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## REVIEW ESSAYS

### The Complexities of Blurred Color Lines

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Social scientists are loath to make predictions. They are even less likely—and in some cases constitutionally unable—to involve themselves in the practical implications and interpretations of their research. These orientations are unfortunate. People outside of the ivory tower are usually less interested in our descriptions and analyses of the social world than with how they can use our work to inform their actions and (possibly even) make the world a better place. Fortunately, Richard Alba is a social scientist who understands this. *Blurring the Color Line* is a book conceived and written specifically to help all Americans anticipate and prepare for the racial future.

Alba is more optimistic than most scholars who study race and ethnic relations in the United States. Based upon his understanding of American immigration history (presented in the first couple of chapters) and current demographic and economic trends (the next few), Alba sees tremendous opportunities opening up for new immigrants and native minorities and thus the “chance” for a “more integrated” society, as his subtitle so provocatively and precisely puts it. (Just to be clear: the blurring of boundaries is, for Alba, a welcome development; like many Americans, he wants a society in which color recedes to the background—not that we are there yet.)

This is not totally unfamiliar ground for Alba. His award-winning 2003 book *Remaking the American Mainstream* (written with Victor Nee) argued, contrary to the claims and predictions of multiculturalists, pluralists, and critical race theorists, that new immigrant Americans were largely and increasingly assimilating into mainstream society, blurring racial boundaries in the process. That analysis was based largely on the

*Blurring the Color Line: The New Chance for a More Integrated America*, by **Richard Alba**. Cambridge, MA: Harvard University Press, 2009. \$29.95 cloth. 306pp. ISBN: 9780674035133.

characteristics and behaviors of immigrant and minority groups themselves. This new volume—which grew out of the Nathan Huggins lectures Alba delivered at Harvard in 2008—takes an even larger, more comparative and systemic view.

Key to Alba’s thinking is his innovative (if somewhat inelegant) concept of “non-zero-sum mobility.” “Such mobility,” Alba explains, is “when members of lower-situated groups can move upward without affecting the life chances of members of well-established ones” (p. 15). (A zero-sum situation exists, by contrast, when the gains of some must come at the expense of others. It is, in Alba’s view, the far more typical and expected situation.) Non-zero-sum mobility is essentially what Alba says happened for white ethnic immigrants of the previous century: a booming economy and restrictive immigration policies helped create opportunities for recent immigrants and their children, opportunities that didn’t threaten the power and privilege of the established Anglo-Saxon core and thus allowed newcomers to move into the mainstream of American culture and society. (Alba’s own considerable scholarly reputation is built in part on his early work on Italian Americans.)

Such conditions are ripe again, according to Alba’s careful consideration of current economic projections and demographic trends. An expanding global economy, in which the United States is still well

positioned, along with the exit of baby boomers from the labor force and the declining birth rates of the majority white group, are creating the conditions for minorities and new immigrants to achieve socioeconomic mobility without provoking the usual competitive backlash. Alba's extensive analysis of all the relevant trend data and tables is erudite, and surprisingly accessible.

None of this is automatic, of course. Demography isn't destiny. In the last couple of chapters, Alba devotes considerable attention to what he calls "the contingencies of change"—the factors that could complicate or even curtail the trends toward integration. Changes in immigration policy, for instance, especially policies that would invite richer, more educated elites into the American labor market, are one such contingency. Another would be shifting gender dynamics in employment, and Alba writes insightfully about the challenges of converting socioeconomic gains into social and cultural proximity to whites as well. But "the most serious threat to the potential for ethno-racial change" Alba identifies is the "lagging educational attainment" of African Americans and Hispanics (p. 181). The problem, Alba convincingly argues, is that without proper education and training individuals from these groups simply will not be able to fill the places otherwise made available by retiring baby boomers and the expansion of the economy.

With these points, Alba moves from prediction to prescription, and gives his title a provocative double meaning. "Blurring the color line," at least in my reading, refers not only to the racial patterns that appear to be unfolding of their own accord in contemporary American society; it also involves the more intentional, programmatic steps that need to be taken in order to hasten, enhance, and ensure these largely positive trends.

Much as I learned from this book and much as I see it as a model of and for a mature, engaged social science, I was not entirely swayed by Alba's prognosis. For one, I am not so optimistic as Alba about the future of the American economy in this increasingly global and uncertain era. Also, while it is hard to disagree with the policy implications Alba sketches in the conclusion, they do not fully face up to the dilemmas of race and racism in contemporary America,

especially the political constraints involved. Investing in education for racial minorities, for example, seems like a no-brainer (and not just for racial progress), yet this has not happened for decades.

At least part of the problem is the persistence of prejudice and discrimination in the United States. Alba is not unaware of the realities, obviously. Indeed, near the end of the contingencies chapter, he offers a brief survey of research on what Lawrence Bobo has called "anti-black attitudes." Here, Alba writes: "This record does not induce confidence that most whites are prepared to perceive socioeconomically mobile African Americans as their moral equals in a way that reproduces the shift in perceptions that greeted newly successful white ethnics in the middle of the last century" (p. 208). However, these points find no real purchase in Alba's larger framework. "Now is not the place," Alba says, "to try to settle a disagreement that divides scholars who have devoted their careers to understanding racial attitudes" (p. 208). A book like this is precisely the place to take a stand on these issues—because for many social scientists and cultural critics, racism remains the central structural impediment standing in the way of many native American minorities and at least some more recent immigrants. Which brings me to the question of the colorline itself.

The idea of a colorline goes back, in sociology at least, to W.E.B. DuBois' famous proclamation that "the colorline" would be the problem of the twentieth century. In recent years, new scholarship has begun to challenge this singular framing, arguing that new, more multifaceted racial combinations and lines are emerging—the tripartite Latin Americanization thesis advanced by Eduardo Bonilla-Silva, for example, the three-pronged model described in Portes and Zhou's segmented assimilation theory, or the five-fold "racial pentagon" David Hollinger derived from categories in recent U.S. Census data. Alba clearly does not see this complexity emerging; however, he does tend to see the colorline in a very particular way.

Despite his criticisms of the recent work on whiteness, Alba mostly thinks in terms of a white/nonwhite boundary—white being the category of the majority, of openness and of equality of opportunity.

His conception of and discussion of “non-zero-sum mobility” is built precisely upon the possibility that opportunity for new or previously disadvantaged groups doesn’t need to threaten the status of those already in the privileged group. This conception may work well for some minority groups—Asian Americans for example and many Latin Americans—however it is not as useful for Native American minorities or immigrants with darker skin tones. For these populations the question (and relevant colorline) has less to do with whether they are allowed to become white and is more about how they continue to be seen and stigmatized as black. This is not just a semantic distinction: a “black/non-black” conception of the colorline implies a different set of questions, concerns, and realities than a white/non-white orientation. To put it more in the terms of Alba’s own framing: it may be that current social trends are blurring the colorline at the upper end of the racial-status hierarchy, but this doesn’t necessarily imply that such blurring is unfolding for those folks on the lower, more stigmatized end of things. Here, it is worth remembering that even as white ethnics were shaking off the negative stereotypes that accompanied their entry

into the U.S. social system 100 years ago, African Americans continued to be shackled with the legacies of slavery and Jim Crow. And one of the keys to white ethnic immigrant success (as has been demonstrated in the critical whiteness scholarship that Alba tends to discount) was their ability to distinguish themselves from blacks in order to position themselves for the mainstream, mostly white schools, occupations, and organizations that were the basis for their mobility and eventual assimilation.

None of this is to dismiss Alba’s predictions and prescriptions out-of-hand. Any reader who does that will miss a unique opportunity to better appreciate the complexities and possibilities of America’s emerging racial order. It is, however, to caution against their overgeneralization and to suggest that we think more critically about who they apply to and who they do not. It is also to expand the scope of the conditions we must track and the actions that we must take if America is to move forward toward the goal of a more just, equitable society, one where racial boundaries are blurry and individual opportunity is determined more by the content of one’s character than the color of one’s skin.

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## Redefining the Welfare State: Is American Exceptionalism a Myth?

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The modern welfare state was created in the boom of the post-World War II era, as rising productivity and robust economic growth made it possible for nations to fund an array of benefits that reduced economic insecurity, alleviated poverty, and promoted social stability. Social scientists initially argued that the phenomenal post-war growth of the welfare state was the inevitable outcome of the process of modernization and that all nations would converge along a similar trend line. Yet despite the common trends, welfare states varied significantly in generosity and structure, even in nations with comparable levels of economic development. That posed a puzzle: how could

*Wealth and Welfare States: Is America a Laggard or Leader?*, by **Irwin Garfinkel, Lee Rainwater, and Timothy Smeeding**. Oxford, UK: Oxford University Press, 2010. 254pp. \$35.00 paper. ISBN: 9780199579310.

variation be explained? One answer emphasized the power resources of the working class. According to this theory, the most generous welfare states occur in nations where trade unions formed a political party, created cross-class coalitions with other groups and then used their numerical superiority

to enact programs that protected the working class against the vagaries of the market (Korpi and Palme 1998). Thus, according to power resource theory, variations across nations in public benefits can be explained by the strength of trade unions and degree of working class solidarity.

Although a lively debate ensued as to whether trade union strength was the most important indicator of welfare state generosity or whether the causal sequence accurately reflected the historical record, nearly everyone agreed that the American welfare state was a distinctive case. It was accepted wisdom that the United States was slower than other nations to develop national social programs, spent less generously and lacked benefits like health insurance and family allowances that were otherwise universal. In *Wealth and Welfare States*, three prominent poverty researchers, Irwin Garfinkel, Lee Rainwater, and Timothy Smeeding, take a fresh look at debates about the origins and functions of the welfare state and revisit the question of American exceptionalism.

In most wealthy nations social welfare expenditures make up 30 to 40 percent of the total value of goods and services produced. Given that welfare benefits consume such vast resources, it is important to understand what they contribute to overall well-being. Garfinkel, Rainwater, and Smeeding ask whether the welfare state is a drag on the economy, an issue that has been on the agenda since the 1970s and has become even more salient as population aging and expanding public budgets have strained the fiscal capacity of states. Consistent with most comparative research, they first analyze social insurance expenditures and conclude that early expansions increased growth and more recent increases have not harmed it. Overall, the net effect of social insurance on growth is zero. Yet, they convincingly argue, an analysis focused only on social insurance excludes alternative mechanisms nations use to achieve similar objectives and thus underestimates the full effect welfare states exert on economies. To remedy this problem, they add private employer benefits and tax expenditures, which are (less progressive) ways to achieve some of the same goals as direct spending. They also include education as part of the

welfare state, a choice they justify on the basis of evidence showing that public education does as much or more than traditional benefits in promoting productivity and growth. By expanding the definition of the welfare state to include these other mechanisms for redistributing societal resources, they are able to assert that the welfare state's net contribution is positive.

A second objective of *Wealth and Welfare States* is to challenge the main premise of arguments about American exceptionalism, that the welfare state in the United States is significantly smaller than that of other nations. The authors compare spending patterns in 14 rich nations and find that, according to the most commonly-used measure of size, total social welfare transfers as a share of GDP, the welfare state in the United States is indeed smaller. If a different measure of size is used, however, per capita expenditure, then spending in the United States is greater than in almost all other countries. The results also vary depending on how the parameters of the welfare state are defined. In terms of direct public spending, the English-speaking nations, and particularly Ireland and the United States, spend the least. When employer-provided benefits and tax expenditures are included, however, the picture changes considerably. Adding these benefits increases the U.S. welfare state by nearly 50 percent and substantially narrows the cross-national variation. Including education has even greater consequences. Education increases the American welfare state by another 38 percent and changes the status of the United States from a laggard to a leader. Thus, the overall point is that arguments about American exceptionalism depend on how benefits are measured and which benefits are included.

What is exceptional about the American welfare state is the preference for safety net programs for the poor and private benefits for the more affluent. As an example of the former, the authors discuss the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, which required poor mothers to work in return for assistance and limited total federal lifetime eligibility to five years. In the first six years after PRWORA was enacted, welfare caseloads fell by 56 percent while the labor

force participation rates of single mothers increased and poverty rates declined. The authors note, however, that these trends cannot be attributed solely or even primarily to welfare reform, for PRWORA was enacted around the same time as assistance outside welfare expanded. In 1993 Congress increased the earned income tax credit (EITC) and childcare funding substantially and enacted a new child health insurance program (SCHIP). Those benefits, coupled with a sustained period of economic prosperity, made it possible for low-income women to fare better from work than from welfare.

It would have been helpful for the authors to assess in greater detail whether the work incentives in PRWORA were largely punitive or whether they fit with international trends. Everywhere welfare states designed for an industrial economy and the male breadwinner family type that accompanied it are in the process of being retooled for service-oriented economies where dual-earner and female-headed households are becoming the norm (Esping-Andersen 2009). Was PROWRA the result of a right-wing power shift, as the authors assert, or rather part of a general restructuring of benefits to support the labor force participation of women?

The authors cite private health insurance in the United States as a prime example of a benefit tilted toward the more affluent. They argue that because the private market, left to itself, produces too little health insurance, the only way to make coverage affordable is to eliminate the element of choice, make insurance mandatory and spread the risk across a broad population. This book went to press before Congress passed the Patient Protection and Affordability Act of 2010, which contains several features that meet these objectives, including mandates on individuals and employers and subsidies to help low-income people purchase health plans (Quadagno 2010). It would be interesting to learn whether the authors believe that health care reform has transformed the American welfare state in any significant way or whether it merely represents business as usual.

Where *Wealth and Welfare States* falters a bit is in the authors' theoretical analysis.

This becomes especially apparent in their inclusion of education as part of the welfare state, which raises the question: how broad can the concept of the welfare state become before theories of welfare state development no longer apply? In their analysis of spending patterns, the authors group nations according to Esping-Andersen's (1990) three regime types, liberal, conservative, and social democratic. They find that this typology is roughly consistent with social insurance expenditures but, not surprisingly, does not fit education. What better explains educational development, they argue, is the median voter model. According to this model, democratic nations find the most effective solutions to common problems, because universal suffrage maximizes the expression of diversity and restrains the power of elites. Thus, as the franchise is extended, public funding for education should increase, eventually becoming universal. That assertion raises the question of how the median voter model can explain *variation* in education spending across fully democratic nations. It would be more satisfying to focus on how political coalitions, not just voters, influence decision-making processes. The authors do suggest that declining strength of the left was responsible for the rightward turn in policy in the United States, but they do not elaborate this argument formally. Theory takes a back seat to data.

Despite these minor quibbles, *Wealth and Welfare States* is a major achievement and a welcome addition to the vast literature on the welfare state. It contains quantitative analyses on spending patterns in fuller detail than any previous research and an insightful discussion of recent developments in welfare state trends. The focus on education is likely to generate a lively debate about how the welfare state should be defined and the inclusion of a broader range of benefits will cause many scholars to rethink their basic premises. As for me, this book will be first on my list when I prepare my syllabus for my graduate seminar on the welfare state.

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## Violent Rejectionists: Islamists and Other New Religious Extremists

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Suicide bombings by Islamists in Iraq, Afghanistan, Somalia, and Pakistan are in the news almost every day. We hear about such events in Russia, Western Europe, parts of East Africa, India, Indonesia, Yemen, Algeria, and in Israel and Palestine. The memory of 9/11/2001 in the United States remains a lively topic. Type into Google "CIA terrorism" and the CIA's Web page comes up (CIA.gov). The first heading that appears is a quote by Director Leon Panetta from a May 2009 conference in which he said:

Counterterrorism is CIA's primary mission. Al-Qaeda remains the most serious security threat that we face, most serious security threat to America and to U.S. interests and our allies overseas...

That is why the United States remains at war in Afghanistan, and 9/11 was the American government's excuse for invading Iraq in 2003. The real reason was the mistaken belief by our leaders that it would be possible to establish a friendly, secular, democratic Arab state in Iraq, and that this would unleash a series of changes throughout the region that would eliminate Islamist extremism, establish peace throughout the region, and not coincidentally, secure Middle Eastern oil from the threat of being seized by America's Muslim enemies. Arabs would make peace with Israel, and the United States would become less dependent on the good will of Wahhabi dominated Saudi Arabia, the original home of most of the 9/11 perpetrators and of

*Global Rebellion: Religious Challenges to the Secular State, from Christian Militias to al Qaeda*, by **Mark Juergensmeyer**. Berkeley, CA: University of California Press, 2009. 370pp. \$18.95 paper. ISBN: 9780520261570.

*Radical, Religious, and Violent: The New Economics of Terrorism*, by **Eli Berman**. Cambridge, MA: The MIT Press, 2009. 300pp. \$24.95 cloth. ISBN: 9780262026406.

*Princeton Readings in Islamist Thought: Texts and Contexts from al-Banna to Bin Laden*, edited and introduced by **Roxanne L. Euben** and **Muhammad Qasim Zaman**. Princeton, NJ: Princeton University Press, 2009. 516pp. \$26.95 paper. ISBN: 9780691135885.

Usama bin Laden, Al-Qa'ida's titular leader. Of course it has not quite worked out.<sup>1</sup>

So obviously extremist Islam, its connection to terrorism, and its call to war against what bin Laden calls the "Zionist-Crusader alliance and their collaborators" is something well worth studying in detail (bin Laden 1996, "Declaration of war against the Americans occupying the land of the two Holy Places [i.e., Saudi Arabia]" in Euben

<sup>1</sup> There are many ways to transcribe Arabic words. Unless in a direct quote from another source, I am using the system used by Euben and Zaman (2009).

and Zaman 2009: 436), though few American sociologists have.

Mark Juergensmeyer is one of the rare ones to study this problem, and certainly the best known, though there are many in other disciplines who also have. His recent book, *Global Rebellion* (2009) catalogues violent religious challenges to secular nationalism and modernity throughout the world, not just among Muslims, although Islam takes up about half the book.

In 1994 he published a warning that a new cold war was emerging, the struggle between religious nationalism and the secular state throughout the world. In 2001, he published *Terror in the Mind of God* that specifically went over many examples, from the strange Buddhist sect in Japan that tried to poison thousands of Tokyo subway riders (but only killed a few) to Christian extremists in the United States, as well as Muslim cases. In his new book, he covers much of the same ground, but expands his reach.

The problem has been studied by others for some time. Gilles Kepel, a French political sociologist, wrote about Egyptian Muslim extremism in 1984 in *The Prophet and Pharaoh*. The prophet was Sayyid Qutb (hanged in 1966 by Gamal 'Abd al-Nasser, Egypt's dictator). He is now recognized as one of the key intellectual inspirers of violent anti-American and anti-Western Islamism. (See also the new book by historian John Calvert, *Sayyid Qutb and the Origins of Radical Islamism* [2010].) The "pharaohs" were Egypt's secular rulers, Nasser and his successor Anwar al-Sadat, who signed the first Arab peace treaty with Israel and was assassinated by infuriated Egyptian Islamists in 1981.

Even earlier, Fouad Ajami (now a favorite of the neo-conservative Republicans and a frequent *Wall Street Journal* opinion writer) had pointed out that Israel's smashing victory in the 1967 war against its Arab neighbors had discredited modernizing, largely socialist and secular nationalism in the Arab world. Its hero, Nasser, had been exposed as a braggart who could not follow through on his promises of modernization and the restoration of Arab grandeur (Ajami 1981).

That, on a world scale, is the essence of Juergensmeyer's thesis. Violent religious extremism arises where the promise of the

secular, modernizing nationalist state has failed to deliver what it promised. Religious critics see the secular West as corrupt and amoral because in modernizing it has lost its faith and produced only social alienation and the decline of community.

This critique, however, is a good two centuries old in the West itself and even played an important role in the birth of sociology. Karl Marx, Emile Durkheim, and Max Weber all worried about modern alienation or anomie, excessive individualism, and the corruption of selfish capitalist materialism. So have countless Western and non-Western intellectuals, religious figures, and political leaders. A vast range of solutions has been proposed, ranging from fascism and communism to mildly reformist social democracy, liberalism, or moderate social conservatism. Religious solutions have been sought by various Catholic popes, very liberal American Protestant theologians like Reinhold Niebuhr, and rabidly conservative, anti-Enlightenment American evangelicals like Pat Robertson (Juergensmeyer 2009: 182–83, 261–62). Anti-Western responses have ranged from Mohandas Gandhi to Ayatollah Khomeini who shared contempt for hypocritical Western imperialists and led their societies against them, though their strategies and political ideals were drastically different.

Juergensmeyer does not distinguish enough between his cases and therefore casts his net too widely in *Global Rebellion*. He includes so many religiously disparate movements, from Mongolia, to Buddhists in Sri Lanka, to paranoid racist Christian fanatics in Idaho, that he loses focus. Nevertheless, this should not diminish his accomplishment. He does an excellent job examining the theological basis of Islamist extremism, and he provides some intriguing comparisons with American Christian extremists whose conspiracy theories and fantasies of a coming Armageddon (2009: 182–92) sound strangely like the ravings of Iranian President Ahmadinejad.

Eli Berman's book takes a very different approach. He is an Israeli-American economist who, following a major trend in contemporary social science, does not take theology or ideology very seriously. Rational choice theory and basic economics explain



why what look like religiously motivated terrorists act as they do. Following the influential economist who has made this approach central to the analysis of religion, Laurence Iannaccone, his sociological followers like Rodney Stark and Roger Finke, and such political scientists as Anthony Gill, Berman points out that extremist religious groups behave like exclusive clubs. They insist on allegiance and sacrifice from their members, in return for which they provide important services, a sense of self-worth, and social support. They thrive when establishment religions and political institutions fail to provide adequate public goods.

The more rigorously demanding such groups are, the harder they are for outsiders to penetrate, but the more easily they can prevent defections or free riding by their members. So, if they turn to violent political action, they can get members to make huge sacrifices.

Terrorism, as Berman and many others insist, is not an ideology as such, only the tactic used by those who are too weak to confront their enemy in a direct military way. To conduct a "war against terrorism" is an absurdity. It may be a war against radical Islamists, Tamil Tigers, Northern Irish Republican bombers, or Maoists in India, but not against a tactic common to all of them. Nevertheless, by discussing many violently extremist Christian, Jewish, Muslim, and other examples together, and insisting over and over that it is not the specific ideology or theology of any group that really matters, Berman falls into another trap that mixes all of those who use terrorism as a tactic into an undifferentiated category. In this, he has a lot of support from prominent social scientists.

Berman cites Israeli research by Ariel Merari showing that suicide bombers (only those who failed and survived were interviewed) are not especially hateful or religious (2009: 9–12). Alan Krueger (2007) has demonstrated that terrorists are not usually poor, deluded, or badly educated. Robert Pape (2005) has presented similar findings. Berman repeatedly cites political scientist David Laitin's work (with his colleague James Fearon), which claims there is not really much ethnic violence in the world because

ethnic identity is rarely the only cause of conflict (see Laitin 2007). What may appear to be ethnic or religious conflicts arise for material reasons, and groups react rationally.

Since imagined social identities in the minds of these social scientists are not really terribly rational, they could not possibly explain much of anything. Deeply ingrained feelings of distinctiveness, a sense of group humiliation, a desire to redeem a community's honor, and faith that there is a divine or historically inevitable power justifying acts of violence count for little compared to measurable material realities.

There is something to be said for this anti-ideological approach. Clearly, the "club" theory is useful in showing how violent groups recruit and keep members. Successful movements like Hizbullah or Hamas definitely provide important social services in situations where prior political authorities had badly failed their Lebanese Shi'i or Palestinian constituencies. They also reaffirm their groups' honor. It is never religion or ethnicity alone that inevitably leads to conflict. Unfortunately this seriously neglects the fact that the content of ideologies become critical when highly motivated ideological actors get political power.

Islam is not inherently more violent or peaceful than Christianity, Judaism, or Buddhism. Any religious or ethnic identity can be mobilized for conflict. Appropriate passages in holy texts can be always found to justify vicious violence, and the Qur'an as well as the Christian and Jewish Bibles are full of them. They also have passages that preach moderation and tolerance. What matters is what particular adherents choose to emphasize, and the context in which they operate. Once we have taken that into account, the content of the ideology or faith matters a lot.

Ignoring this is why, when proposing ways of combating terrorism, Berman lapses into unhelpful platitudes. He suggests that isolating the extremists, cutting back their revenues, providing good social services, and promoting civil society can do the job. In other words, win the hearts and minds of the population and the bad guys will lose their ability to recruit. General Petraeus and President Obama concur. No doubt it is

true, but the problem is how to go about doing this when the enemy successfully claims legitimacy on religious or ethnic grounds, or both, and the agents of civil society and social services are viewed as infidel aliens or corrupt puppets? And how do you change the hearts and minds of true believers who begin by hating you when you have to go through unreliable interpreters?

Most extremist groups never succeed because their ideologies fail to resonate with wide enough numbers of the people for whom they claim to be fighting, but it hardly takes a majority to seize power and get their way in an unsettled situation. There were only a few thousand Bolsheviks in Russia in 1914, and most Iranians who were disgusted with the rule of their Shah were not particularly interested in being ruled by brutal puritanical clerics in 1975. But there were enough Russians in 1917 hostile to the uneven and failing modernization project of the Tsarist regime, and Iranians who felt the same way about the Shah's corrupt and offensive Westernizing program in 1979 to overthrow those regimes and hand power to ideologically committed revolutionaries whose ideologies changed their worlds (McDaniel 1991).

So, neglecting the specific content of religious extremist thought is a grave error, because it is their ideology that accounts for how determined they are, what they plan to do if they win, and how they engage their enemies. Islamist extremists have been gaining ground in much of the Muslim world for decades because so many of the states in which they live are corrupt and have failed to modernize equitably. Equally important has been the raw wound of Israel's success. The Qur'an is very concerned with relations between Muslims and Christians and Jews because there were so many of these in Arabia and its neighbors in the seventh century, and also because Muhammad took much of his religion from their beliefs. But while some kind of tolerance toward those "People of the Book" who submitted to Muslim rule was prescribed, they were supposed to accept an inferior status, and for 1,300 years the Jews in Muslim lands did. But Israel has not, and its success is a sore point as far away as Indonesia, not

least because it is a constant reminder of prior Western colonialism and continuing American interference and arrogance. For Palestinians this is a real material issue over control of land and resources, but farther away it is a question of dishonor and religious discordance, because in a well-regulated, properly religious world it should not happen that Jews are more powerful than Muslims.

This is why anyone interested in understanding Islamist extremism would be well advised to read *Princeton Readings in Islamist Thought*. The ably introduced and chosen selections are by twentieth and twenty-first century Islamists. They include basic texts by Hasan al-Banna, the founder of the Muslim Brotherhood in 1928, by the influential Indian-Pakistani Mawdudi, and by Sayyid Qutb and many others, finishing with writings of Usama bin Laden and Muhammad 'Ata al-Sayyid, the leader of the 9/11 killers, who was pathologically afraid of being soiled by impure women. All of them, and most of the others in this volume hate the West, and the closer to the present they are, the more they particularly hate Israel and America. Qutb, who visited America in 1948, found it hopelessly corrupt and sexually immoral, and he came to believe that no secular Arab or nominally Muslim government could redeem Islam's rightful dominant place in the world. Only a return to a pure form of the true faith could do that. Qutb used the word *jahiliyya* (ignorance) to characterize his age. The word had in the Qur'an described the pre-Islamic past before Muhammad, but Qutb claimed that the corrupted and impure Muslim world of today has fallen into similarly pagan neglect of the true way (Euben and Zaman 2009: 129–52).

Reading the 1988 Hamas Charter is particularly eye-opening. It lapses into rabid anti-Semitism that, ironically, takes up the same plot theories about a vast global Jewish conspiracy that were the stock and trade of Western anti-Semites a century or more ago. For those familiar with that literature it will come as no surprise that the French Revolution, Rotary Clubs, Free Masons, and communists are all part of the same Zionist conspiracy (Euben and Zaman 2009: 377). Tsar Nicholas II, Adolf Hitler, and Henry Ford would all have agreed, at

least about the Jewish plot to rule and ruin the world.

There is considerable variation in what these Islamist thinkers want, but there is substantial agreement that sometime in the past Islam went astray, possibly as early as 661 when the Umayyad dynasty took power from Muhammad's brother-in-law 'Ali, or perhaps, for the Shi'i in 680 when 'Ali's son Husayn was killed by those Umayyads who, among their other sins, tolerated the study of pagan Greek philosophy. Islamists want Muslims to understand their Qur'an and return to the moral ways of the original Muslims, the *salaf*. They are what Max Weber would have called "this worldly" puritans urging their followers to take power and bring about the rule of the faithful. Most of them are not loath to recommend violence and armed struggle (*jihad*) against the unbelievers in order to obtain their desired end, but then, neither were the puritanical Protestant reformers or their most fervent Catholic enemies in the bloody European wars of religion during the sixteenth and seventeenth centuries.

There are two aspects of these Islamist writings that are deeply disturbing to anyone who wishes Muslim societies well. One is the formulaic use of the Holy Qur'an. Citations of key verses in various *suras* are repeated as proof, but there is very little actual analysis or attempt to arrive at a better understanding of how societies work and what might be done to improve them. In a devastating book, Timur Kuran (2004) showed that so-called Islamic economics is a set of hollow concepts that offer no solution for economic problems. Where they have taken power or influenced policy, Islamists have managed their economies poorly. Their refusal to face up to the reasons for their societies' technological backwardness or to consider adopting the Western Enlightenment's openness to scientific inquiry, as opposed to merely copying existing technologies, augurs poorly.

Then there is the woman issue. The fear of pollution by sex, and the rapid spread of women covering themselves throughout the Muslim world, even in places where it used to be rare, is not a promising sign. The underuse of women in the labor force,

their lack of education, and the repeated insistence found in Islamist writings that women are there to produce more Muslim warriors and be submissive, do not suggest that this is going to improve soon. The Islamist women writing in this volume, the Egyptian Zaynab al-Ghazali and the Moroccan Nadia Yassine clumsily try to make the blatantly sexist writings of male Islamists seem less harsh than they really are. Reading about Yassine, one wonders how she would fare in Saudi Arabia, and whether she would wind up being stoned to death in Afghanistan or just badly beaten.

If these writings represented a small fringe group of extremists, as some apologists claim, and as seemed to be the case at the height of Third World triumphalism in the early 1960s when radical secularist socialism seemed to be sweeping much of the Muslim world from Algeria to Egypt, Syria, Iraq, and as far away as Indonesia, then there would be little reason to take them seriously. But now, even when most Muslim governments are desperately trying to limit their influence, little by little they are gaining more support among educated but often underemployed, bitter youths, including many who are immigrants in the West. These recruits feel humiliated, dishonored and certain that Salafist Puritanism is the only way out for them and their societies. They blame America and Israel, and are sure that faith offers a solution.

Many tragedies have resulted from this, and reading these Islamist texts tells us there are far more to come.

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## Selection as a Key Tool in Sociological Explanations

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### The Basic Line of Argument

For well over two decades, W. G. Runciman (1983–1997) has argued that “selectionist theory” needs to be a part of sociology. Interestingly, some early twentieth century textbooks in American sociology had included chapters or sections of chapters on selection processes, but with the decline of interest in evolution among sociologists for most of the century, especially with the rise of Social Darwinism, selectionist dynamics were pushed from the discipline, as was biology more generally. Still, the effects of selection on sociocultural processes were kept alive in such subdisciplines as urban and, later, organizational ecology. In his recently published *The Theory of Cultural and Social Selection*, W. G. Runciman extends the application of selectionist theory beyond the relatively narrow confines of urban and organizational ecology, arguing that “the agenda of comparative sociology should be reconstituted” (p. 3) with a more Darwinian analysis of selection forces that have driven history. Runciman rejects the extreme reductionism of “ultra-Darwinians” who would reduce cultural and social selection to natural selection and notions of inclusive fitness, but still, cultural and social dynamics give evidence of clear analogies to natural selection.

Runciman begins by outlining three types of selection: *natural selection* (on phenotypes and their underlying genotypes), *cultural selection* (on “memes” or strips of symbolic meanings), and *social selection* (on practices institutionalized in roles). Runciman retains a heavily Darwinian perspective, seeing “competitive selection” among alternative phenotypes, cultural memes, and social practices as increasing the level of adaptation, or the reproduction of genes, memes,

*The Theory of Cultural and Social Selection*, by **W. G. Runciman**. Cambridge, UK: Cambridge University Press, 2009. 257pp. \$27.99 paper. ISBN: 9780521136143.

and behavioral practices over time. He recognizes, however, that there are significant differences between selection on bodies, cultural memes, and social practices, with the latter two “continuously modified” in a more Lamarckian time scale and, moreover, often working at cross-purposes. Still, co-evolution is constantly occurring but, unlike many co-evolutionary approaches, there is more than gene-culture co-evolution but also gene-social and culture-social forms of co-evolution. Furthermore, selection occurs at different rates, with cultural selection being more rapid than either natural or social selection and with social being more rapid than natural selection; and as noted above, selection at one level can be adaptive, while at another level it can be maladaptive.

Runciman then seeks to rehabilitate the often criticized “just-so” stories of evolutionary psychology on how this or that behavior increases fitness; he offers a more general selectionist methodology for comparative sociology. This methodology presents an account of how patterns of culture and institutionalized practices evolved as a result of selection pressures; since there will always be inadequate or incomplete documentary data on the historical past, the selection explanation will always have the elements of a “just-so” story. Yet, selectionist just-so stories will be value-neutral and free of teleological narratives and, instead, will attempt to isolate the emergence, adoption,

diffusion, and transmission of cultural memes or social practices under competitive selection. Throughout the book, Runciman employs truncated just-so stories to illustrate the utility of this approach. Indeed, one of the real pleasures of the book is to read these protracted examples on such diverse topics as slavery, religious beliefs and practices, world-system (empire) dynamics, wars, politics in diverse societies, and many other illustrations that suggest the utility of a selectionist methodology for developing evolutionary explanations. As Runciman concludes near the end of the book, selection theory is to focus on the resemblances and differences between human cultures and societies, and so, every selectionist explanation "...will be a story about winning and losing memes and practices" (p. 192).

There is, however, a problem with this mode of theoretical analysis: the difficulty of making more abstract statements. Runciman appears to have some ambivalence about whether or not law-like explanatory theory is possible, useful, or even desirable because evolution is about the emergence of "mutant" memes and practices (for whatever reason, often unknown), their adoption (through imitation, learning, or imposition by those with power), and their reproduction over time. This kind of explanation has the same propensity as comparative historical analysis to focus on empirical events more than general processes (beyond selection) that might also be at work and capable of being theorized with more nomothetic forms of theory. Thus, selection theorizing is limited in what it can offer, but this limitation should not be viewed as an argument against its utility as an important theoretical tool.

In Chapter Four on cultural selection and acquired behavior, there are glimpses of theorizing in a more nomothetic mode, although the generalizations offered are still rather embedded in historical examples. But, let me assert what Runciman would probably see as the imposition of an inappropriate formal analysis on his analytical generalizations. Table 1 lays out what I see as some of the key generalizations that offer a sense of how selectionist theory can be more explanatory, above and beyond constructing just-so stories. I have only loosely grouped the

generalization, and it is clear that these propositions need to be made more systematic and parsimonious. Unfortunately, at least from my viewpoint, this is not how Runciman argues; much of the theory in this book is "argument by example"—indeed, fascinating examples—but if there is "a theory of cultural selection," as the title of the book suggests, it must at some point take these kinds of generalizations and convert them into more law-like statements.

Thus, Runciman's "theory" is, to my mind, a pre-theory. There is a conception of selectionist dynamics and a method (just-so stories), but what would constitute an explanation? The just-so story alone? Apparently the answer is yes, but how satisfying are these as explanations and how would they be integrated with other modes of sociological explanation? We are not told.

### Additional Questions

As I have argued in many places, we need to be careful in adopting Darwinian-inspired ideas in sociological explanations. Runciman is diligent and avoids many of the traps in so doing, but he has not resolved what I would see as critical details that must be part of a selectionist theory. One problem is the use of the concept of *memes* which, he admits, is not essential, but which nonetheless distorts the explanation to a view of culture as a "gene pool" when in fact culture is *organized* sets of symbols carrying meanings and varying degrees along many dimensions (e.g., expected conformity, evaluative power, abstractness and generality, etc.). Runciman would not dispute this point, and yet, why not use existing sociological conceptions of culture—even a simple one emphasizing such dimensions of culture as values, ideologies, and various levels of norms—instead of the vague notion of memes proposed by the sociobiologist Richard Dawkins so many years ago?

A second issue is the unit on which selection is working. What dimensions of culture and social practices in what types of social units are subject to selection, and how do the dynamics of selection vary with the differences in the units involved? None of this detail is in the theory but a sociological theory needs more than glosses over the nature

**TABLE 1**  
**Potential Laws of Meme Dynamics?**

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<p>Memes move more rapidly when transmitted by imitation rather than by either learning or individual trial and error, with imitation increasing in social contexts where there is collective organization and fluid forms of collective behavior (p. 95).</p>
<p>Horizontal transmission of memes is more rapid and more likely to depend upon imitation than vertical transmission of memes that will depend more upon formal instruction (p. 112).</p>
<p>Memes are more likely to diffuse to, and be acquired by, groups and cliques differentiated by cultural markets (p. 102).</p>
<p>Memes are more likely to be acquired by a two-step process of leaders acquiring memes and then communicating them to followers (p. 96).</p>
<p>The distribution of memes is correlated with the level of economic, ideological, and political power attached to their carriers (p. 97).</p>
<p>Memes are more likely to be acquired by those in close social space (institutionalized roles in groups) than those in distal social space (pp. 97, 103), and memes that are acquired in close social circles will increase pressures for members to conform to the dictates of these memes (p. 113).</p>
<p>Memes that out-compete other memes and become encoded in groups are more likely to be reproduced and positively sanctioned (p.117), while mutant memes that challenge orthodoxies will be negatively sanctioned and confined to their extremist adherents (p. 129).</p>
<p>Meme mutation and transmission will be slow in social environments where rates of reproduction of traditional memes are high, even when new memes are adopted by prestigious and powerful leaders (p. 113)</p>
<p>Memes that meet universal psychological needs (e.g., identity verification and enhanced self-esteem) and that make the environment more manageable for individuals are more likely to be acquired and reproduced (pp. 102, 104).</p>
<p>Acquisition, spread, and reproduction of doctrinal memes (about unobservable phenomena such as the supernatural) depend upon semantic memory, routinization, and standardization of authoritative teaching, while imagistic memes (about observable phenomena) depend upon infrequent performance and high arousal in groups without centralized authority (p. 102).</p>
<p>The proportion of memes transmitted by parents declines with increases in societal complexity (p. 112).</p>
<p>The rate of meme transmission will be high in social structures that allow for choices in (lifestyles and other behaviors) but the accumulation of these memes will be shorter-lived (p. 113).</p>

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of units that make up the social order. To his credit, Runciman emphasizes group selection but what is the nature of the “group”? For me, roles are not just “institutionalized” (a vague idea to begin with) but embedded in corporate units (revealing divisions of labor in pursuit of goals) of which there are only three types: groups, organizations, and communities. And these are the building blocks of institutional domains which, in turn, are key building blocks of stratification systems, societies and inter-societal systems (Turner 2010). A theory needs to be more specific about the cultural and social structures under selection—groups, communities, organizations, societies, inter-societal systems, classes, values, ideologies, norms?

A third issue not fully resolved in Runciman’s approach is: what is evolving? He emphasizes phenotypes and underlying genotypes, culture, and social practices, and despite his emphasis on co-evolution,

the analysis of culture and social practices does not fully conceptualize these as combined units, or what some call *sociocultural formations*. It is perhaps useful to isolate the culture from the practices, but what about their interconnections? Co-evolution does not capture fully these interconnections (indeed, it surprisingly conflates them); and indeed, sociocultural formation is more often the unit that evolves rather than either memes and practices. In fact, changes in memes and practices are the outcome of the evolution of sociocultural formations in which they are embedded.

A fourth problem may seem surprising: Is Darwinian selection the only type of selection in human systems? Runciman simply assumes that such is the case, and he devotes some time criticizing Spencer and early functionalists but fails to appreciate their great insight: memes and social structures often evolve not so much in response to

competition among alternative forms but *in the absence of any existing alternative form*. Much of societal evolution in the past and today *does not* resolve competition among alternatives under conditions of niche density (or any force increasing competition) but is instead a desperate search by agents and actors to find a sociocultural formation that can resolve new kinds of selection pressures. When societies first grew, the problem was not competition but finding a solution to problems of production, regulation, and distribution that could sustain the larger population. Today, there is selection pressure for global-level political regulation, but the selection here is not competition among existing formations (although this is still involved) but for self-interested agents to discover a new form that does not exist in the present. Runciman is correct to emphasize *entrepreneurs* as the driving force of social selection, but it is not the kind of selection that is Darwinian. I call it Spencerian because it involves actors using their capacities for thought and agency to find new kinds of structures in the absence of any other relevant structures, and the dynamics of this process are very different from Darwinian selection (see Turner, 2010; Turner and Maryanski, 2008a). Urban and organizational ecology posit a Darwinian view of selection, but functional analysis suggests another form of selection that has been even more powerful in the evolution of human societies than Darwinian selection: selection without competition but, instead, pressures to find solutions to new problems in the environment, or die. Natural or Darwinian selection is a sorting mechanism that increases fitness of organizational forms within dense resource niches, whereas functional or Spencerian selection is a *generative process* that creates new kinds of structures where none currently exists to deal with problems of adaptation. Societal evolution, I believe, has been driven more by Spencerian selection, as entrepreneurial actors have sought to find solutions to adaptive problems in the absence of structural formations that can deal with these problems. Spencerian selection, in many ways, allows us to

sustain an evolutionary view and, yet, at the same time, to recognize the importance of agency as human actors try to adapt to ever-changing environments

## Conclusion

There are other points of dispute with Runciman's argument, but I do not want to belabor these (see Turner and Maryanski [2008b] for a longer review of problems in adopting Darwinian and evolutionary ideas from biology to sociology). Despite my criticisms, I like this book very much. It is smart, erudite, and sophisticated in the way that only a senior European scholar like Runciman can be. It makes the case for bringing selectionist arguments into sociological theory. But, it is more like a prolegomenon than a finished theory. It needs a more robust conception of culture and social structure (as distinct from memes and practices in institutionalized roles), and it needs to consider non-Darwinian selection processes. And, as one committed to a more nomothetic theorizing, I believe that it needs more attention to developing abstract principles on selection dynamics to go along with the methodology of constructing just-so stories. Big criticism, perhaps, but they do not diminish the importance of this book for social theorists in general and sociological theorists in particular. This is a short but powerful book, and it should be read by sociologists interested in explaining the dynamics of the social universe.

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## Poverty Politics and Crime Control in Europe and America

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The growth of prisons and jails over the last thirty years transformed the social experience of American poverty. Penal confinement became commonplace for poor men of working age. Incarceration added to the unstable home life of poor children and their mothers, whose own imprisonment rates had also grown rapidly.

Loïc Wacquant's *Prisons of Poverty* (in part, first published in French in 1999) can be read as a forerunner to the author's trilogy, *Urban Outcasts* (2007), *Punishing the Poor* (2009), and *Deadly Symbiosis* (2009). Together, these works describe a new urban poverty embedded in the institutions of criminal punishment. In the main thesis of *Prisons of Poverty*, punitive crime policy joined with a stingy welfare state to propel neoliberal political projects in the United States and Europe. Though regularly trafficking in free market rhetoric, the neoliberal state assumed a muscular role in the lives of the inner-city poor through the agency of the police and penal institutions.

In Wacquant's account, the punitive management of urban poverty was set in motion in the late 1960s. Richard Nixon, in the presidential campaign of 1968, touted a law-and-order message that resonated with white voters discomfited by urban riots and Civil Rights protest. As the problem of urban disorder was thrust into the political spotlight, penal policy experts—and their commitment to rehabilitation—were marginalized. The political and cultural backlash to the tumultuous 1960s ran headlong into the economic collapse of the ghetto. In describing how the jobless ghetto contributed to mass incarceration, Wacquant (pp. 155–56) writes that,

*the penal system has partly supplanted and partly supplemented the ghetto as a mechanism of racial control, after the latter revealed itself unsuited to keeping the black urban (sub)proletariat consigned to the place assigned to it in the new*

*Prisons of Poverty*, by Loïc Wacquant. Minneapolis, MN: University of Minnesota Press, 2009. \$20.00 paper. 217pp. ISBN: 9780816639014.

American social space emerging from the upheavals of the 1960s and the accelerating restructuring of the metropolitan economy.

A populist politics of law and order perpetrated on a surplus population of chronically idle young men greatly enlarged not just the prison population but also the numbers under parole and probation supervision. State budgets were consumed by correctional spending. The conditions of penal confinement became harsher and, says Wacquant, the penal population became darker. These trends, he writes, comprise nothing less than the criminalization of poverty. Punishment escalated not to solve the problem of crime ("criminal insecurity") but to solve the problem of *social* insecurity produced by a newly-precarious economy devoid of a supportive welfare state. This is perhaps the most radical thesis of the book: that the scale and contours of the punishment regime are unrelated to crime. Indeed, "the discourses that seek to connect crime and punishment in America have no validity other than ideological" (p. 159).

Wacquant sees the rise of the penal state as a turning point for American race relations and urban poverty, but its greatest effects may lie in the future and abroad. In the most novel discussion—for American readers, at least—the author describes how the posture and language of U.S. crime policy were adopted abroad by political leaders on the left and right. Here, the book focuses not on the prisons of its title but on police and the zero-tolerance tactics of the New York Police Department of the early 1990s. The language of zero tolerance was seized



most notably by politicians on the European left who announced their no-nonsense intentions for delinquent youth and criminal offenders. Even more clearly than in the United States, where there was little welfare state to retrench, tough crime policy in Europe was embraced as a response to the social problems created by welfare cuts at the end of the 1990s. American-style crime control was readily exported by conservative think tanks which advocated the NYPD's brand of broken-windows law enforcement.

As much a collection of political interventions as sociological essays, the book brims with moral outrage. "The neoliberal utopia," writes Wacquant, "brings in its wake, for the most dispossessed... not an enhancement of freedom... but its abridgment, nay its suppression, as result of a regression toward a repressive paternalism of another age, that of savage capitalism, but augmented by an omniscient and omnipresent punitive state" (p. 130).

Wacquant goes on to provide a glimpse of the good society implied by his critique of neoliberalism. America is a lost cause, but,

Europe stands at the crossroads, faced with a historic alternative between on the one side... the mass confinement of the poor and intensified police surveillance and penal control of the populations destabilized by the revolution in wage work and the weakening of social protections it mandates, and, on the other side, starting today, the creation of new citizens' rights—such as guaranteed minimum income independent of work performance, lifetime education and job training, effective access to housing for all, and universal medical coverage... leading to the creation of a European social state worthy of the name (pp. 130–31).

Wacquant's unremittingly critical perspective is his strength and his weakness. The key insight that punishment intensified in tandem with welfare state retrenchment has profound implications for poverty research, urban sociology, and race and ethnic studies. If Wacquant is to be taken seriously, researchers in all these fields must

expand their scope to consider the novel and far-reaching role of penal institutions in contemporary America. Indeed, poverty researchers are increasingly weighing the effects of incarceration on the well-being of poor mothers and their children (Wildeman 2009). Research on racial inequality has studied discrimination in the newly-punitive policy context (Pager 2006; Schram et al. 2009). Several excellent ethnographies have examined how the criminal justice system now structures daily life and the life course of poor men in the inner-city (Black 2009; Goffman 2009). Originally writing in 1999, Wacquant anticipated by a decade some of the most interesting research on American inequality.

But Wacquant's critical ardor is also a flaw: controversy is sometimes presented as fact, the analysis can veer to functionalism, the autonomy of punishment from crime is overstated and, from an American viewpoint, the politics are fatalistic. Wacquant's critical perspective often yields a broad-brush analysis in which the empirical world is bent to fit the theory. This arises in the comparative discussions that present the rightward shift of labor and socialist parties in Europe as toes dipped in the pool of American social policy. The United Kingdom is held out as the most American of the European welfare states and a key case of Americanization. The prison population did grow under Tony Blair's New Labour government and Blair famously vowed to get tough on crime, as well as its causes. At the same time, however, Blair launched a significant anti-poverty initiative that expanded early childhood education and introduced new cash benefits for low-income parents. The child poverty rate fell significantly as a result (Waldfogel 2010). An unvarnished criminalization of poverty is difficult to sustain in this case.

From an American perspective it is striking that European crime policy has become more punitive in some respects while a comparatively strong anti-poverty commitment remains institutionalized in the welfare state. In short, the large differences between Europe and the United States overshadow modest similarities in policy trend. The stylized presentation of fact sometimes opens

the door to functionalism. Thus, prisons flourished because the ghetto had failed in its key function of racial domination. Mass unemployment undermined the ghetto's capacity for social control. A new—penal—institution emerged to ensure the subjugation of African Americans. As Wacquant (2009) has written elsewhere, the prison succeeded the ghetto, which in turn had succeeded Jim Crow and slavery before that. For this analysis, racial domination is not contingently constructed in the face of resistance and reform. Instead it seems that white supremacy is irreversibly engraved on American race relations. Different institutions have arisen at different times to guarantee this imperative.

The critique over-reaches too by asserting a “crime-incarceration disconnect” that over-estimates the autonomy of punishment. The relationship between crime and incarceration is complex and deserves more detailed treatment than Wacquant provides. It is true that year-to-year variation in crime rates does not consistently track year-to-year variation in incarceration. Still, it does not follow that the level of crime and the scale of punishment are unrelated. Wacquant himself acknowledges that the distribution of punishment across the population at a point in time partly reflects differences in criminal activity. The key example is the murder rate among black men and the high rate of African American incarceration for murder. What is more, the emergence of mass incarceration by the end of the 1990s was foreshadowed by a real and substantial rise in crime rates unfolding over two decades from the early 1960s. Incarceration does not rise and fall in lockstep with the crime rate, but broad trends in crime are an important part of the social context in which crime policy, and incarceration rates, are ultimately produced.

The indirect links between crime and incarceration affect our understanding of the political process. Elected officials were certainly race-baiting and vigorously pitching government authority as Wacquant claims, but they were also addressing voters' anxieties about criminal victimization. Indeed, new histories of American politics of the period have shown that conservative politicians of the 1960s were more attuned

to fears about public safety than liberals who remained, at least for a while, more focused on the root causes of criminal behavior than protection from victimization (Flamm 2007; Perlstein 2001).

Which brings us, finally, to politics. Speaking to a Continental audience, Wacquant's preference for a “social state” over punitive crime policy offers a rousing endorsement of Euro-socialism but says little to the American political scene where over-incarceration is most acute. U.S. welfare politics are saturated with moral judgment, and voters and political leaders have little appetite for uplifting criminal offenders. Any American constituency for welfare over punishment will be more motivated by crime control than improving economic opportunity. Indeed the language of personal responsibility, public safety, and state paternalism that Wacquant derides may all have useful roles in promoting efforts to build programs for young men involved in street crime.

But what policies would be effective? Wacquant's perfunctory welfare wish-list provides little guidance. If we frankly acknowledge the criminal involvement of poor and idle young men, alongside their acute social disadvantage and tough treatment by the authorities, employment emerges as a central goal for policy. As a large research literature shows, a steady job is important not only for its improvements in material well-being, but also for the structure and predictability it lends to daily life. For a population that averages only ten years of schooling and reads at an eighth grade level, education too would seem critical. Employment and education are not only important on the egalitarian grounds that Wacquant emphasizes, but because such policies yield a sustainable and socially integrative public safety.

Whatever the flaws of *Prisons of Poverty*, they are by-products of its brisk polemic. Like all fine polemics, the book should be read as a provocation, a provocation to policy and research. Wacquant's bold account of a punitive neoliberalism offers a rich source of hypotheses for careful empirical work, and a challenge to those who would discount the threat to social justice posed by the growth of America's prisons. His research program tinged with its European

perspective makes an important contribution to a political understanding of the contemporary sources of American social inequality and the historically novel role the prison now plays in the lives of America's poor.

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