

# Correspondence

Assessing the Synergy Thesis in Iraq

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*To the Editors (John Hagan, Joshua Kaiser, and Anna Hanson write):*

Americans are inclined to remember their nation's wars victoriously. "Let it be remembered," President Barack Obama told the Minneapolis American Legion veterans of the Vietnam War on August 30, 2011, "that you won every major battle of that war."<sup>1</sup> He repeated this message on May 28, 2012, during the commemoration ceremony of the fiftieth anniversary of this war at the Vietnam Veterans Memorial.<sup>2</sup> How soon might we hear talk of winning the major battles in Iraq?

Stephen Biddle, Jeffrey Friedman, and Jacob Shapiro (hereafter Biddle et al.) caution that "[t]he decline of violence in Iraq in 2007 does not mean that the war was necessarily a success."<sup>3</sup> Their implication, however, is that the war was not necessarily a failure either. Biddle et al. write that the 2007 drop in violence from 2006 was a "remarkable reversal." They ask, "What caused this turnaround?" (p. 7). Their answer is that the United States devised a strategy that stopped the violence in Iraq with a "synergistic" combination of the U.S. troop surge and the U.S. subsidized Sunni Awakening that "stood up" the Sons of Iraq (SOI).

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1. Barack Obama, speech given at the Ninety-third Annual Conference of the American Legion, Minneapolis, Minnesota, August 30, 2011.

2. Barack Obama, speech given at the commemoration ceremony of the fiftieth anniversary of the Vietnam War at the Vietnam Veterans Memorial, Washington, D.C., May 28, 2012.

3. Stephen Biddle, Jeffrey A. Friedman, and Jacob N. Shapiro, "Testing the Surge: Why Did Violence Decline in Iraq in 2007?" *International Security*, Vol. 37, No. 1 (Summer 2012), pp. 7–40. Subsequent references to this article appear parenthetically in the text.

We argue first that the Biddle et al. synergy thesis and the evidence the authors present in its support overestimate the SOI role in the reduction of violence. Second, we argue that they underestimate the significance of the decision by Shiite leader Muqtada al-Sadr to limit the Mahdi Army's criminality by declaring a unilateral cease-fire. Furthermore, al-Sadr's political calculations of the increasing costs of the Mahdi Army's spiraling violence in 2007 to his Sadrist movement may have motivated this unanticipated cease-fire. Thus, our third argument is that the cease-fire played a major role alongside the surge in reducing the violence and increasing al-Sadr's political influence in the governance of Iraq.

#### OVERESTIMATION AND SELECTIVE SAMPLING

The data Biddle et al. use for their empirical assessment of the synergy thesis consist of "significant activities" (SIGACTs) recorded by the Multinational Force–Iraq in 38 areas of operations (AOs) from 2004 to 2008. They regard these data as "objective and consistent" measures, although it should be noted that use of the U.S. military's own data to evaluate U.S. military policies is open to question—especially given that these data were collected in a time of intense congressional and journalistic scrutiny. Independent data collection is preferable; the temptation to "define down" targeted activities is well recognized in evaluation research.<sup>4</sup>

More important, the AOs that Biddle et al. chose for their sample omit areas of Iraq where the SOI did not stand up, leading to a likely overestimation of the SOI policy impact across all of Iraq. The selected AOs also include only one neighborhood (i.e., Rusafa) east of the Tigris River in Baghdad, thus omitting Sadr City and the other areas of Baghdad most extensively controlled by the Shiites and al-Sadr's Mahdi Army. This impoverished community contains about a third of Baghdad's total population. The sample further omitted Adhamiyah, the location of violent sectarian fighting and subsequent efforts during the surge to reduce the mayhem by installing miles of twelve-foot cement wall barriers. A stretch of these barriers completely walled off a Sunni enclave, stabilizing and preserving one of the few surviving Sunni neighborhoods in east Baghdad.

Some accounts of the decline in violence suggest that it was the result of a mass cleansing of Sunni from mixed and predominantly Sunni neighborhoods in Baghdad.<sup>5</sup> Biddle et al. initially dismiss sectarian cleansing explanations for the decline of violence by focusing on the shift of the Mahdi Army's attacks from targeting mixed Sunni/Shiite areas to targeting Sunni majority neighborhoods to the west of the Tigris. Their point is that "unmixing" the mixed neighborhoods of Baghdad did not exhaust the violence. Instead, the violent attacks persisted and expanded into Sunni-dominated areas—until, Biddle et al. argue, they were subdued by the surge with the essential reinforcement of the SOI.

In the AO trend analyses that Biddle et al. present in their figure 2, however, only two of the neighborhoods—Dora and Sayidiyya—named in their cleansing discussion and included in the AO sample fit the model in which violence rises immediately before and

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4. For a recent example of concerns about the "defining down" of outcome measures for favorable evaluation, see Wendy Ruderman, "Crime Report Manipulation Is Common among New York Police, Study Finds," *New York Times*, July 29, 2012.

5. A unique effort to capture the effects of sectarian cleansing is John Agnew, Thomas W. Gillespie, Jorge Gonzalez, and Brian Min, "Baghdad Nights: Evaluating the U.S. Military 'Surge' Using Nighttime Light Signatures," *Environment and Planning*, Vol. 40, No. 10 (October 2008), pp. 2285–2295.

then declines soon after the standing up of the SOI. In the other two neighborhoods—Ghazaliyah and Mechanic—the violence shows signs of falling before the standup of SOI (see their figure 2). Karkh and Mansour are parenthetically also cited as mixed AOs that do not fit the “cleansing exhaustion of violence” model, but these neighborhoods do not fit Biddle et al.’s synergy model either, with declines in violence preceding rather than following the SOI standups.

#### AN AD HOC MODEL OF SYNERGY

In suggesting an alternate explanation to sectarian cleansing, Biddle et al. clarify the differences they intend to test between the surge, Awakening, and synergy models. A key assertion of the synergy model is that “the surge without the Awakening would have improved security temporarily but would not have broken the insurgency” (p. 23). Because Biddle et al. omit any existing counterfactuals (i.e., areas where the SOI did not stand up) from their data, they must create an ad hoc test to evaluate the relative superiority of the synergy model. Their key testable proposition builds on the following logic: “Proponents of the synergy thesis thus see the Awakening as necessary for the surge to succeed. In this view, however, neither the surge nor the Awakening was sufficient, nor did these factors combine in an additive way” (p. 26). Biddle et al. are saying that the synergy model predicts a “nonadditive”—in other words, multiplicative—interaction effect of the combined surge and SOI standups that is the mechanism required to effectively reduce the violence. “To test these implications,” Biddle et al. explain, “we compared SIGACTs trends before and after SOI standup in each of the 38 AOs for which our interviews provide specific standup dates” (p. 27).

Biddle et al. emphatically describe the support they find for the synergy thesis. They conclude that “24 of 38 AOs where SOIs stood up (63 percent) show violence trending downward more sharply after SOI standup than before” (p. 28). Yet this support is based on their often misleading regression analyses. First, Biddle et al. have an interaction effect in mind when they reach their conclusions, offering the summary statement that “the surge, though necessary, was insufficient and that an interaction between it and the Awakening offers the strongest explanation” (p. 36)—but they include neither a measure of the surge in their models nor an interaction term. At most, their models can only purport to show the importance of the SOI.

More significantly, Biddle et al. confuse what happened in many, if not most, of the AOs by using simple ordinary least squares (OLS) regression measures to reach summary judgments that careful inspection of the actual trends in their figure 2 do not support. Their models make use of only two multimonth slopes: before and after the SOI stood up. Trends in violence rise and fall within the two periods, however, so linear estimation of slopes before and after the SOI standups distorts the nonlinear changes in the violence. The OLS-estimated slopes sometimes mistake declines in violence that start well after the SOI standups for effects of the standup, or worse, they often miss the declining violence that came before SOI standups. A regression discontinuity design or an event-history analysis, both of which are designed to account for time-varying covariates, might have better suited their purposes.

Moreover, these estimations do not include measures that control for the preceding and simultaneous cleansing, surge, and cease-fire processes. It is unclear whether the close intermixture of the influences of these variables in a compressed time frame could be sorted out in expanded models. In any case, a careful visual inspection of the data points in Biddle et al.’s figure 2 is likely a more fitting place to start before more elaborate methods are contemplated.

#### CONTRARY RESULTS

Consider eight of the AO trends Biddle et al. cite as confirming the synergy model. We have abstracted and enlarged these eight areas for inspection in our figure 1. In each of these areas—al Hillah, Baladrooz, Kanan, Karkh, Latifiyah, Mansour, Rawah, and Sadr al Yusufiyah—the violence is trending downward before the SOI standup, and there is no unprecedented plunge in the violence that would reflect a nonadditive interaction soon after the standup. The proportion of confirming districts without these eight areas is actually 42 percent. There is more evidence against their synergy model than for it. The comparison of before and after slopes through regression analysis glosses over the nonlinear ups and downs apparent in a visual examination of Biddle et al.'s figure 2.

To more clearly see the problem of the Biddle et al. methodology in our figure 1, consider two of the problematic cases involving two important parts of Baghdad, the Karkh and Mansour neighborhoods. Both of these AOs are counted as confirming the synergy thesis in Biddle et al.'s table 1. The distribution of SIGACTs before the SOI standup in Karkh, however, appears curvilinear and descends to approximately the post-standup level months before the standup occurs. Biddle et al. impose a linear regression line on this curved distribution and estimate a slope of +0.6. Meanwhile, the trend after the standup is only slightly downward, but not notably so compared to the months before the standup. Their slope estimate for this period is -1.7. They categorize this difference in regression slope estimates in their table 1 as confirmation of the synergy model. Yet visual inspection of the before and after trends leads us to the opposite conclusion.

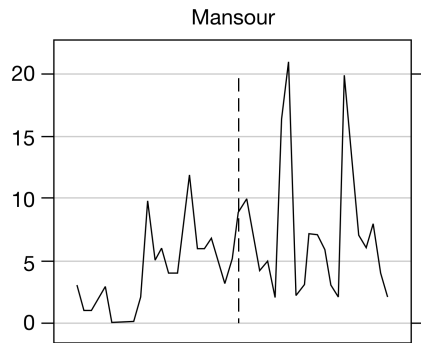
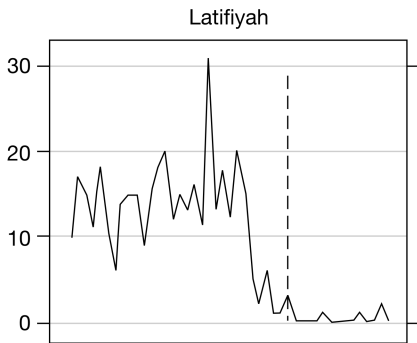
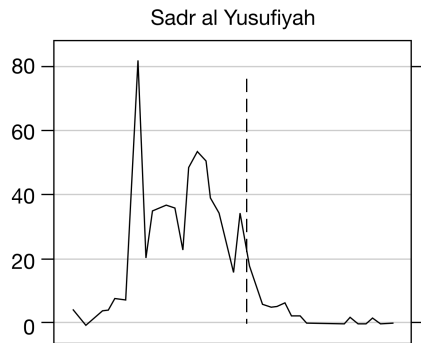
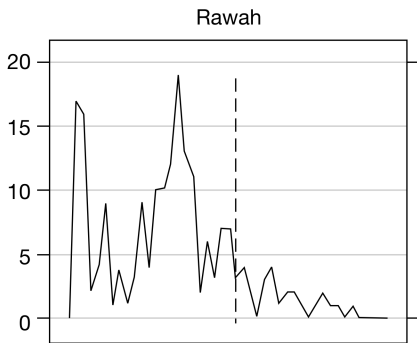
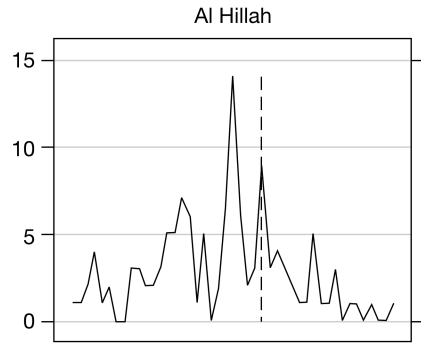
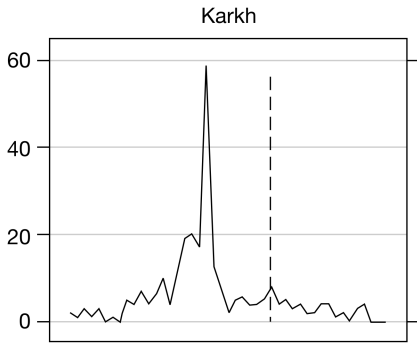
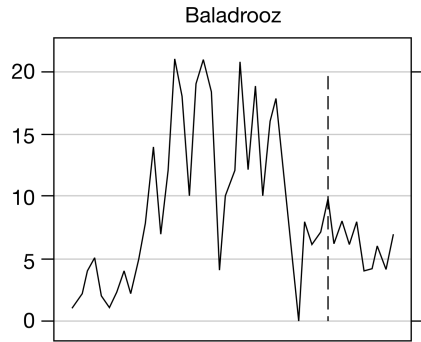
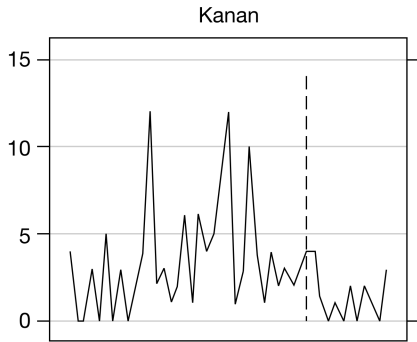
The regression slopes Biddle et al. report for Mansour are at least as misleading, if not more so. Although Biddle et al. interpret the negative slope after the SOI standup in Mansour as confirming its effectiveness, the levels of violence in Mansour after the SOI standup are visibly higher than before the standup—and they only really decrease months afterward. The problem is again with imposing a straight regression line and focusing exclusively on its direction without comparing its results to the data points in the figure and seeing whether the displayed activity levels worsened rather than improved after the standup. Doing so would have indicated the value of statistical models that take into account the effects of time, trends over time, and events that may cause abrupt or gradual changes in those trends.

Biddle et al. go on to describe their “confirming” results in AOs where SOIs stood up prior to August 2007 as indicating that “[w]hereas violence was increasing in each of these AOs at standup, it reversed and plummeted thereafter” (p. 28). Yet, again, close inspection of their figure 2 reveals that this is not the case immediately before and soon after the SOI standups in Fallujah, Hurriyah, Khalidiyah, Latifiyah, Mansour, Rawah, and Sadr al Yusufiyah. In each one, either the “plummet” begins before the SOI stood up—and in many, the levels of violence appear to have already stabilized at a low level—or the violence fluctuates widely both before and after the standup. In Mansour, the violence even increases afterward. The regression analyses that Biddle et al. present obscure more than they reveal.

#### UNDERESTIMATION OF AL-SADR'S CEASE-FIRE

To be sure, there was a downward plunge in violence in Iraq, but it was likely more closely linked to al-Sadr's August 2007 cease-fire than to the standup of the SOI. The downturn in violence can be seen in several data sources. For example, if we break down

Figure 1. Reassessment of Nine "Confirming" Areas of Operation



the Iraq Body Count death toll by months, the greatest monthly drop in violence is between August and September 2007—the month immediately following the cease-fire.<sup>6</sup> Between August and September alone, the Iraq Body Count across Iraq dropped by nearly half: from 2,390 to 1,287. If we break the count down by quarter, the sharp plunge is in the final quarter of 2007, soon after the cease-fire. Of course, Iraq Body Count itself has limitations resulting from its reliance on news reports of deaths—yet there is little or no reason to suspect that such bias would play out in a way that would artificially create a 50 percent drop in violence immediately following al-Sadr's cease-fire.

Perhaps the most compelling data on the improved security situation in 2007 come from the ABC News/BBC/NHK representative surveys of Iraqis analyzed by Gary Langer of Langer Research Associates.<sup>7</sup> The ABC Iraq probability sample surveys were conducted at six-month intervals, with one survey conducted from August 17 to August 24, a week before al-Sadr's unilateral cease-fire, and another six months later in March 2008. The surveys asked about the previous six months and provide unique representative assessments of Iraqi experiences during the surge and after the cease-fire.

The surge was announced by President George W. Bush in January 2007 and implemented in a five-step sequence through the first half of the year. The Sunni Awakening began before the surge and continued throughout it and beyond. Al-Sadr declared his cease-fire more than a half year after the onset of the surge and Sunni Awakening. When the August ABC survey asked in the week before the cease-fire announcement about the last half year of the surge and Awakening, it found that more respondents in the representative sample of Iraqis perceived the security situation to have worsened than to have improved. In the left-hand side of figure 2, we see that when Iraqis answered in terms of their own neighborhoods, about one quarter (24 percent) thought security had improved, whereas nearly a third (31 percent) thought it had worsened, leaving just under half (45 percent) who thought the previous six months of the surge and the Awakening had left things unchanged. On the right-hand side of figure 2, reporting respondents' perceptions for Iraq overall, the results are even worse. Well over half (61 percent) of Iraqis thought security was worse, compared to just 11 percent who thought security was better across the country. Similarly dismal results were revealed when the surveys asked about crime protection and freedom of movement.

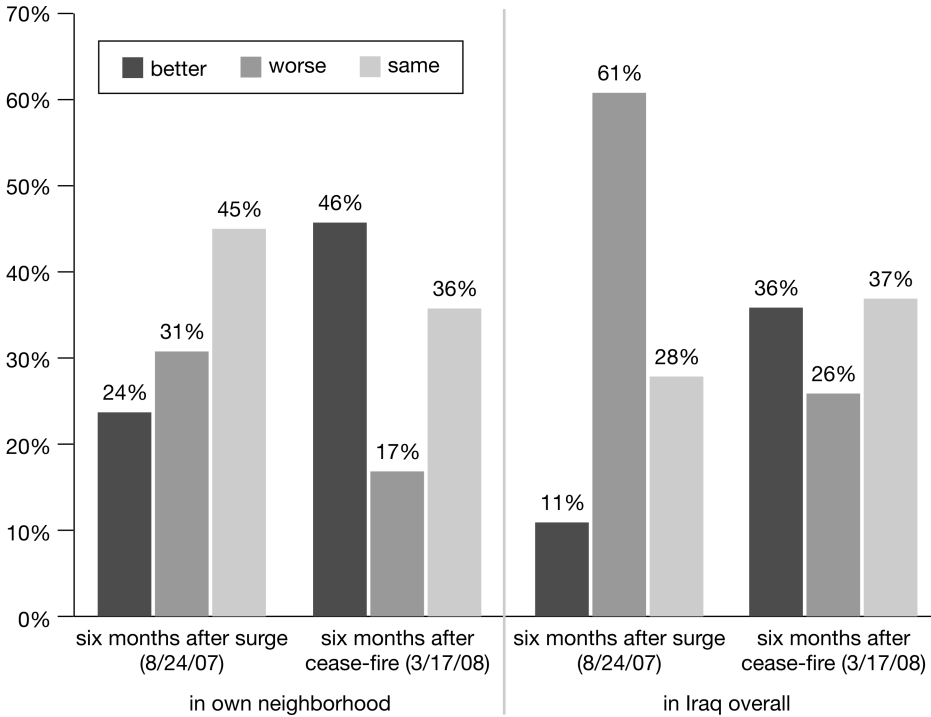
The six months that followed the cease-fire revealed far more positive results. The proportion of respondents answering that security improved in their own neighborhood jumped from about a quarter (24 percent) to nearly half (46 percent), and from 11 percent to 36 percent in Iraq overall. The proportion responding that security worsened in their neighborhood dropped from 31 percent to 17 percent, and in Iraq overall the drop was from well over half (61 percent) to about one quarter (26 percent). Perceptions of security improved far more in the six months after the announcement of the cease-fire than during the half year after the announcement of the surge. Similar improvement following the cease-fire was revealed when the surveys asked about crime protection and freedom of movement.

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6. See "Iraqi Deaths from Violence, 2003–12," Iraq Body Count, January 2, 2012, <http://www.iraqbodycount.org/analysis/numbers/2011/>.

7. Gary Langer, "Dramatic Advances Sweep Iraq, Boos Support for Democracy," *ABC News*, March 16, 2009, <http://abcnews.go.com/PollingUnit/story?id=7058272&page=1>; bar graphs constructed from data presented in tables 11a and 12.

Figure 2. Perceived Security in Iraq



#### THE SURGE AND AN UNANTICIPATED OUTCOME

Our point is not that the surge and the Awakening with its SOI produced no improvement in the security and violence situation in Iraq. The Biddle et al. analysis simply overestimates the value-added by the Awakening and SOI, while overlooking the contribution of al-Sadr's cease-fire to the improvement in security and violence. With specific regard to the synergy thesis, we conclude that the unanticipated benefit of the cease-fire was greater than the anticipated benefit of the SOI. Al-Sadr's cease-fire, of course, was unanticipated in the sense that the U.S. and British forces did not initiate negotiation of the cease-fire and initially expressed doubts about its likely impact. The British military spokesperson said, "We don't know how real this is."<sup>8</sup> Yet within two days, the U.S. military issued a statement that was much similar to the argument Biddle et al. make for the synergy of the SOI, saying that the cease-fire would allow the U.S. and Iraqi forces to "intensify their focus on Al-Qaeda in Iraq . . . without distraction from [Mahdi Army] attacks."<sup>9</sup> Thus the U.S. forces saw a synergy with the cease-fire.

The unilateral nature of the cease-fire suggests that al-Sadr saw it as being in the interests of his movement and leadership. His announcement of the cease-fire came immediately after 52 died and 279 were injured in fighting involving the Mahdi Army

8. Quoted in "Mahdi Army Begins Six-Month Ceasefire," History Commons, August 30, 2007, [http://www.historycommons.org/context.jsp?item=us\\_occupation\\_of\\_iraq\\_tmtn\\_278](http://www.historycommons.org/context.jsp?item=us_occupation_of_iraq_tmtn_278).

9. Ibid.



to the south of Baghdad in Karbala. Al-Sadr had already encouraged his followers to stand down from attacks on the U.S. military in Baghdad, and the death toll was already dropping there. An aide to al-Sadr explained that the stand down of the Mahdi Army was intended to allow his leadership "to restructure it in a way that will preserve its principles."<sup>10</sup> Another aide explained that undisciplined members of the militia were "working for their personal interests . . . to hurt the Mahdi Army's reputation."<sup>11</sup>

There were many indications in 2006–07, as the Mahdi Army swept across Baghdad, replacing non-Shiite with Shiite residents in the mixed neighborhoods of the city, that factions within the Mahdi Army movement were increasingly using violence without al-Sadr's operational control. Al-Sadr needed to consolidate his territorial gains and re-take control of his militia. The remarkable extent of the expanded Shiite domination of Baghdad neighborhoods imposed between 2003 and 2008 primarily by the Mahdi Army is indicated in maps developed by Michael Izady.<sup>12</sup>

The unanticipated consequence of the surge was to give al-Sadr an opportunity to rein in his militia with his unilaterally declared cease-fire. The widespread and systematic displacement of non-Shiite residents from their Baghdad neighborhoods constituted a major crime against humanity. As factions of the Mahdi Army became even more violent in their forays across Baghdad and beyond, the organized criminality of al-Sadr's movement became increasingly unpopular with Shiite as well as other groups. The surge offered al-Sadr a timely means of scaling back these activities and retrenching his movement. As stated earlier, he had already won the battle for Baghdad's neighborhoods.

Al-Sadr did not immediately or entirely change his methods following the cease-fire. His militia was involved in the battle for Basra in 2008, and he again abandoned the fight when losses among his followers began to mount. Both of al-Sadr's stand-downs in 2007 and 2008, however, marked the beginnings of his transition from organized criminality to more conventional politics. In 2010, his followers won thirty seats in parliamentary elections, and al-Sadr became a part of the ruling coalition. By July 2012, the *New York Times* would report of al-Sadr, "Now, with the United States military gone, he has emerged as something more prosaic: a mainstream political leader looking for new paths to secure the claims to power that his movement achieved through violent opposition to the American occupation."<sup>13</sup> The unanticipated synergy of the surge and the cease-fire likely played a central role in the transition. It was becoming increasingly plausible to say of al-Sadr that "he lost key battles but won the war." The thesis of an unanticipated synergy of the surge and the cease-fire is more consistent with the available evidence than the Biddle et al. thesis of an anticipated synergy of the surge and the Awakening.

#### CONCLUSION

Biddle et al.'s synergy thesis and the evidence the authors present in its support overestimate the SOI role in the reduction in violence in Iraq. They simultaneously underesti-

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10. Quoted in Ewen MacAskill, "Al-Sadr Declares Ceasefire in Iraq," *Guardian* (London), August 29, 2007, <http://www.guardian.co.uk/world/2007/aug/29/iraq.usa>.

11. *Ibid.*

12. This map is reconfigured from the work of Michael Izady, The Gulf/2000 Project, <http://gulf2000.columbia.edu/maps.shtml>.

13. Tim Arango, "U.S. Antagonist in Iraq Takes a Political Gamble," *New York Times*, July 9, 2012, <http://www.nytimes.com/2012/07/10/world/middleeast/moktada-al-sadr-recast-as-political-insurgent-in-iraq.html>.



mate the significance of the decision by Shiite leader Muqtada al-Sadr to declare a unilateral cease-fire. By mid-2007, al-Sadr's Mahdi Army had succeeded in gaining control over a significant number of previously mixed and Sunni neighborhoods in Baghdad. During the surge of U.S. forces in 2007 and early 2008, the violent criminality of the Mahdi Army continued, provoking confrontations with the U.S. led forces. In August of 2007, al-Sadr declared a unilateral and unanticipated cease-fire, which likely played a larger role than the SOI in reducing violence from the 2006 and 2007 peak in Baghdad and Iraq. Al-Sadr's tactical alternation between violence and cease-fires was a significant part of the process by which he emerged as an unanticipated powerbroker in the political governance of Iraq.

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*To the Editors (Jon R. Lindsay and Austin G. Long write):*

Stephen Biddle, Jeffrey Friedman, and Jacob Shapiro (hereafter Biddle et al.) evaluate three competing explanations for the remarkable decline of violence in Iraq in 2007: ethnic cleansing, Sunni realignment, and U.S. troop reinforcements.<sup>1</sup> Marshaling an impressive range of quantitative and qualitative data and methods, they conclude that "synergy" between the U.S. "surge" and the Iraqi "Awakening" provides the strongest explanation. This finding, if true, has important implications for civil war theory and counterinsurgency (COIN) policy.

In their article, however, Biddle et al. employ an overbroad notion of "surge" that conflates troops and tactics and muddles "population centric" COIN with improved lethality. Furthermore, their "Awakening" hypothesis is a straw man, and their empirical evaluation of it misconstrues the critical history of Anbar Province. "Synergy" thus becomes a grab bag of different causal mechanisms that provides little explanatory power and undervalues Iraqi strategic choices. In this letter, we offer a counterfactual hypothesis suggesting that the Awakening did not require additional troops, making the surge a tragic waste of resources and a poor template for future intervention.

#### SURGE TROOPS AND DOCTRINE

Biddle et al. acknowledge that "troop count and doctrine are logically independent" (p. 23), but they conflate them as a single "surge" throughout the text. For instance, they write: "The 2006–07 SOIs [Sons of Iraq or Awakening groups] . . . had the surge to protect them from [AQI (al-Qaida in Iraq)] attacks" (p. 21), and yet the "original Awakening predated the surge, so it could not have been caused by it" (p. 27). These contradictory statements seem to refer, respectively, to "surge doctrine" before 2007 and

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1. Stephen Biddle, Jeffrey A. Friedman, and Jacob N. Shapiro, "Testing the Surge: Why Did Violence Decline in Iraq in 2007?" *International Security*, Vol. 37, No. 1 (Summer 2012), pp. 7–40. Subsequent references to this article appear parenthetically in the text.

“surge reinforcements” during 2007. Elsewhere they state that the “surge-Awakening synergy thesis . . . sees the reinforcements and doctrinal changes as necessary but insufficient” (p. 23), which logically implies that if ever reinforcements or doctrinal changes prove unnecessary, then one must reject their thesis. The authors do, in fact, provide evidence that U.S. forces were able to achieve success before 2007 in places such as Anbar and Tal Afar, making a prima facie case that reinforcements were not required for doctrinal changes. Their insistence on reinforcements is perplexing given that two of the authors (Biddle and Friedman) have argued elsewhere that troop preponderance is not required for battlefield success.<sup>2</sup>

Biddle et al. are also unclear about what they mean by doctrinal changes. They describe “U.S. forces out among the population . . . protecting Iraqi civilians” (p. 23) in contrast to “pre-surge methods” (p. 21); and they claim that “[Gen. David] Petraeus insisted on their consistent, theaterwide adoption and thus regularized such methods across Iraq” (p. 23). Biddle et al. are skeptical, however, of the efficacy of economic development and motivational propaganda (e.g., p. 38), even though both feature prominently in the Petraeus doctrine, *U.S. Army Field Manual (FM) 3-24*. Instead, they laud tactics that “enable cooperation with turncoat Sunnis and exploit their knowledge to direct coalition firepower against the still-active insurgents” (p. 26), but *FM 3-24* cautions against working with nonstate militia for fear of undermining government legitimacy.<sup>3</sup> Taken to an extreme, intelligence-driven targeting resembles counterterrorism as practiced by U.S. Special Operations Forces (SOF), a remarkably different approach from population-centric COIN in terms of both its lethality and small footprint.<sup>4</sup> Biddle et al. casually dismiss the SOF alternative because “the four schools we discuss capture the main lines of debate in the literature to date” (p. 9 n. 6). This is simply false: a major axis of the COIN debate concerns large-force occupation versus small-force counterterrorism (newly invigorated by the increased reliance of President Barack Obama’s administration on drone strikes).

The expansive ambiguity of Biddle et al.’s concept of surge misrepresents the strategic choices faced by the United States in 2006 as being limited to status quo, surge, and synergy options. In reality, U.S. forces could vary in troop strength, in population-centric versus enemy-centric tactics, and in unilateral action versus cooperation with irregular militias. Policy debates over the relative merits of counterinsurgency, counterterrorism, or unconventional warfare all reflect very different configurations of these choices, but Biddle et al. do not distinguish them.

#### THE AWAKENING AND ANBAR

While Biddle et al. stretch the concept of surge too far, they turn the Awakening hypothesis into a straw man. Proponents of this alternative (cited on p. 8 n. 3) do not argue that Iraqis acted alone, but rather fully acknowledge the importance of U.S.

2. Stephen Biddle, *Military Power: Explaining Victory and Defeat in Modern Battle* (Princeton, N.J.: Princeton University Press, 2004); and Jeffrey A. Friedman, “Manpower and Counterinsurgency: Empirical Foundations for Theory and Doctrine,” *Security Studies*, Vol. 20, No. 4 (December 2011), pp. 556–591.

3. See, for example, U.S. Army, *Field Manual 3-24: Counterinsurgency* (Washington, D.C.: U.S. Government Printing Office, 2006), par. 3-20.

4. Jon R. Lindsay, “Reinventing the Revolution: Technological Visions, Counterinsurgent Criticism, and the Rise of Special Operations,” *Journal of Strategic Studies* (forthcoming).

support.<sup>5</sup> The real distinctions between the Awakening and surge-synergy explanations concern the type of support and forces needed, and by extension, which were unnecessary. We are thus led back to the definitional confusion over troops and tactics discussed above. Furthermore, the Awakening hypothesis makes an important point about Iraqi agency in the marriage of convenience with the Americans. No new troops or tactics would have mattered had Sunnis not taken the initiative to exploit them to advance their political interests.

Evaluation of the Awakening explanation hinges on events in Anbar Province. The heartland of the Sunni insurgency was all but given up for lost, but then the Awakening emerged and the province was pacified, all prior to the 2007 surge.<sup>6</sup> Biddle et al. attempt to downplay the significance of Anbar as “a small-scale experiment” (p. 27), but in reality it was a profound tipping point that inspired American commanders throughout Iraq to seek out their own “Sons of Iraq” on the Awakening model. Biddle et al. argue that events in Anbar actually demonstrate the weakness of the Awakening hypothesis, because tribesmen “made at least four attempts to realign with coalition forces; [but] none succeeded” (p. 18). Their examples, however, not only fail to address the question of which type of U.S. support was required but also cast doubt on surge explanations. Both authors of this letter worked in Anbar during the surge and are thus familiar with the details of the case.

Biddle et al. first consider resistance by the Albu Nimr tribe in 2004, which they say collapsed because only “a single Special Forces detachment of a dozen soldiers” was provided to support the tribe (p. 19). There were, in fact, conventional forces nearby, including elements of two Marine battalions posted in Hit.<sup>7</sup> Albu Nimr resistance did not end in 2004, however, and by the spring of 2005, as the authors themselves note, “Sunnis from the Albu Mahal tribe in al-Qaim (together with Albu Nimr elements from the city of Hit) created an armed resistance movement” (ibid.). Moreover, the Albu Nimr continued to cooperate with Special Forces around Hit, and by 2006 they had convinced the tribe to contribute hundreds of men to police recruiting drives. According to one source, this occurred despite, rather than because of, the presence of a U.S. Army

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5. For specific reference to the importance of U.S. support in these examples, see Dick Couch, *The Sheriff of Ramadi: Navy SEALs and the Winning of al-Anbar* (Annapolis: Naval Institute Press, 2008), p. 133; Austin Long, “The Anbar Awakening,” *Survival*, Vol. 50, No. 2 (April–May 2008), p. 67; Steven Simon, “The Price of the Surge,” *Foreign Affairs*, Vol. 87, No. 3 (May/June 2008), p. 63; and Daniel R. Green, “The Fallujah Awakening: A Case Study in Counter-Insurgency,” *Small Wars and Insurgencies*, Vol. 21, No. 4 (December 2010), p. 596.

6. As the I Marine Expeditionary Force G-2 observed in a classified assessment in August 2006, “The social and political situation has deteriorated to a point that MNF [multinational forces] and ISF [Iraqi security forces] are no longer capable of militarily defeating the insurgency in al-Anbar.” Thomas E. Ricks, *The Gamble: General David Petraeus and the American Military Adventure in Iraq, 2006–2008* (New York: Penguin, 2009), p. 331. Yet remarkably, by mid-2007—when the surge was progressing only gradually in provinces beyond Anbar—U.S. forces were cooperating with tribal fighters and former insurgents in all of Anbar’s major cities (al-Qaim, Haditha, Ramadi, Fallujah, and Hit) and many of its minor ones.

7. Of the twenty-one battalions in Anbar in 2004, fifteen were supporting operations in Fallujah, leaving six for the rest of the province; elements of 1st Battalion 7th Marines and 1st Battalion 23rd Marines were in Hit. See Carrie Lee, “Iraq Order of Battle Data—District Level,” dataset, 2011; and Carter Malkasian, “Did the Coalition Need More Forces in Iraq?” *Joint Forces Quarterly*, No. 46 (3d Quarter 2007), pp. 120–127.

battalion in the region, as the battalion commander was reluctant to support a tribal engagement strategy.<sup>8</sup>

The authors' next two examples concern events around the border town of al-Qaim in 2005. The tribal militia known as the Hamza Brigade received little U.S. support and was thus unsuccessful, but its reincarnation as the Desert Protectors did receive U.S. support during Operation Steel Curtain. Biddle et al. somehow fail to mention that this support resulted in a decisive defeat of al-Qaida in Iraq (AQI) in the region from which it never recovered.<sup>9</sup> Moreover, the members of the Desert Protectors did not stop fighting AQI after "the program largely disbanded" (p. 20), but rather, as another source notes, "joined the local police and continued to enhance local security, though not as part of the army."<sup>10</sup> When the Albu Mahal turned their guns against AQI, only a single Marine battalion was responsible for the area of operations around al-Qaim, hardly a surge.<sup>11</sup> Tribesmen sought U.S. assistance not for a \$300 paycheck, as Biddle et al. mischaracterize Awakening motivations (p. 18), but rather to protect their lucrative smuggling trade across the Syrian border and to regain local political control from AQI. Albu Mahal's success in co-opting U.S. firepower for private political ends inspired other tribal leaders to realign with U.S. forces and evict AQI. This crucial success occurred two years before the surge; and as Anbari sheikhs would often observe, it provided an important precedent for the more famous Ramadi-based Awakening.<sup>12</sup>

Biddle et al.'s final proto-Awakening is the Anbar People's Council (APC), which as they note, attempted to resist AQI around Ramadi in late 2005 and early 2006, before being decimated by it. The APC received no U.S. support, and does not seem to have asked for any, so it is unsurprising to advocates of the Awakening hypothesis that it was unable to fight AQI. If Biddle et al. were able to demonstrate that U.S. forces had the will but not the troops or doctrine to support the APC, they might have a case where the surge was necessary. But within months of the demise of the APC—and well before the surge—cooperation with Sheikh Sattar and other tribal fighters blossomed around Ramadi, strongly suggesting that cooperation with the APC was possible in late 2005 had both sides been willing.<sup>13</sup>

The surge did not play a role in the pacification of Anbar. The few surge troops deployed to Anbar were employed in a mostly fruitless search for AQI remnants in re-

8. Ann Scott Tyson, "In a Volatile Region of Iraq, US Military Takes Two Paths," *Washington Post*, September 15, 2006.

9. This defeat is described in Long, "The Anbar Awakening," which Biddle et al. cite in their passage on this episode (p. 19 n. 34).

10. Thomas R. Searle, "Tribal Engagement in Anbar Province: The Critical Role of Special Operations Forces," *Joint Forces Quarterly*, No. 50 (3d Quarter 2008), p. 65. See also Malkasian, "Did the Coalition Need More Forces in Iraq?" p. 124.

11. See James Russell, *Innovation, Transformation, and War: Counterinsurgency Operations in Anbar and Nineva Provinces, Iraq, 2005–2007* (Stanford, Calif.: Stanford University Press, 2011), pp. 65–67; and Malkasian, "Did the Coalition Need More Forces in Iraq?" p. 124.

12. See the discussion in John McCary, "The Anbar Awakening: An Alliance of Incentives," *Washington Quarterly*, Vol. 32, No. 1 (January 2009), pp. 48–49; and Kimberly Marten, *Warlords: Strong-Arm Brokers in Weak States* (Ithaca, N.Y.: Cornell University Press, 2012), pp. 164–169.

13. For a similar argument for a possible missed opportunity with the APC in late 2005, see Timothy S. McWilliams and Kurtis P. Wheeler, eds., *Al-Anbar Awakening*, Vol. 1, *American Perspectives: U.S. Marines and Counterinsurgency in Iraq, 2004–2009* (Quantico, Va.: Marine Corps University, 2009), pp. 125–129.

mote areas such as Lake Thar Thar. Prior to the surge, Anbar Province did have five times as many conventional forces per capita as Baghdad;<sup>14</sup> in the pockets where effective Sunni resistance emerged, however, only modest numbers of troops were needed to support them. Instead, U.S. support provided crucial capabilities that the Awakening lacked, such as armor, artillery, aviation, communications, technical intelligence, organizational discipline, and in many cases money. Furthermore, some key U.S. support, from the State Department but especially from the Central Intelligence Agency, had nothing to do with the military.<sup>15</sup> As with SOF counterterrorism, covert assistance contrasts starkly with surge doctrine as described in *FM 3-24* and should be treated as a separate explanatory factor (and potential policy prescription).

#### THE SURGE BEYOND ANBAR

Iraq is far more complex than Anbar, of course, and probably requires different explanations for different “wartime political orders” in the Kurdish North, Sunni West, Shiite South, and ethnically mixed Baghdad regions.<sup>16</sup> Biddle et al. make little allowance for regional variation, however. Instead, they advocate synergy as a blanket explanation for the drop in violence. Violence patterns and U.S. tactics, however, exhibited variation temporally and spatially.

Some units used surge doctrine before the surge, with uneven results. A study of Anbar and Ninewa Provinces found that, before 2007, units “had already built successful COIN competencies and were experiencing battlefield successes.”<sup>17</sup> One cavalry squadron commander remarked bluntly, “The only significant difference between what we did in 2006 (and before) as compared to 2007 onward is the use of combat outposts. . . . But their role in bringing about the lowered levels of violence in 2007 is vastly overstated.”<sup>18</sup>

Other units did not use surge doctrine even after the surge, again with uneven results. In rural Diyala, surge forces conducted a series of large-scale firepower-intensive operations with martial names reminiscent of Vietnam.<sup>19</sup> In one instance, by no means unique, villagers near Khan Bani Sad indicated that U.S. troops remained in a combat outpost for only a month or so before moving on; AQI returned almost immedi-

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14. Lee, “Iraq Order of Battle Data.”

15. Former Director of Central Intelligence Michael Hayden confirmed CIA involvement in the Awakening in 2008. See Stephen Manning, “CIA Chief: Military Strikes Offer Lessons,” Associated Press, September 17, 2008. For additional discussion of CIA involvement, see Marten, *Warlords*, p. 173; Timothy S. McWilliams and Kurtis P. Wheeler, eds., *Al-Anbar Awakening*, Vol. 2, *Iraqi Perspectives: From Insurgency to Counterinsurgency in Iraq, 2004–2009* (Quantico, Va.: Marine Corps University, 2009), pp. 57, 122, 206–208; and Seth Jones, *Hunting in the Shadows: The Pursuit of al Qa’ida since 9/11* (New York: W.W. Norton, 2012), p. 255.

16. Paul Staniland, “States, Insurgents, and Wartime Political Orders,” *Perspectives on Politics*, Vol. 10, No. 2 (June 2012), pp. 243–264; and Jon R. Lindsay and Roger Petersen, *Varieties of Insurgency and Counterinsurgency in Iraq, 2003–2009*, CIWAG Case Study Series, 2011–12 (Newport, R.I.: U.S. Naval War College, Center for Irregular Warfare and Armed Groups, 2012).

17. Russell, *Innovation, Transformation, and War*, p. 2.

18. Quoted in Judah Grunstein, “The Limits of the Surge: An Interview with Gian Gentile,” *World Politics Review*, April 11, 2008, <http://www.worldpoliticsreview.com/articles/1924/the-limits-of-the-surge-an-interview-with-gian-gentile>.

19. In addition to the two operations that Biddle et al. mention—Iron Reaper and Iron Harvest—these include Arrowhead Ripper, Iron Hammer, and Lightning Hammer I and II. These operations were part of larger corps-level operations, such as Phantom Thunder and Phantom Phoenix.

ately after the troops' departure—hardly a recipe for protecting SOIs.<sup>20</sup> In 2007 roughly three to four times as many civilian deaths from U.S. airstrikes were reported as in 2006—hardly the mark of a universal embrace of tactics centered on protecting the population.<sup>21</sup> Yet despite heavy casualties from AQI retribution and U.S. firepower, many Iraqi citizens still fought on with U.S. support. Some collaboration predated surge operations and persisted through them, as in Muqdadiah, where Multinational Force–Iraq reported the creation of a successful “neighborhood watch” in March 2007, nearly a year earlier than Biddle et al. report the standup of SOIs in February 2008.<sup>22</sup>

The reality of force employment in Iraq is therefore vastly more complicated than the Manichean story Biddle et al. tell of the arrival of General Petraeus and FM 3-24. This attribution of sweeping change in counterinsurgency campaigns to a single commanding figure is not only common, but wrong.<sup>23</sup> Both before and after Petraeus assumed command in Iraq, there was substantial variation in tactics, yet SOIs and their precursors still bloomed across Iraq.<sup>24</sup> These disparate and sometimes conflicting efforts do not add up to a coherent causal or empirical explanation of surge doctrine or numbers. Moreover, these episodes point toward the importance of local Iraqi decisions rather than simply U.S. tactical choices in determining the progress of the war.

#### THE IRAQI CALCULUS

If U.S. actions alone are insufficient to explain success, as the synergy hypothesis correctly implies, then Iraqi actions become critical for an explanation. Biddle et al. have little to say, however, about specific Iraqi strategic choices, how important they were, or indeed whether they even required extensive U.S. encouragement. Methodologically, Biddle et al. fail to adequately measure or infer the causes and consequences of Iraqi

20. Interviews by Austin Long, Forward Operating Base Warhorse, Iraq, November 27–28, 2007.

21. Nir Rosen reports 250 casualties in 2006 and more than 940 in 2007. Rosen, *Aftermath: Following the Bloodshed of America's Wars in the Muslim World* (New York: Nation Books, 2010), p. 227. Iraq Body Count (IBC) reports 254 casualties in 2006 and 681 in 2007. IBC, <http://www.iraqbodycount.org>. Biddle et al. reference this heavy firepower outside Baghdad in a footnote (p. 38 n. 70), but they say nothing about its meaning for their argument about surge tactics.

22. Multinational Force–Iraq, “Neighborhood Watch Program Shows Success in Muqdadiah District,” press release, April 8, 2007. The terms “neighborhood watch” and “concerned local citizens” were used to describe cooperating armed groups before “Sons of Iraq” (SOI) was coined. This raises methodological concerns about the correlation between the SOI standup that was reported by Biddle et al.’s interviewees and the diminution of violence. The term “SOI” was coined only as Multinational Force–Iraq struggled to come to grips with the emergence of the Awakening; this would give units an incentive to identify and to report SOI activity just as it became most salient, thus contributing to selection effects in Biddle et al.’s data. How many U.S. units had active neighborhood watches prior to newly reporting them as SOIs?

23. Joshua Rovner, “The Heroes of COIN,” *Orbis*, Vol. 56, No. 2 (Spring 2012), pp. 215–232.

24. Petraeus deserves some credit for helping to reverse the long-standing U.S. government resistance to supporting armed groups outside the formal Iraqi state, most of which had spilled U.S. blood. There had been outreach to insurgents as early as 2004, but efforts were often stymied by bureaucratic infighting among the military services, the CIA, and the State Department, as well as a general U.S. reluctance to negotiate with those viewed as terrorists. By 2007 both of these problems had been ameliorated. On policy change and its implementation, see Marten, *Warlords*, pp. 156–161; Marc Lynch, “Explaining the Awakening: Engagement, Publicity, and the Transformation of Sunni Political Attitudes,” *Security Studies*, Vol. 20, No. 1 (Spring 2011), pp. 54–58; Rosen, *Aftermath*, pp. 279–281; and Jeanne Hull, “Iraq: Strategic Reconciliation, Targeting, and Key Leader Engagement,” Letort Papers (Carlisle, Pa.: U.S. Army War College, 2009).



calculations. They rely heavily on the accounts of U.S. personnel and on SIGACTS (significant acts) recorded by U.S. forces, but these sources reveal little about ethnic cleansing, factional fighting, systematic criminal violence, or third-party (Iranian) intervention that may have occurred with little relation to the disposition of U.S. troops. Nuances of Iraqi behavior that were inscrutable to U.S. forces on the ground would have remained so in Biddle et al.'s data. The authors' blindness regarding Iraqi incentives and activities leads them to overstate the efficacy of U.S. activity. We lack the space to explore alternative explanations that take Iraqi decisions seriously, but we will comment briefly on Biddle et al.'s insufficient handling of two of them: ethnic cleansing and intra-Shiite violence.

The authors' section on ethnic cleansing is the only explicit hypothesis in their article that deals with Iraqi Shiite decisionmaking. Biddle et al. state that "most of this literature advances cleansing and its burnout as an alternative to the surge" (p. 14). A more accurate rendering of the hypothesis is that unmixing enables defensible borders that abate a security dilemma, not that it causes spontaneous burnout.<sup>25</sup> The correct specification of this hypothesis is essential in that it points toward a very different sort of synergy whereby Sunni SOIs provided just such a defense. As Shiite militias gained ground in Baghdad, Sunnis in Baghdad, Diyala, and other mixed areas perceived a serious threat not only from the Shiite militias but also from the same AQI their kinsmen in Anbar faced. Sunnis sought to take advantage of U.S. firepower to beat back AQI, as well as to halt Shiite gains and improve their postwar bargaining position from bad to perhaps tolerable. Unmixing in the capital gave Anbari Sunnis strong motivation to ensure that ethnic cleansing stopped there. Thus in 2007, just as Petraeus was enabling U.S. forces to negotiate deals, Sunni insurgents caught between AQI, U.S. forces, and the Shiite militias were becoming desperate for aid. Supply and demand explains the explosive growth of SOIs outside Anbar in 2007: U.S. troops were finally allowed to make deals just as Sunnis were realizing they had no other options.<sup>26</sup>

Biddle et al. similarly underrate the importance of Iraqi factors in describing the origin of the Jaysh al-Mahdi (JAM) cease-fire announced by the organization's founder, Muqtada al-Sadr. They mention JAM's struggle with another militia, the Badr Brigade, almost in passing, instead arguing that Sadr's fears of "another beating from the coalition" (p. 26) led to the cease-fire. Most observers at the time, however, argued that the cease-fire sprang directly from the culmination of months of JAM-Badr conflict in August 2007, with a major battle in Karbala and smaller battles elsewhere.<sup>27</sup> Coalition forces did not play a substantial role in the Karbala battle, further underscoring that if Sadr feared anything it was his fellow Shiites, not the surge.<sup>28</sup> Yet even this cease-fire

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25. Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security*, Vol. 20, No. 4 (Spring 1996), pp. 136–175.

26. For a similar argument, see Rosen, *Aftermath*, pp. 237–238. Biddle has also made a similar argument, so it surprising that it does not appear in the article. See Stephen Biddle, "Stabilizing Iraq from the Bottom Up," testimony before the U.S. Senate Foreign Relations Committee, April 2, 2008, pp. 4–5.

27. Ambassador Ryan C. Crocker noted, "Recent attacks by elements of the Iranian backed Jaysh al-Mahdi on worshipers in the holy city of Karbala have provoked a backlash and triggered a call by Muqtada as-Sadr for Jaysh al-Mahdi to cease attacks." Crocker, "Report to Congress on the Situation in Iraq," September 10, 2007, p. 5.

28. See Stephen Farrell, "50 Die in Fight between Shiite Groups in Karbala," *New York Times*, Au-



was tenuous, as JAM was fragmented and attacks on coalition forces by so-called JAM Special Groups continued. The extent to which these groups were independent of Sadr is unclear, with some being much closer to Iranian covert operatives than to Sadr. Other groups may have been a mechanism for continuing attacks while giving Sadr plausible deniability. The mere fact that attacks on coalition forces continued (albeit at a reduced rate) while intra-Shiite fighting contracted much more substantially indicates that fear of the coalition was not the primary driver of the behavior of Shiite militias.<sup>29</sup>

Further, Biddle et al. do not discuss Iran's role in the war, including in their calculations of Shiite militias. Indeed, the very word "Iran" does not appear in the article, despite looming large in the minds of Shiite and Sunni Iraqis as well as U.S. forces. As General Petraeus remarked in congressional testimony in September 2007, "[N]one of us earlier this year appreciated the extent of Iranian involvement in Iraq, something about which we and Iraq's leaders all now have greater concern."<sup>30</sup> Although the exact role of Iran may remain opaque, to avoid even mentioning the most significant external actor in Iraq (apart from the United States) in a discussion of the decisionmaking of its Iraqi allies seems misguided at best and highlights the problems of the U.S.-centric version of synergy proposed by Biddle et al.

We would like to suggest an alternative (and more dismal) synergy argument, not about an Awakening-empowering surge, but rather about a Sunni Awakening that bolstered the "success" of Shiite ethnic cleansing. Although this is just one alternative explanation among many, we are not alone in providing a hypothesis on violence in Iraq based primarily on changing local and national political dynamics, with an important but distinctly secondary U.S. role, which in turn had little to do with either surge reinforcements or surge tactics. Douglas Ollivant, chief of plans for Multinational Division Baghdad from 2006 to 2007, has advanced a similar argument.<sup>31</sup> Both Ollivant and Gian Gentile have separately argued a counterfactual that if Petraeus's predecessor, Gen. George Casey, had stayed in place and received the substantially smaller number of troops he requested, the decline in violence witnessed in 2007–08 would still have taken place.<sup>32</sup> Based on evidence from Anbar, we would argue an even stronger counterfactual, that no additional troops were needed. Without challenging these and similar counterfactuals directly, Biddle et al. cannot claim to have "tested the surge."

#### CONCLUSION

The strategic realities of Iraq, read from a non-U.S.-centric viewpoint, featured mafia-like competition for political control at local levels and a sectarian security dilemma at

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gust 29, 2007; and Damien Cave, "Iraqi Calls for Broader Cease-Fire," *New York Times*, September 1, 2007.

29. For example, lethal rocket attacks were part of a major indirect fire offensive against coalition bases timed around Ramadan in 2007, which Austin Long witnessed at both Camp Victory and Camp Bucca, near Basra. See also David Rising, "Gen: Iran-Made Rocket Used in Victory Attack," *Army Times*, September 13, 2007.

30. Gen. David H. Petraeus, "Report to Congress on the Situation in Iraq," September, 10–11, 2007, p. 7. See also Petraeus's repeated emphasis of the importance of Iran, in *ibid.*, pp. 1–2, 4.

31. Douglas Ollivant, "Countering the New Orthodoxy: Reinterpreting Counterinsurgency in Iraq" (Washington, D.C.: New America Foundation, 2011).

32. See *ibid.*, pp. 6–7; and Gian Gentile, *Wrong Turn: America's Deadly Embrace with Counterinsurgency* (New York: New Press, forthcoming). For Casey's perspective on troop levels, see George Casey, *Strategic Reflections: Operation Iraqi Freedom July 2004–February 2007* (Washington, D.C.: National Defense University Press, 2012), pp. 113–115, 142–146.

the national level. The war followed an idiosyncratic course whereby the United States first lost ground to a coalition of nationalist and fundamentalist Sunnis in Anbar even as sectarian violence worsened; the United States then “won” only as nationalists borrowed U.S. combat power to vanquish one set of rivals (AQI) and shore up defenses against another (Shiite militias). Biddle et al.’s surge and surge-synergy hypotheses, by contrast, not only stretch the concept of a surge beyond recognition; they also impute too much efficacy to tactical measures operating with insufficient regard to the dynamic structure of power in factional war.

Biddle et al. are right to be skeptical about the power of troops or tactics to achieve results beyond Iraq. As they write, “Afghanistan has not produced a movement analogous to the Awakening, and without this one should not expect 2007-like results” (p. 37). At the same time, they have overplayed its ability to explain progress in Iraq in 2007 and 2008. This view is dangerous for defense policy, because it is a recipe for “overkill” in irregular warfare or civil war. It suggests that, were an Afghan Awakening to take place, then large numbers of U.S. forces would be needed to support it, so the policy of reducing troop levels would preclude such a movement by Afghans. Awakenings do not necessarily need surge numbers to arise, however, and they do not necessarily require surge tactics to thrive. By contrast, they may need powerful patrons to enhance their firepower and improve their political position. Intervention in civil war is a dangerous gamble in the best of circumstances. Policymakers determined to intervene are advised to look toward ruthless negotiation to improve the odds rather than focusing on tactics or troop numbers.

—Jon R. Lindsay  
La Jolla, California

—Austin G. Long  
New York City, New York

*Stephen Biddle, Jeffrey A. Friedman, and Jacob N. Shapiro Reply:*

We welcome the opportunity to respond to Jon Lindsay and Austin Long’s and John Hagan, Joshua Kaiser, and Anna Hansen’s comments on “Testing the Surge.”<sup>1</sup> Our critics disagree not only with us, but also with each other: Lindsay and Long argue that we understate the Sunni Awakening’s importance; Hagan et al. think we exaggerate it. In fact, we got it about right.

ON LINDSAY AND LONG

Lindsay and Long advance two main arguments.<sup>2</sup> First, they say we conflate the role of reinforcements and doctrine in the 2007 surge in Iraq. They can be hard to pin down on the nature of the asserted error and its consequences, however. At times, they imply

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1. Stephen Biddle, Jeffrey A. Friedman, and Jacob N. Shapiro, “Testing the Surge: Why Did Violence Decline in Iraq in 2007?” *International Security*, Vol. 37, No. 1 (Summer 2012), pp. 7–40. Subsequent references appear parenthetically in the text.

2. Jon R. Lindsay and Austin G. Long, “Correspondence: Assessing the Synergy Thesis in Iraq,” *International Security*, Vol. 37, No. 4 (Spring 2013), pp. 181–189. Subsequent references appear parenthetically in the text.

that 2007-style doctrine was necessary to explain the reduction in violence, but that the reinforcements were not (p. 182). At other times, they imply that the only change necessary was a command decision to support realigning Sunnis, with neither reinforcements nor doctrinal change required (pp. 185, 188). At still other times, they imply that no U.S. changes were needed, and that the Awakenings alone would have sufficed with pre-2007 U.S. behavior (pp. 188–189). Second, they say we misreport the history of pre-2007 “proto-Awakenings.” We see these as failures in the absence of U.S. support. Lindsay and Long’s assessment varies: sometimes they see the proto-Awakenings as success stories showing how U.S. assistance was unnecessary (p. 185); at other times they see the proto-Awakenings as failures, but only because the United States chose not to offer assistance (pp. 183–184).

Several points bear consideration in response. We do not, for example, conflate reinforcements and doctrine. On the contrary, we explicitly discuss their differences (see, e.g., pp. 8, 21–23, and 39).<sup>3</sup> Yet we do not try to parse 2007’s reinforcements, doctrine, and command decisions to assign relative causal weights, as Lindsay and Long would like us to do. This is because there is no evidence that could sustain a finding on this, as we state on page 39. A rigorous analysis would require data on variance in U.S. tactics in 2006 and 2007 and a source of variation in both that was independent of local conditions. In fact, our interviewees reported little variation across units in 2007, and there are no theaterwide data on tactical behavior. Without such data, any finding would be speculation. Lindsay and Long present no such data. Instead, they repeat two isolated examples, which we present ourselves, showing surge-like methods prior to 2007 (Anbar and Tal Afar), and add one more (Gian Gentile’s assessment of his own cavalry squadron). Increasing the sample size from two brigades to two and one-third hardly establishes a theaterwide pattern for the dozens of brigade rotations in Iraq before 2007. There is a clear, albeit subjective, consensus in the secondary historiography and contemporary journalistic accounts that Sean MacFarland’s methods in Anbar and H.R. McMaster’s methods in Tal Afar were uncommon before 2007.<sup>4</sup> Gentile believes otherwise, but has presented no evidence beyond his account of his own unit.

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3. We are also explicit on the doctrinal changes we see as important (pp. 21–23). Lindsay and Long, however, lump us together with *FM 3-24*, the U.S. counterinsurgency manual, assuming with no basis that we endorse provisions in the manual about which we say nothing. In fact, there are only two references to *FM 3-24* in our article, both of which critique it (see p. 25 n. 46 and p. 38 n. 66). Nowhere in our article, for example, do we argue, as Lindsay and Long claim, that firepower is detrimental to success in COIN (pp. 182, 189); on the contrary, we argue exactly the opposite in our conclusions (p. 38). Below, we defend only what we actually wrote.

4. See, for example, Michael R. Gordon and Bernard E. Trainor, *Endgame: The Inside Story of the Struggle for Iraq, from George W. Bush to Barack Obama* (New York: Pantheon, 2012). Gordon and Trainor note that “[t]he Anbar Awakening itself took off in 2006 after Colonel Sean MacFarland’s troops, in an exception to the drawdown strategy, pushed into enemy-held areas to stay” (p. 687); and that “[H.R. McMaster’s unit in Tal Afar] had been successful because it had used time-tested counterinsurgency tactics. But its approach was the exception rather than the rule. The sort of counterinsurgency tactics McMaster employed . . . were not seen as a core principle of the campaign plan. The overarching strategy was still to be shrinking the American military footprint and handing over to Iraqi forces” (p. 169). For similar assessments, see, inter alia, Thomas E. Ricks, *The Gamble: General David Petraeus and the American Military Adventure in Iraq* (New York: Penguin, 2009), pp. 61, 95; Bob Woodward, *The War Within: A Secret White House History, 2006–2008* (New York: Simon and Schuster, 2008), p. 36; and Linda Robinson, *Tell Me How This Ends: David Petraeus and the Search for a Way Out of Iraq* (New York: PublicAffairs, 2009), pp. 14, 38–39, 122–123.

We clearly state (p. 23 and p. 22 n. 40) that exceptions existed in which apparently unusual commanders used surge-like methods before 2007; however, we also show that these exceptions do not challenge the synergy thesis. Among other reasons for this finding, note that the troop densities in Anbar and Tal Afar were far higher than those available elsewhere at the time: as Lindsay and Long themselves note, Anbar had five times as many troops per capita as Baghdad in 2006; troop density in Tal Afar was more than three times higher than in the theater as a whole when it was cleared in 2005.<sup>5</sup> This hardly demonstrates that reinforcements were unnecessary in 2007. We would like to go further than the claim we advance in the article, which states only that some combination of reinforcements and behavioral change was necessary (but insufficient) for 2007's reduction in violence, but we cannot. There is already plenty of speculation in this debate—our aim was to contribute an analysis based on systematic evidence. The evidence permits only the claim we made. Neither we nor Lindsay and Long possess the data needed to go beyond this.

Lindsay and Long also critique our histories of the four so-called proto-Awakening that preceded the successful 2006–07 Anbar Awakening, yet they present no evidence that contradicts our findings. For two of the four cases, their assessment is actually the same as ours: they acknowledge that “the Hamza Brigade received little U.S. support and was thus unsuccessful,” and they agree that the Anbar People’s Council (APC) “received no U.S. support” and was then “decimated” by al-Qaida in Iraq.<sup>6</sup> We concur: see pages 19 to 20—indeed, this is precisely our point. Again Lindsay and Long would like us to go beyond the evidence to specify whether this lack of support was the result of troop count or behavioral choice, but our argument on pages 19 to 20 is the most we believe we can sustain, and Lindsay and Long provide no evidentiary basis for going further.

For the other two cases, Lindsay and Long sometimes appear to argue that these were success stories rather than failures. They write, “[T]he Albu Nimr continued to cooperate with Special Forces around Hit, and by 2006 they had convinced the tribe to contribute hundreds of men to police recruiting drives,” and “[the Desert Protectors’] crucial success occurred two years before the surge” (p. 184). At other times, they seem to accept our view that these were failures, but speculate that failure could have been averted if only nearby U.S. forces had not been “reluctant to support a tribal engagement strategy” (p. 184).

Let us consider the first interpretation, that the Nimr and the Desert Protectors were actually successes. What do Lindsay and Long believe they succeeded at? The outcome we seek to explain is a theaterwide reduction in violence; our claim is that the Awakening’s rapid spread across most of threatened Iraq played a critical role in this, and that it would not have happened without the surge. None of the pre-surge realignment attempts spread, and none even survived as an organized entity for more than six

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5. On troop strength in Tal Afar, see Carrie Lee, “Iraq Order of Battle Data—District Level,” dataset, 2011.

6. Lindsay and Long also discount the APC’s demise on the grounds that it occurred only six months prior to the beginning of the successful Anbar Awakening (pp. 184–185); it unclear why this matters. These were separate movements, with separate memberships from different tribes, and the APC ended operations before Sheikh Sattar’s Anbar Awakening began. There is no obvious reason why events six months prior to Sattar’s standup should be any more or less pertinent than any others, nor do Lindsay and Long offer one.

months. How does this demonstrate that realignment without the surge would have sufficed to stabilize Iraq? Uprisings that disbanded without spreading and did so in the midst of heavy ongoing violence in Iraq offer no reason to suppose that the 2006–07 Awakening would have survived, caught on, and spread without the surge, which offered U.S. military assistance to the Awakening but not to its predecessors. This is not to disparage the Nimr, the Desert Protectors, or the Americans who worked with them. Nor does this mean their labors yielded nothing. AQI did experience a setback for a time in the Desert Protectors' zone, and some survivors of the Nimr's initial uprising later contributed to coalition efforts elsewhere. One can choose to call these "successes" or not depending on one's criteria. For our purposes, however, what matters is whether these cases are consistent with the specific causal claim we advance for the determinants of theaterwide violence reduction—and nothing in the Nimr or Desert Protectors' experience suggests that the 2007 violence reduction could have been achieved without the surge.

Let us then consider the second interpretation, that the proto-Awakenings were indeed failures, but only because idiosyncratic U.S. command decisions denied them readily available assistance. Lindsay and Long speculate that the Nimr, in particular, could have produced 2007-like results if other U.S. forces in the area had come to their aid. Well, yes—after all, our whole point is that realignments could succeed if supported by enough U.S. troops employing appropriate doctrine. So of course if those conditions had been met, then we would expect the Nimr realignment to have succeeded. The conditions were not met, as we all agree. The only way this finding contradicts the synergy thesis is if one makes a series of demanding but unstated assumptions for which Lindsay and Long provide no evidence: (1) that these other U.S. troops were using surge-like methods already; (2) that these troops were conducting unimportant missions from which they could have been safely diverted to assist the Nimr without any offsetting opportunity costs in the absence of other reinforcements; (3) that the critical barrier to this diversion was unimaginative, easily reversible decisionmaking by senior commanders; and (4) that this pattern also characterized the rest of the theater prior to 2007. Of these four assumptions, the last is the most important: the first three could all be true; but if the last were false, then the synergy thesis would still be sound. In principle, one could test the first three assumptions by identifying the units involved and interviewing their commanders. Testing the fourth, however, would be much more difficult.<sup>7</sup> Lindsay and Long's assumption here conflicts with the historical and journalistic consensus, and to overturn that on the basis of evidence would require constructing a new large-*n* dataset on the distribution of U.S. military methods over time and space in Iraq from at least 2006 to 2008. As we note, these data do not exist. Without them, Lindsay and Long's inference from the Nimr case lacks any basis in systematic evidence.

In addition to these primary arguments, Lindsay and Long chide us for insufficient attentiveness to "Iraqi agency" and Iraqi motives, inattentiveness to the complexity of varying local conditions in Iraq, and what they see as a straw man version of

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7. Note also that the second requirement is especially implausible: the Battle of Fallujah was ongoing at the time, leaving U.S. forces elsewhere in Anbar (where the Nimr were operating) stretched to the limit. See U.S. Army Military History Institute, Iraq Surge Collection, audio file 48; and our discussion on p. 19 of our article, on how shifting U.S. forces to Fallujah undermined Nimr realignment.

the Awakening literature (pp. 183, 187–189). These points warrant at least a brief response. The first claim is puzzling. We obviously do not think Iraqi decisions were unimportant—in fact, the whole point of our synergy discussion is that Sunni realignment was a product of Iraqi decisions and was not merely an epiphenomenal consequence of the surge (see, especially, p. 35). We do not, however, try to explain the Awakening’s motives—our purpose is to explain its consequences. If something about its motives undermines our analysis of its consequences, then this would be a problem, but Lindsay and Long identify no such logic flaw. As for Iraq’s complexity, we suspect that few readers intrepid enough to plow through our figure 1 and table 1 would echo Lindsay and Long’s assessment that our article fails to present enough local variations in Iraqi violence. On the contrary, we argue that it is precisely the richness of local variability in Iraq that permits us to sustain causal analysis: not only do we embrace the complexity of this theater, but our method requires it to reach a finding (p. 13). This is why we provide an eyestrain-inducing array of almost forty different violence trend plots for different localities in Iraq, why we analyzed these trends across several different regional and temporal subsets, and why we interviewed seventy different officers who served in twenty-two of the twenty-five most violent districts in Iraq. Any analysis could always delve deeper into the variability of social reality than it does. It is far from clear, however, that valid analysis here requires even more local disaggregation than our separate treatment of thirty-eight different areas of operation (AOs) across the vast majority of threatened Iraq during more than two years of warfare. As for our treatment of the literature, it is worth noting that Lindsay and Long find remarkably little real difference between our analysis of the proto-Awakening and theirs (see the discussion above), yet they believe we mischaracterize Austin Long’s *Survival* article cited in our study; we stand by our assessment of the literature, including but not limited to the Long piece.<sup>8</sup>

ON HAGAN, KAISER, AND HANSEN

Whereas Lindsay and Long think we understate the Awakening’s importance, Hagan et al. think we overestimate it.<sup>9</sup> They base this claim on two main arguments: that our

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8. In this respect, we find Lindsay and Long’s treatment of the literature interesting. On page 182 of their letter, for example, they express surprise that Stephen Biddle, the author of *Military Power: Explaining Victory and Defeat in Modern Battle*, would decline an opportunity to showcase the irrelevance of “troop preponderance . . . for battlefield success,” as they claim *Military Power* concludes. Yet the book explicitly excludes counterinsurgencies such as Iraq from its explanatory domain. See Biddle, *Military Power: Explaining Victory and Defeat in Modern Battle* (Princeton, N.J.: Princeton University Press, 2004), p. 6. Nor does the book say what Lindsay and Long claim it does, even for the conflicts it covers. Figures 4.3 on p. 76 and A-9 on p. 227 of the book treat Lindsay and Long’s claim directly, but present a rather different finding. Similarly, Lindsay and Long state there is a contradiction between the notion that the surge helped to reduce violence in Iraq and Jeffrey A. Friedman’s recent article on manpower in counterinsurgency; but in that article, Friedman explicitly states that large-*N* patterns cannot be used to draw conclusions about individual cases such as Iraq. See Friedman, “Manpower and Counterinsurgency: Empirical Foundations for Theory and Doctrine,” *Security Studies*, Vol. 20, No. 4 (November 2011), p. 578. Either way, both authors believe strongly in restricting one’s findings to what the evidence will support.

9. John Hagan, Joshua Kaiser, and Anna Hanson, “Correspondence: Assessing the Synergy Thesis in Iraq,” *International Security*, Vol. 37, No. 4 (Spring 2013), pp. 173–181. Subsequent references appear parenthetically in the text.



quantitative analysis overestimates the contribution of the Sons of Iraq (SOI), and that we overlook the role played by Muqtada al-Sadr's cease-fire declaration.

Their critique of our quantitative findings rests partly on a misunderstanding of the role that regression analysis plays in our argument, partly on a highly selective reading of our article that ignores analysis that directly contradicts their claims, and partly on an idiosyncratic application of assessment criteria to an eight-AO subset of the thirty-eight AOs that we consider, but to none of the other thirty (p. 177). As for the role of regression analysis, Hagan et al. seem to believe that the ordinary least squares (OLS) slope calculations in table 1 represent a typical statistical model to explain violence by reference to partial correlations across some set of independent variables, with our findings emerging from coefficients and standard errors calculated for those variables. Hence, on pages 175 and 176, they chide us for failing to include a variety of controls, and for failing to use statistical techniques they think would be better suited to the data (but which they do not actually use themselves). As we state, however, this is not what we did. Our study is not a statistical analysis by partial correlation—because the relationship between the available data and the arguments we address does not permit this. As we point out, “First, many of the arguments [in the literature] have no obvious implications for variance between observable factors at fixed, comparable geographic units (e.g., district-months). Second, there is no systematic theaterwide data on important variables such as Awakening forces' availability. Third, and most important, there is no viable source of plausibly exogenous variation in critical variables such as coalition force levels or operational methods” (p. 13 n. 19).

Instead, our article uses univariate OLS only as a simple, consistent, objective means of computing pre- and post-SOI violence trends in individual AOs. Violence in AOs varied month to month. We claim that this violence fell faster after SOI standup; we thus need some way to measure how fast violence was falling (if at all) prior to and after standup in the presence of this variation. One could do this subjectively by eyeballing individual AOs' time-series and trying to hand-fit some intuitive shape to the noisy violence data à la carte; this is essentially what Hagan et al. do for their selected eight AOs. This process is fraught with opportunities for motivated bias, however. As an illustrative example, note how subtle ad hoc changes in the start and end date for the time-series in any given AO taken in isolation can change the findings for that AO: for Al Hillah, for example, if one considers only the two-month interval before and after SOI standup, the result is a dramatic reversal of the trend; but if one chooses a four-month interval, the result is essentially no change; for Rawah, a two-month interval shows essentially no change, but a five-month interval shows a dramatic reversal. This is not surprising. With data displaying any meaningful variance, ad hoc alterations in features such as start and end dates can create almost any finding—the process is not unlike asking different people what they see in a Rorschach inkblot.

To defend against the danger of imposing our preferences on the data, we chose a consistent, objective means of measuring slopes: OLS. And to avoid tailoring the results to fit our preferences, we treat all thirty-eight AOs using the same cutoff for the number of pre- and post-SOI data points to include—we did not try to custom-tailor the cutoff AO-by-AO in a way that might have subtly empowered us to tune the analysis for more convenient results. We then reran our results for all possible cutoff variations between the theoretical minimum of two months (i.e., two data points) and twelve months, for both symmetric pre- and post-SOI intervals (i.e., the same interval before and after)



and asymmetric ones. In none of these variations did the results disconfirm synergy. In fact, we picked the three-month cutoff to report in table 1's detailed presentation because it produced the weakest synergy finding of any possible value. Nor did nonlinear fits make a difference: we tested the standard candidates, and none of these changed the results, either. This is all described, explicitly, in note 52 on page 28 and note 56 on page 33. We then considered eight different subsets of the thirty-eight AOs selected according to various criteria (most violent, most populated, earlier standups, and so on), and found that the confirmation rate rose for every subset we could think of that might connote special explanatory significance—these results are presented in table 2. Our synergy finding was sustained in every systematic variation we could think of that made sense and was appropriate to the data. All of this was designed to make our analysis as robust and objective as possible, and is described in detail in the article.<sup>10</sup>

By contrast, Hagan et al. use highly idiosyncratic coding rules and apply them selectively. By no explicit criterion, they reconsider only eight of thirty-eight AOs, all of them synergy-confirming in our analysis; apply ad hoc, subjective criteria for evaluating trends in only these eight; declare them now disconfirmatory; then observe, unsurprisingly, that if one now excludes eight of twenty-four confirmatory cases, the residual is a lot less confirmatory (pp. 176–178). Because they consider only confirmatory cases, they exclude any possibility that a new rule, consistently applied, would reverse disconfirmations elsewhere—their procedure guarantees that a new coding rule can reduce the confirmation rate but never increase it, regardless of the rule (or the data). Moreover, because their new coding rules are subjective and custom-made for the AOs they chose to reexamine, it is impossible to recode the other thirty to see how the results would change for the data as a whole, given that we cannot know what Hagan et al.'s coding rules would be for any other AO. Given the potential for bias, this is the kind of ad hoc approach that we sought to avoid. If one instead applies any sensible, straightforward coding rule across all thirty-eight cases—not just a handpicked subset of confirmations—then the results are the opposite of what Hagan et al. claim, as we demonstrate in note 52 on page 28. In fact, all rules we tested contradict Hagan et al., even when applied to a substantively meaningful subset of the thirty-eight, as we demonstrate in table 2. Hagan et al. reach different results by applying ad hoc coding rules on a selective basis, yet chide us for selectivity when we consider all the data available to us using a wide variety of different, but consistent, transparent, and objective, standards.<sup>11</sup>

Hagan et al. also argue that we overlook the role of Muqtada al-Sadr's cease-fire,

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10. Hagan et al. repeatedly chide us for failing to consider possibilities that are in fact analyzed and reported in our article. They claim that we “gloss over the nonlinear ups and downs” of the data (p. 176), yet we explicitly test for nonlinearities and reject this (p. 33 n. 56). They say that we overstated SOIs' impact by including AOs where SOIs stood up when violence was already “trending downward” (p. 175), yet we test for this in table 2 and find exactly the opposite: if we exclude such AOs and consider only ones where violence was rising at standup, this strengthens support for synergy. They say we should have excluded AOs where SOIs stood up when “violence appears to have already stabilized” (pp. 175, 176), yet we test for this, too, in table 2, and again find the opposite: when we consider only the AOs that were more violent at SOI standup, the results again show stronger support for synergy, not weaker. We are frankly at a loss to understand how Hagan et al. could have overlooked so much of the article's explicitly reported findings.

11. Regarding Hagan et al.'s assertion that we present evidence selectively (p. 174), note that our sample covers twenty-two of the twenty-five districts responsible for 90 percent of the reduction in

which they see as a greater contributor to the reduction in violence than the SOI movement's stand-down of the Sunni insurgency (p. 179). They apparently missed our discussion of the Sadr cease-fire (see pp. 25–26 of our article).<sup>12</sup> We hardly ignore it. We do, however, disagree with Hagan et al.'s characterization, on two important scores.

First, whereas Hagan et al. assume that Sadr's decision was unrelated to the surge, we do not. Hagan et al. see Sadr's announcement as a response solely to the intra-Shiite fighting between his Jaish al-Mahdi (JAM) and the rival Badr Corps militia in Karbala. We, too, note the role of the Karbala fighting (p. 25), but we see this as one among several contributing factors—including the JAM's growing factionalism and criminality, but also the surge, the widespread SOI standup by late summer 2007, and the JAM's consequent military prognosis. By August 2007, the combination of the surge and the Sunni stand-down via the SOI movement was freeing large numbers of U.S. combat troops for use against the JAM. At the same time, factional conflict within the JAM itself was threatening Sadr's control of his own militia. Sadr had fought the U.S. military twice before, and knew that a repeat would mean heavy casualties. In the past, he could rely on his popularity among Shiites to recruit replacements, but the JAM's increasing factionalism and criminality were undermining his ability to do so. Another bruising battle with the Americans could thus mean a permanent loss of combat power; we argue that he responded by choosing a cease-fire rather than resisting the coming U.S. offensive. Hagan et al., by contrast, imply that neither the surge nor the Sunni stand-down influenced Sadr's decision, which was apparently inspired entirely by the Karbala fighting with the Badr Corps. Of course, there is no hard evidence on Sadr's true motives: his decisionmaking is famously mercurial, and we are aware of no credible interview evidence to present his own account. All assessments are therefore necessarily circumstantial. Yet it is worth noting that the Karbala fighting Hagan et al. see as the exclusive cause for Sadr's cease-fire cost him all of 52 fatalities and 279 injuries. By comparison, the JAM lost perhaps six times that many in each of its previous battles with the Americans.<sup>13</sup> Did Sadr really see moderate losses to a lightly armed, ill-trained

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violence in 2006 that we seek to explain, and it represents no act of selection on our part: as we state, our only criterion for the interviews that produced this coverage was that the interviewee had served in Iraq sometime between 2006 and 2008—any officer with pertinent service who responded to our solicitation was interviewed (p. 12 n. 15). Hagan et al. also ask to see analysis of AOs without SOIs, but we provided this for all thirty-eight of the AOs we covered: our whole point is to compare, for each AO, violence without SOIs and violence with them. In some cases, SOIs stand up early, in others late—and sometimes very late (the latest SOI standup in table 1 is May 2008). None of the thirty-eight provide AOs where SOIs never stood up, however. This is not surprising, given that by spring 2008 there were more than 200 separate SOI groups operating across most of central Iraq. Nor can one establish permanent absence of an SOI group from an interviewee who reports seeing none during his tour, given that interviewees rarely have detailed knowledge of events after their return home. It is not clear what we could learn about the impact of the Awakening and surge from an AO that never saw an SOI that we would not learn from AOs that went SOI-free through spring of 2008, but in practical terms, none of our interview evidence established a permanent absence. This did not result from any imposed selection criterion.

12. Lindsay and Long seem to have missed this discussion as well: they assert, incorrectly, that the "section on ethnic cleansing is the only explicit hypothesis in their article that deals with Iraqi Shiite decisionmaking" (p. 187).

13. See, for example, Michael Howard, "U.S. Troops Kill 300 in Najaf Raid," *Guardian*, August 6, 2004; Patrick Cockburn, *Muqtada al-Sadr, the Shia Revival, and the Struggle for Iraq* (New York: Scribner, 2008), pp. 149, 155, 162; Thomas Ricks, *Fiasco: The American Military Adventure in Iraq* (New York: Penguin, 2006), p. 338; and Ali A. Allawi, *The Occupation of Iraq: Winning the War, Losing the Peace* (London: Yale, 2007), pp. 274, 323, 332.

Badr militia as a bigger threat than a repeat of his own heavy casualties in previous fighting against a vastly better equipped, vastly more lethal U.S. Army that was now both reinforced and increasingly freed of the need to fight Sunnis, and was instead massing for a new battle with the JAM? Perhaps this U.S. threat never even entered Sadr's mind. If it did, however, then at least some of the violence reduction that Hagan et al. attribute to the Sadr cease-fire is in fact creditable to the ongoing combination of the surge and the SOI standup (see pp. 25–26).

Our other disagreement with Hagan et al. involves their assessment of the Sadr cease-fire's contribution to Iraq's overall violence reduction, which they see as primary; we see it as important but secondary. Hagan et al. cite a combination of poll results and the August–September change in IBC's (Iraq Body Count's) civilian fatality figures, both of which show major security improvements at about the same time as the cease-fire. They conclude from this that (1) the cease-fire caused the improvement; and (2) this improvement exceeded anything attributable to the Sunni Awakening or the surge or any combination of the two (pp. 178–180).

Among the many difficulties here are the confounding effects of everything else ongoing in Iraq at the time. Both the poll and the IBC data are national aggregates. Hence they combine experience in places where Sadr's JAM was operating and places where it was not. Sadr's cease-fire could not have reduced violence in places where he had no fighters, yet more than 25 percent of the August IBC fatality total is attributable to a single incident in an area where there were no JAM militiamen, an attack on August 14 by four bombers directed at Yazidi communities in Sinjar that killed between 516 and 525 people.<sup>14</sup> This event had nothing to do with the JAM; yet without this single outlier, the August–September change in IBC civilian fatalities falls from 978 to 462, and the apparent acceleration in the reduction of violence disappears: without the Sinjar attack, fatalities decline by 591 from July to August, and then by only 462 from August to September. Instead of a discontinuous plummet following the cease-fire, the data now simply extend an essentially continuous ongoing reduction that began in July, not August. Nor is the Sinjar episode the only potential problem with Hagan et al.'s causal attribution. In particular, the SOI movement was expanding rapidly during this same period, with what we believe to have been important effects in reducing violence. Eleven of the thirty-eight AOs we studied saw SOI groups stand up between June and September of 2007, and twenty-five, or almost two-thirds of the total, were operating by September. Hagan et al. imply that the entire 978-fatality drop in IBC's violence data is attributable to the Sadr cease-fire, with none resulting from the ongoing SOI-surge interaction we describe. This is implausible. Set aside the fact that at least half of this reduction occurred in places with no JAM fighters and thus cannot possibly be attributed to the cease-fire; Hagan et al. also assume that none of this reduction is attributable to a continuation of a preexisting stabilization trend that predated the cease-fire and thus could not have been caused by it. By contrast, SOI-surge synergy could in principle account for the entire reduction—and the analysis we document in table 1 of our article offers empirical support for our claim that synergy accounts for much of it.<sup>15</sup> This is not

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14. Iraq Body Count, incident k7225, <http://www.iraqbodycount.org>.

15. The polling data that Hagan et al. cite are even more problematic on this score (pp. 178–179). The poll covers two pertinent time intervals, February to August 2007, and September 2007 to March 2008. Each thus lumps together everything that happened to the respondents during the six months before Sadr's cease-fire and the seven months after it. Violence in Iraq fell radically be-

to say the cease-fire was irrelevant. On the contrary, we argue that it played an important role, which we see as part of the synergy causal logic, for the reasons we describe (pp. 25–26). Hagan et al., however, insist on a much more consequential and exclusive role for the cease-fire than this; the data do not support their more sweeping claim.

#### CONCLUSION

We stand by our analysis. Our critics sometimes want us to go beyond the available evidence. Sometimes they do so themselves. At other times, they mischaracterize the analysis we did perform, ignore it, or reanalyze our data using selective ad hoc procedures in lieu of the kind of consistent, transparent, objective rules we adopted to ensure against bias. In spite of this, it is striking how many of our conclusions our critics accept. Perhaps our most important finding is that the surge, while an important contributor, was insufficient to stabilize Iraq—and thus caution is warranted in assuming that similar methods will yield similar results elsewhere (pp. 10–11, 36–37). None of our critics disagree. Lindsay and Long’s concluding section agrees with our caution on generalizing from the Iraq case. And earlier in their letter, they agree that the surge played an “important” role in 2007 (p. 188). Hagan et al. concur that SOI-surge synergy produced “improvement” (p. 179). And though they offer no policy implications, we assume that they would likewise counsel caution in applying the Iraq case elsewhere. Nor do any of our critics think Iraq’s violence could have fallen without an important contribution from U.S. troops: Lindsay and Long think this contribution was possible without reinforcements; Hagan et al. think the U.S. contribution interacted chiefly with Sadr’s cease-fire rather than the Sunni Awakening; but all see a critical role for the U.S. military presence in 2007, and all see its role chiefly in interaction with decisions reached by Iraqis. Our critics are especially emphatic that the surge should not get full credit for Iraq’s violence reduction, but we never said it should—the actual analytical difference on cause and effect here, while real, is smaller than may meet the eye.

—*Stephen Biddle*  
Washington, D.C.

—*Jeffrey A. Friedman*  
Cambridge, Massachusetts

—*Jacob N. Shapiro*  
Princeton, New Jersey

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tween summer 2007 and spring 2008; one would certainly expect Iraqis to report feeling more secure by the end of the second interval—this tells us nothing about which of the many changes during those seven months were responsible. In fact, there is no evidence to support any particular decomposition of this change into contributing causes, yet Hagan et al. attribute all of it to the Sadr cease-fire and none of it to, say, surge-SOI synergy. Their evidence offers no support for this claim.