market, the women did the less strenuous work because that is where their comparative advantage lay. When women did hard physical work such as laundering clothes, they received a wage premium. When they reaped, they used sickles because they lacked the strength to mow with the scythe. And so on. Burnette includes some econometric modeling to show that in farming, the number of women hired relative to men in rural England in the late eighteenth century depended on economic factors. Women’s wage had a negative effect on women’s employment, and male and female labor were substitutes.

Burnette highlights schoolteaching—where females outnumbered males by 32,403 to 22,384 in 1841—as an occupation “governed only by competition,” where “women could thrive” (p. 314). I enjoyed her account greatly, but she might have enriched her analysis by exploiting—or trying to explain—the truly massive regional variation across the United

Kingdom in the ratio of female to male teachers. In Middlesex, women teachers outnumbered men by almost three to one. They also dominated in the home counties, but in Scotland there were twice as many men as women teachers, and in Ireland the ratio was 2.6 to 1 in favor of men. How should we interpret the marked north-south gradient in the ratio of men to women? How important was segregation within categories of teaching? What of the earnings of male and female teachers? I don't know about Britain, but in pre-Famine Ireland, parochial schools managed by women tended to be smaller than those managed by men and to cater more for female students. In other words, women teachers almost certainly earned less. So I am not entirely convinced by the picture of "women thriving" as schoolteachers.

Trade unions and gender ideology are the villains in Burnette's dissertation. She usefully documents how nineteenth-century unions resisted female labor. However, unskilled labor could not unionize successfully and so could not exclude women. If, as Marx believed, the Industrial Revolution had tended to deskill work, it would have benefitted women more than men. But if, as Burnette concedes, both skills and the influence of skilled unions increased during the century, opportunities for women may have diminished relative to men.

Then there was the problem of gender ideology, often mediated through the family. A key question here is how much of women's "exclusion" from skilled occupations was imposed by men and how much was women's own choice. It may sound sexist to argue—with Joanna Bourke—that "it was women themselves who made [housework] into a way of life," but it may be anachronistic not to allow for that possibility.¹ The endogeneity or otherwise of the ideology that prevented women from pursuing certain occupations has obvious welfare implications for the outcome described so well by Burnette.

A pleasing feature of Burnette's work is its honesty. She admits that she cannot prove that discrimination was absent and that her evidence that men and women competed in the same market for unorganized labor is "weak." Nor does she trace a clear pattern to trends over time. Long familiarity with her supervisor's work made me wonder whether Burnette might not have extracted more about the effects of the Industrial Revolution from cross-sectional differences. Because British labor market integration still had some way to go even in midcentury, might county data not provide a pseudo-time-series for the analysis of occupational and wage patterns?

Burnette's focus is on the causes of gender segregation in the workplace, but it prompts a whole series of difficult questions about its consequences. How much difference did it make in the aggregate? How much higher would GDP or industrial output have been in its absence? Would marital fertility have been lower? How would the premium for skilled labor have been affected? And so on.

My other two dissertations deal with roughly the same period in Brazilian economic history, 1870 to 1920. The period is one for which no reliable national account series exists, and one wishes that either of our scholars had made some stab at plugging that fundamental gap. Still, between them, Ruthanne Deutsch and William Summerhill provide an excellent introduction to the key features of the Brazilian economy: its dominant role in the world production of coffee, a crop particularly suited to its climate and soil; the segmentation of internal markets by natural and artificial barriers; a labor force that was largely illiterate and therefore voiceless in the political system; and the declining importance of Brazil's other main staple, sugar. However, country and period apart, these are two very different dissertations.

Over the last decade or so, the influence of external economies and imperfect competition in many areas of economics has grown. Deutsch's "Bridging the Archipelago" reflects the shift, but her account of why the regions of Brazil bucked the convergence

¹ Bourke, *Husbandry*, p. 283.

trends highlighted in Jeff Williamson's Presidential address owes as much to Nicholas Kaldor and Jane Jacobs as it does to Paul Krugman.

Largely on the basis of travellers' accounts, Deutsch asserts that "the quality of life and the level of economic development in the Northeast rivaled, if it did not surpass that of the Southeast" around 1870. It must be said that her *statistical* evidence for this claim is weak. Today, per capita incomes in the Southeast are about four times those in the Northeast. Identifying *when* the economic and demographic center of Brazil shifted south is an important issue, because dating "when" more precisely might provide a more precise answer to "how" and "why." Deutsch merely says that it was "sometime between" the census of 1872 and the dawn of the twentieth century (p. 83). How much of the divergence had occurred by 1920 also remains a moot point. More hard information on the state of play at the outset, in 1920, and since would have been welcome.

Still, Deutsch is very good on the symptoms of uneven development. In the Southeast, manufacturing followed finance in a virtuous circle. Human capital and manufacturing was increasingly concentrated in the region, where population rose from 4 million in 1872 to almost 14 million in 1920. The rail network, heavily concentrated there, reflected the urban distribution. Rio's administrative role also mattered. The Northeast's population grew too, but the growth was mainly in the impoverished countryside. The result was a stunted urban network. The usual log linear pattern of urban rank-size distributions—according to a recent issue of *The Economist* (26 July 1995), "nobody knows quite why this is"—identified by historical geographers was absent in the Northeast, with middling-sized cities notably lacking in 1920.

Deutsch's approach to the past is less theoretical and more discursive and narrative than Burnette's. Models of regional imbalance are outlined but not rigorously tested. But her very clear accounts of the role of politics, fiscal federalism, and financial innovation more than make up for the lack of theoretical rigor. For Deutsch, the Northeast's descent into a low-level equilibrium trap in the late nineteenth century stemmed less from geoclimatics than politics. Its ruling elite contented themselves with the tariffs that protected their traditional ways; by 1908 a ton of sugar could be bought in London for less than one-third of what it fetched in Rio. The region adapted by shifting from being a major player on export markets to becoming a backwater supplying the Southeast with sugar. At the end of the day, a niggling suspicion that the Southeast had a comparative advantage in coffee remains.

William Summerhill has written about railways in Brazil. By now, four decades after Robert Fogel and a good two after Jacob Metzger and John Coatsworth, we are a long way down the social savings product cycle. Summerhill, however, offers much more than a replication of earlier work; he does an excellent job of integrating his findings into some of the broader controversies in Brazilian economic history and advances the history of the railway in Brazil beyond the level of "folklore and sensationalism."

The railway came late to Brazil, and by 1914 only 24,000 kms of track had been laid down (compared to about 50,000 kms in Russia by 1910 and 300,000 kms in the United States by 1895). Given the virtual absence of river transport, the delay is all the more puzzling, because before the railway, the pack mule was the main transport mode. Still, it was hardly surprising to read that the social savings from the railway in Brazil were high, though it has to be said that the range of Summerhill's estimates of social saving on freight, from 8 to 38 percent, is uncomfortably wide.

In Brazil, Summerhill shows, backward linkages to other sectors, particularly industry, were weak, and resources leaked abroad to buy and operate railways. The combined outflow of dividend payments, debt interest, and spending on parts and fuel meant that 70 percent of revenues generated by the railway network in 1914 leaked abroad. Still, the total amounted to only 0.4 percent of GNP, which seems a small price to have paid for the resource gains. Policy kept most of the gains in Brazil.

Nor was the benefit limited to the "export" sector. In support, Summerhill claims that

freight charges were “biased” against coffee. I was not quite convinced by his evidence on this point, which is simply that the rate charged per ton-kilometer of coffee exceeded the average charge per unit on all freight. That would be all right if all other freight consisted of eggs, but not if it consisted of stones. Here a comparison with freight rates in other jurisdictions would help; data are plentiful and easy to find. More convincing is his demonstration that coffee and the “export” sector generally accounted for only a small share of freight in 1913. Most of the social savings accrued to the domestic market, not the coffee “enclave.” This is the gist of Summerhill’s critique of *dependencia* thinking on the railway’s role in the Brazilian economy.

Summerhill’s efforts also spark off some questions. One is whether and to what extent did the policy of capping rates deter the construction of other railways and delay the creation of a national network? To what extent did the government give with one hand what it took away with the other? Second, because the network expansion still had a long way to go in Brazil in 1914 (mileage would rise by another 50 percent by 1930), would it not have made sense to calculate the social savings at some later date?

A third question concerns the time-series aspect. If the spread of the railway had already indeed added a monumental two-fifths to Brazilian GNP by 1914, then surely the impact of additions to the network before that date should be sought in the shifting pace of GNP and total factor productivity growth before 1914? Again, this underlines the need for careful national accounts.

Finally, a point that concerns both of these welcome dissertations on Brazil. Since they are fundamentally about the integration of commodity and factor markets—or the lack of such integration—I believe that they would be strengthened by some tests of integration of factor and product markets. I remain unconvinced that some old-fashioned digging could not generate the requisite wage series and overdraft rates for a few regions or cities. The price data required to test the degree of commodity market integration at different periods through, say, straightforward error correction models, must also exist in newspapers or official sources.

In conclusion, I wish all those who submitted dissertations and those short-listed, successful careers, and I am sure we will hear much more from them in the future.

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Comments on Majewski, McDevitt, and Surdam

John Majewski titled his dissertation “Commerce and Community,” and he begins by locating his inquiry with respect to a social historiography of the capitalist transformation that posits an irreconcilable conflict between “commerce” and “community.” His first theme—one of four—is to demolish that dichotomy by documenting the ways in which communities, far from resisting the capitalist transformation, in fact constructed it. Acting through development corporations and networks of kith and kin, communities seized the opportunities presented by emerging markets, designed the infrastructure, and recruited