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**Authors' Response to Michael C. Horowitz's "Adoption Capacity and the Spread of Suicide Bombing: A Response to Andrea Gilli and Mauro Gilli, 'The Spread of Military Innovations: Adoption Capacity Theory, Tactical Incentives, and the Case of Suicide Terrorism,'** *Security Studies* 23:3 (2014): 513-547. Published on 13 January 2015 as http://issforum.org/ISSF/PDF/RE26.pdf

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Authors' Response by
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We thank Michael Horowitz for his response to our article, "The Spread of Military Innovations: Adoption Capacity Theory, Tactical Incentive and the Case of Suicide Terrorism."[2] We are glad for Horowitz's close reading of our work, and for the several insightful and constructive comments that he has offered. Such comments significantly contribute to the academic debate on the diffusion of military innovations and should drive further research in the field. However, Horowitz's response to our article fails to address the problems we originally raised. As a result, the conclusions we reached in our article are still valid: because of the problems in Horowitz's research design, we cannot

[1] We would like to thank for feedbacks and suggestions Jon Caverley, Ferdinando Monte, Steve Nelson, Costantino Pischedda and Anne Sartori. We are responsible for any remaining errors. We would also thank Diane Labrosse for the editorial assistance.

[2] Michael C. Horowitz, "Adoption Capacity and the Spread of Suicide Bombing: A Response to Andrea Gilli and Mauro Gilli," *H-Diplo*, January 13 2015; Andrea and Mauro Gilli, "The Spread of Military Innovations: Adoption Capacity Theory, Tactical Incentives, and the Case of Suicide Terrorism," *Security Studies*, Vol. 23, No. 3 (2014): 513-547.

conclude that the variation in organizational constraints across terrorist groups explains the variation in adoption and non-adoption of suicide bombing.

From his statistical analysis of 233 terrorist groups, Horowitz infers that less flexible organizations are inherently less likely to adopt disruptive innovations like suicide attacks.[3] In our article, we argue that when we correct for some problems in Horowitz's research design, the statistical results we derive no longer support adoption capacity theory (ACT) – the theory he has developed. We want to stress that we do not believe or argue that organizational factors are unimportant with regard to the adoption of military innovations. In fact, drawing from Horowitz's own work, we have shown in another article that organizational challenges constrain the diffusion of drone warfare.[4] Yet, that we believe organizational factors affect the diffusion of military innovations in general does not imply that that they have also constrained the diffusion of suicide terrorism in particular.

Our response is organized in two main parts. In the first three sections, we briefly summarize each of our original criticisms of Horowitz's research design as well as Horowitz's reply, and finally we illustrate why we believe our concerns are still valid. Thereafter, in the following five sections, we summarize and respond to Horowitz's criticism of our findings. Conclusions follow.

## 1. First Problem in Horowitz's Research Design: Organizational Age

In this section we summarize our first criticism, we then summarize Horowitz's reply, and then we present our counter-response.

As we argued in our original criticism, according to ACT, more rigid organizations are less likely to overcome the organizational resistance to the adoption of disruptive innovations such as suicide bombing. Drawing from Mancur Olson's work, Horowitz relies on age to operationalize his independent variable, the flexibility of terrorist organizations (which he calls organizational capital).[5] In our article, we claim that Horowitz's measure is inappropriate to test his theory since it is inconsistent with the literature in management ACT builds upon. Accordingly, we suggest that the size of a terrorist organization is a better proxy for organizational flexibility and thus, ultimately, for testing ACT's empirical

[3] Michael C. Horowitz, *The Diffusion of Military Power: Causes and Consequences for International Politics* (Princeton, NJ: Princeton University Press, 2010); and Michael C. Horowitz, "Nonstate Actors and the Diffusion of Innovations: The Case of Suicide Terrorism," *International Organization*, Vol. 64, No. 1 (January 2010): 33-64.

[4] Andrea Gilli and Mauro Gilli, "The Diffusion of Drone Warfare? Industrial, Organizational and Infrastructural Constraints: Military Innovations and the Ecosystem Challenge," *Security Studies* (forthcoming).

[5] Mancur Olson, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities* (New Haven, CT: Yale University Press; 1982).

validity.[6] Clayton Christensen's work on disruptive innovations, from which Horowitz draws, explicitly discusses the obstacles that larger – not older – organizations face when dealing with disruptive innovations. Additionally, Christensen also provides several examples of young organizations that were unable to deal with disruptive innovations and of older organizations that succeeded – the opposite of what Horowitz expects.

According to Horowitz, our criticism derives from our "misunderstanding" of his theory.[7] Specifically, in his account, it is "theoretically irrelevant" that Christensen's original formulation did not focus on age because ACT is not a direct application of Christensen's disruption innovation theory.[8] Conversely, Horowitz explains, ACT draws from a wide literature in management, such as

> Darby and Zucker, Henderson and Clark, Clark, Tushman and Anderson, and others. In particular, adoption capacity theory explicitly draws on Rebecca Henderson's work on radical innovation in the photolithographic alignment equipment industry.[9]

We appreciate Horowitz's clarification about his theoretical framework. Three considerations justify our suggestion to use size rather than age to measure the organizational flexibility of terrorist groups. First, Horowitz employs Christensen's terminology (that of *disruptive* and *sustaining* innovations) rather than that of Rebecca Henderson (*radical* and *incremental* innovations), of Michael Tushman and Phil Anderson (competence-destroying and competence-enhancing innovations), of Rebecca Henderson and Kim Clark (architectural, modular, *incremental*, and *radical* innovations) or of other scholars he cites in his book and in his reply to our article, which thus led us to believe that Christensen's disruptive innovation theory played a key role in ACT.[10]

Second, while Horowitz now claims that size is "inappropriate for testing adoption capacity theory", that "there is no theoretical reason to expect organizational size is related to suicide bombing adoption," and that we incorrectly assign a central role to Christensen's

[6] Gilli and Gilli, "The Spread of Military Innovations," 525.

[7] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 1-2.

[8] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 3.

[9] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 3.

[10] Rebecca Henderson, "Underinvestment and Incompetence as Responses to Radical Innovation: Evidence from the Photolithographic Alignment Equipment Industry," *The RAND Journal of Economics* 24, no. 2 (Summer 1993): 248-270; Michael L. Tushman and Philip Anderson, "Technological Discontinuities and Organizational Environments," *Administrative Science Quarterly* 31, no. 3 (Sep., 1986): 439-465; and Rebecca M. Henderson and Kim B. Clark, "Architectural Innovation: The Reconfiguration Of Existing Product Technologies and the Failure of Established Firms," *Administrative Science Quarterly* 35, no. 1 (1990): 9-30.

work in his ACT,[11] a footnote in his original article runs against these claims and provides support for our interpretation. The footnote reads as follows:

> On size and disruptive innovations see Christensen 1997. In fact, for the reasons Asal and Rethemeyer 2008, 439, lay out for the positive correlation between size and lethality—experience and human capital that build expertise— size may be negatively correlated with the adoption of disruptive innovations. However, the data necessary to systematically test this question is lacking.[12]

In other words, by using organizational size, we have in the end done nothing less and nothing more than what Horowitz suggested one should do if data permits: we gathered the data and we tested empirically the proposition that size negatively affects the likelihood of adoption.

Third, even if age were an appropriate measure for measuring the organizational flexibility of terrorist groups, there would still be an equally compelling case for employing size as a proxy. The literature in management, (including the very work by Tushman and Anderson[13] and of Henderson[14] discussed by Horowitz in his reply) assigns in fact a central importance to size.[15] For instance, Henderson explicitly argues that both size and age negatively affect the capacity of an organization to adopt *radical* innovations:

> Large established firms have an advantage over entrants in the pursuit of incremental innovation because incremental innovation builds upon their existing knowledge and capabilities, but these assets can simultaneously reduce substantially the effectiveness of their attempts to exploit radical innovation.[16]

Thus, even if Christensen's theory does not play a central role in ACT as Horowitz argues in his response, there is little reason to believe that the size of a terrorist organization is inappropriate to capture how flexible that organization is. This consideration is particularly appropriate given the problematic way in which Horowitz measures age – as

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[11] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 1, 2 and 5.

[12] Horowitz, "Nonstate Actors and the Diffusion of Innovations," 45, fn. 51.

[13] Tushman and Anderson, "Technological Discontinuities and Organizational Environments," 442.

[14] Henderson, "Underinvestment and Incompetence as Responses to Radical Innovation," 251.

[15] It is important to highlight that the innovation literature in management is often vague about age. It generally adopts, in fact, the term "established" in a casual way, which often simply means market leaders. Henderson and Clark, for instance, discuss several examples of market leaders that emerged in a matter of few years and were quickly replaced by new innovators. See Henderson and Clark, "Architectural Innovation," 23-27.

[16] Henderson, "Underinvestment and Incompetence as Responses to Radical Innovation," 251.

we discuss in the following section.

## 2. Second Problem in Horowitz's Research Design: Measurement

In our article, we also argue that that even if age were an appropriate indicator of an organization's flexibility, Horowitz's measurement of his independent variable would still be problematic.[17] In this section we summarize our original criticism, Horowitz's reply, and our counter-response.

Our focus on size was driven not just by the theoretical criticism we have illustrated above, but also by the fact that the specific measure Horowitz has employed to capture organizations' flexibility does not permit scholars to test the causal mechanism underlying ACT. Horowitz has calculated the organizational age of a terrorist group by subtracting the year in which an organization was created from the benchmark year 2006. Organizational age is hence a cardinal variable that measures 1 for groups created in 2005, 2 for groups created in 2004, 3 in 2003 and so forth. In our article we emphasized that such a measure does not reflect the true age of an organization; because of this coding procedure, two organizations that were created in two different years (say 2005 and 1983), but that were active for only one year, will have in fact very different "organizational ages" (respectively 1 and 23 years).[18] This means that the correlation Horowitz finds tells us that organizations created more recently are more likely to adopt suicide attacks. This coding procedure is problematic for two reasons. First, this measure does not permit us to test ACT's causal mechanism about bureaucratization, since there is no reason to believe that groups created in 1985 are more bureaucratized than those created in 2003. Second, and more importantly, this measure systematically biases Horowitz's results in favor of ACT given the large number of terrorist organizations that were *created* after (and potentially because of) 9/11 and of the ensuing global war on terror and that have employed suicide attacks (out of 41 adopters in Horowitz's dataset, 18 belong to this group).

Horowitz responds that the measure he employs to operationalize organizational capital (organizational age) is appropriate. Precisely, he claims that

> [the] formulation for the link between organizational age and suicide bombing centers, as it does for all innovations, on the relationship between the start year of a group and the 'debut' of an innovation. [...A]ccording to adoption capacity theory, the core prediction is that groups founded before 1981 should be less likely to adopt than groups that started after 1981.[19]

[17] Gilli and Gilli, "The Spread of Military Innovations," 524-525.

[18] Conversely, two organizations created in the same year (say 1983) but that were active for different period (say 1 and 23 years respectively), will have the same "organizational ages".

[19] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 4.

In his reply, Horowitz also proposes a new, alternative way of measuring age and implements additional changes to his statistical analysis (in one model he controlled for the last year of activity of a terrorist group, and dropped terrorist groups that ceased to exist before 1987, i.e. when suicide bombing was still in its infancy).

There are three problems with Horowitz's response. First, even if the core prediction of ACT is that groups created after 1981 are more likely to employ suicide attacks, the problem we raised in our article still applies: the relationship between the two variables is endogenous. With Horowitz's measure, it is in fact not possible to understand whether groups created more recently are more flexible and therefore more likely to adopt suicide attacks, or whether 9/11 and the ensuing global war on terror *created* terrorist groups that wanted to employ suicide attacks. Empirically, we have strong reasons to believe the latter to be the case given that 18 out of the 41 terrorist groups in Horowitz's dataset that adopted suicide terrorism were created after 9/11.[20]

Second, if the core prediction of ACT is that groups created after 1981 are more likely to employ suicide attacks – as Horowitz maintains in his response – this means then that ACT's central focus is not on the *internal* capacity (flexibility) of a group to adopt disruptive innovation (as Horowitz's initial formulation suggests), but about the social environment, the *external* conditions in which a group operates. Intuitively, a terrorist group that was created in 1977 and that ceased to exist ten years afterward would have been exposed to very few suicide attacks. Conversely, a group that was created in 1987 and was active for the following ten years would have lived through the campaigns of suicide attacks by the Tamil Tigers, Hamas, and others, and thus would be more likely to adopt because of possible imitation or learning effects. This means that the correlation Horowitz finds between the year in which an organization was created and the probability that it adopts suicide attacks captures a completely different causal mechanism from the one postulated by ACT.

Third, the new statistical tests Horowitz has performed do not address the endogeneity problem we have raised. This means that from his new results it is still not possible to understand whether groups created more recently are more flexible and therefore more likely to employ suicide attacks, or whether suicide terrorism and in particular 9/11 (and the ensuing global war on terror) created terrorist groups that wanted to employ suicide attacks. Yet, these new statistical tests represent a welcome contribution as they try to address a different but very important problem, namely that the terrorist organizations in Horowitz's dataset were exposed to different eras of suicide bombing. Those that operated until the late 1980s had very little exposure; those that operated until 2001 had slightly more exposure; those that operated after 2001 had extremely high exposure. Since we did

[20] Horowitz defends his result by pointing to a matrix in his original article that shows "it is younger groups, even outside the context of a complicated statistical model, that are more likely to adopt suicide bombing." Unfortunately, the graph in his original article does not permit to derive such a conclusion. This matrix does not address the endogeneity problem we have raised, and thus still provides little insights about the causal effect of age on the probability of adoption. For the quotation, see Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 4.

not raise this problem in our article, we discuss this aspect briefly in the conclusions.

## 3. Third Problem in Horowitz's Research Design: Unwarranted Assumption

In our original article, we maintain that Horowitz's analysis relies on a central but unwarranted assumption, namely that terrorist groups "should have inherent interests in thinking about the adoption of new tactics such as suicide attacks" since they are facing the "constant threat of extinction."[21] As we argue, there is no intuitive reason (whether logical, theoretical or empirical) to believe that all the terrorist groups in his dataset had the same "interest" in adopting suicide attacks.[22] Thus, without controlling for the incentive to adopt, Horowitz ends up treating all instances of non-adoption as instances of incapacity to adopt – which thus provides artificial support to his theory.

In his response, Horowitz does not address this aspect. Thus, from the correlation Horowitz originally found between the year in which a group was created and the probability of adopting suicide bombing, we cannot disentangle the capacity from the will to adopt. When we include a measure that captures groups' incentive to adopt suicide bombing (the level of mechanization of the enemy-country for each terrorist group) we cannot reject the null hypothesis for ACT's key variable at the 5% conventional level in 7 out of the 9 models we have run.[23] This means that even if we ignored our previous criticisms about Horowitz's definition and measurement of the flexibility of terrorist groups, our analysis would still cast doubt on the findings that organizational constraints explain the variation in adoption and non-adoption of suicide bombing.

## 4. Horowitz's Criticism N. 1: Is Mechanization an Appropriate Proxy?

In our article, we also tested an alternative hypothesis. We claimed that the adoption of suicide attacks depends on terrorist groups' tactical incentives – namely, we hypothesized that groups facing more militarily capable adversaries in conventional terms are more likely to employ suicide attacks than those facing less capable enemies. In order to measure such tactical incentives, we used the mechanization index developed by Todd Sechser and Elizabeth Saunders, which is calculated "by taking the number of main battle tanks, heavy-armored combat vehicles, armored personnel carriers, and infantry-fighting vehicles per one hundred soldiers."[24] Our statistical analysis supports our hypothesis: we found that terrorist groups fighting against more mechanized countries are more likely to employ

[21] See Horowitz, "Nonstate Actors and the Diffusion of Innovations," 42.

[22] Gilli and Gilli, "The Spread of Military Innovations," 525-526.

[23] In order to capture such incentive to employ suicide attacks, in our article we controlled for the level of mechanization of the enemy-country of each terrorist organization, as calculated by Saunders and Sechser. See Todd S. Sechser and Elizabeth N. Saunders, "The Army You Have: The Determinants of Military Mechanization, 1979-2001," *International Studies Quarterly* Vol. 54, n. 2 (June 2010): 481-511.

[24] Gilli and Gilli, "The Spread of Military Innovations," 533.

suicide attacks.

In his response, Horowitz takes issue with our findings. His first concern is that the measure we rely on in order to operationalize our independent variable does not capture the causal mechanism behind our hypothesis. More precisely,

> mechanization… is unrelated to the targets of most suicide attacks. Suicide bombing attacks, even against security targets, generally occur against static targets such as military bases or police stations, but not anything related to mechanization' in the form of tanks…[25]

In other words, Horowitz argues that our measure suffers from measurement error since "there is no evidence presented to support the idea that an increase in mechanization goes along with an increase in the hardening of other targets."[26]

As we discussed in our article, mechanization

> is not perfect: it captures the hardness of military targets only, not the hardness of civil ones, or the hardness of entering a country's territory (as in Berman and Laitin's original formulation). Additionally, it provides a quantitative rather than a qualitative measure.[27]

This being said, we believed and we still do believe that

> as a first step, [mechanization] is an acceptable proxy as increasingly mechanized armies, those with more armored vehicles with respect to the number of soldiers, are more likely to use them and hence represent harder to hit targets. [28]

A few additional considerations support our choice. First, measuring the variation across terrorist groups in the tactical incentives to employ non-traditional terrorist tactics is inherently difficult. That mechanization is not perfect, however, does not mean it is not useful. Other things being equal, we expect that the higher the number of mechanized vehicles a country possesses (which are not just tanks, as Horowitz suggests, but also heavy-armored combat vehicles, armored personnel carriers, and infantry-fighting vehicles), the more of these vehicles will be deployed, also to protect stationary targets like

[25] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 7.

[26] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 7.

[27] Gilli and Gilli, "The Spread of Military Innovations, 533. The work by Berman and Laitin we referred to is: Eli Berman and David D. Laitin, "Religion, Terrorism and Public Goods: Testing the Club Model," *Journal of Public Economics* 92, nos. 10–11 (October 2008): 1942–67.

[28] Gilli and Gilli, "The Spread of Military Innovations, 533.

military bases, police stations, parliaments and so forth. This means, in turn, that for rebel or terrorist groups, striking their enemies with traditional tactics will become more difficult.

Second, Horowitz correctly highlights the distinction between mobile and stationary targets. It is true that we do not prove that as mechanization increases, the protection of stationary targets increases. However, if the two were unrelated, as Horowitz's reply suggests, it would mean that the countries that have invested resources to protect their patrolling troops leave their military bases, police stations, government buildings, and other stationary targets relatively under-protected. This is counterintuitive. Given that cross-country data on the protection of stationary targets is not available (to our knowledge), we have used cross-country data on the protection of mobile targets only, which we still believe is the best available proxy to measure our independent variable.

Third, Horowitz has not provided any reason to believe that our proxy artificially inflates our results or that it captures a different causal mechanism from the one we are testing. On the one hand, if our proxy leads to any measurement error (a possibility we do not dispute), such measurement error should then lead to attenuation bias – which should thus further reassures the readers about our choice and our results. On the other hand, when controlling for regime type and for GDP per capita, mechanization remains statistically significant, which in turn suggests that with our proxy we are not capturing the effect of either democracy or wealth – the two most likely cofounders.

Fourth, Horowitz has not offered an alternative and possibly better proxy to measure the tactical incentives to employ suicide attacks. We conclude that mechanization is, in the end, the best available measure to capture the variation in tactical incentives across terrorist and rebel groups. Other scholars, independently of our work, have employed the same measure to test a similar argument but on different datasets and have reached the same findings. We believe this provides additional support both to our choice to rely on the mechanization index and to our results.[29]

Last but not least, our four qualitative case studies provide further empirical support to our statistical findings. On the one hand, the cases of Hezbollah, the Tamil Tigers, Chechen rebels, and PIRA suggest that the cross-time variation in the relative balance of forces on the ground explains the adoption, abandonment and re-adoption of suicide attacks. On the other, in these cases we observe that suicide attacks were employed against heavily protected targets, and that these groups kept using traditional terrorist attacks against non-protected targets.

## 5. Horowitz's Question N. 2: Do Suicide Attacks Strike Hard Targets?

[29] Simon Collard-Wexler, Costantino Pischedda, and Michael G. Smith, "Do Foreign Occupations Cause Suicide Attacks?," *Journal of Conflict Resolution* 58, no. 4 (2014): 625-657. See also Seung-Whan Choi and James Piazza, "Foreign Military Interventions and Suicide Attacks," *Journal of Conflict Resolution* (forthcoming).

In order to reassure the readers about our findings, we also looked at the type of targets of suicide attacks with the goal of testing whether, "at the micro-level, tactical imperatives played the role we have hypothesized."[30] We found that 64.5% of suicide attacks are employed against security targets (military bases, police stations, etc.), 12.2% against political targets (such as government offices, parliaments, etc.) and 23.3% against civilian targets. Given that both security and political targets are generally highly protected, we concluded that 76.7% of suicide attacks are employed against "hard" targets – a finding that is consistent with our claim that suicide attacks are a tactical response to enemy's superior military capabilities.

Horowitz disagrees with us and argues that

> while is it is true that more than 60% of suicide attacks occur against security targets… that still leaves a significant set of terrorist attacks against targets that are certainly not plausibly hardened, such as suicide attacks on Israeli buses by members of Hamas in the 1990s.[31]

We do not dispute the claim that suicide attacks are employed also against non-hardened targets. In fact, in the article we recognize that different factors play a role in the adoption of suicide bombing. Yet, from the data we reported above, we conclude that the number of terrorist attacks carried out against "targets that are certainly not plausibly hardened" is in the end only 23.3%. This means that 3 out of 4 suicide attacks are consistent with our argument. While not perfect, we believe this is a particularly remarkable performance given the high number of explanations that have been provided for this phenomenon.[32]

## 6. Horowitz's Question N. 3: Lacking Data

Horowitz has raised an additional doubt with regard to our statistical analysis. According to him,

[30] Gilli and Gilli, "The Spread of Military Innovations," 539.

[31] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 7.

[32] See for example Scott Atran, "Genesis of Suicide Terrorism," *Science* 299, no. 5612 (7 March 2003); Jean Paul Azam, "Suicide Bombing as Intergenerational Investment", *Public Choice*, vol. 122 (Spring 2005); Mia Bloom, *Dying to Kill: The Allure of Suicide Terror* (New York: Columbia University Press, 2005); Robert A. Pape, *Dying to Win: The Strategic Logic of Suicide Terrorism* (New York: Random House, 2005; Diego Gambetta (ed.), *Making Sense of Suicide Missions* (Oxford: Oxford University Press, 2006); Robert Braun and Michael Genkin, "Cultural Resonance and the Diffusion of Suicide Bombings: The Role of Collectivism," *Journal of Conflict Resolution* Vol. 58, no. 7 (October 2014): 1258-1284.

> the variable [mechanization] is very sparse – without data after 2001, it is hard to know whether the pattern identified by Gilli and Gilli applies in the most prominent decade of suicide bombing (after the 9/11 attacks)…[33]

Horowitz is right in claiming that our analysis relies on pre-9/11 data. The data on mechanization Sechser and Saunders collected covers in fact the period 1979-2001. However, this is hardly the problem Horowitz believes it is. As we explain in our article, as a result of the increasing threat of suicide terrorism, in several countries (like the US, Israel and India), military expenditure increased after 9/11. If we had included post-9/11 data on mechanization, any correlation between mechanization and the probability of adoption of suicide would have been biased by endogeneity. This is why we did not include this data. Moreover, our conservative coding strategy should, at most, lead to measurement error, and hence again to attenuation bias. Thus, our conservative coding strategy averts the risk of endogeneity and at the same time leads us to underestimate the effect of mechanization on the probability that a group employs suicide attacks.

### 7. Horowitz's Question N. 4: An Endogenous Relation?

Horowitz has raised an additional concern with regard to our findings: he suggests that the relationship we find between mechanization and the employment of suicide attacks is endogenous. He claims:

> to the extent that there is a relationship between mechanization and suicide attacks, it likely means that countries with stronger militaries facing terrorist campaigns are more likely to face suicide attacks. But that is endogenous to why terrorist campaigns happen in the first place, given that terrorist campaigns are weapons of the weak to begin with![34]

Four considerations are in order. First, the correlation we found would be endogenous if our dependent variable (adoption and non-adoption of suicide bombing) caused our independent variable (the level of military mechanization of countries). However, that terrorism is the weapon of the weak does *not* imply that suicide terrorism causes mechanization.

Second, at face value, Horowitz's criticism suggests that since terrorism is the weapons of the weak, the weak should then adopt suicide terrorism (not just terrorism in general, but suicide terrorism in particular). If Horowitz's criticism were correct, we should then observe limited variation in outcome: a large number of terrorist groups should have employed suicide terrorism. In fact, we observe the very opposite: the majority of groups in

[33] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 8.

[34] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 7.

in Horowitz's dataset have actually not employed such a highly effective tactic (this is the case for 192 out of 233 groups).

Third, Horowitz's criticism might be suggesting that our findings suffer from omitted variable bias, namely, that relative weakness in resources in general, not just in military capabilities in particular, is driving our findings. This is a reasonable claim, given that countries' wealth and military capabilities are likely to be correlated. This claim can be tested empirically. If correct, the correlation between the adoption of suicide terrorism and mechanization should vanish when we control for GDP per capita. When we control for GDP per capita (as reported in table 3 of our article) our key independent variable remains significant.[35] Conversely, GDP per capita is not significant and it has a negative coefficient (the opposite of what we would expect according to Horowitz's criticism).

Fourth, in our article we do address the most problematic source of endogeneity for our analysis. When discussing the correlation between enemy-mechanization and the probability that a group adopts suicide attacks, we point out that during the period under consideration

> suicide terrorism has diffused among more and more terrorist groups. This poses two problems. On the one hand, our results might be biased by simultaneous or inverse causation—that is, the threat of suicide terrorism might have been responsible for countries' increasing mechanization. On the other hand, our results might simply reflect a trend in the data.

We address these possible problems by using data about the level of mechanization for each country in the year (1981) before the first suicide terrorist attacks was carried out. As we explain, "the diffusion of suicide terrorism cannot affect countries' levels of armies' mechanization in the year (1981) before the tactic was first employed." Therefore, with this measure we address the possible risk of simultaneous or inverse causation. Moreover, "by using data from the same year for all organizations, we reduce any concern that our independent and dependent variables are simply increasing in time."[36]

In light of these considerations we believe that Horowitz's concern that our findings are biased by endogeneity has little ground.

### 8. Horowitz's Question N. 5: Is Size Measured Appropriately?

Finally, in his response, Horowitz maintains that the variable size – which we used to test ACT – is inappropriate. Organizational size, as we explained in our article, "is an ordinal variable that equals 0 for group with less than one hundred members; 1 for groups with

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[35] Gilli and Gilli, "The Spread of Military Innovations," 536.

[36] Also in this case, the measurement error resulting from this coding procedure should lead to attenuation bias. See Gilli and Gilli, "The Spread of Military Innovations," 537.

one hundred to one thousand members; 2 for groups with one to ten thousand members; and 3 for those with more than ten thousand members."[37]

According to Horowitz, this variable "has limits even as a test of Christensen's disruptive innovation theory." He points out that "[f]or example, a group with a "1" in [Gilli and Gilli's] coding scheme could have membership anywhere from 100 to 1000, a ten-fold difference…" For this reason, he continues, "the lack of granularity in Gilli and Gilli's organizational size data means too many groups of what are in reality very different sizes are grouped together."[38]

Horowitz is correct: the cut-off points in this ordinal variable could affect our results. For this reason, in our original article, we reported the most conservative findings. Namely, we reported the results according to which size has sometimes no significant effect or a positive effect on the probability of adoption (according to the more liberal findings, size has a positive effect, the opposite of what ACT suggests). Horowitz then highlights an important aspect, namely: "the real question […] would be whether groups were large 'enough' to adopt while still being small enough to innovate." [39] We are sympathetic to this consideration. The results in his response, however, are very similar to ours: size has sometimes no effect and sometimes a positive (but non-linear) effect on the probability of adoption, depending on the cut-off points chosen.

## 9. Conclusions

Michael Horowitz's *The Diffusion of Military Power* represents a cornerstone in the study of the diffusion of military innovations and in International Relations more in general. The praises and prizes it has received speak for themselves. In fact *The Diffusion of Military Power* triggered our interests for this area of study, and has played a key role during our graduate education. Horowitz has merged the literature in management with the scholarship in security studies to explain major dynamics in international affairs. He has complemented his theoretical contribution with an in-depth investigation of three historically important and substantially relevant cases: the battleship revolution at the beginning of the XX century, naval aviation and nuclear weapons. However, when it comes to the statistical analysis of the diffusion of suicide terrorism, the available empirical evidence is at best insufficient to support adoption capacity theory, and possibly it contradicts it. Future scholarship should build on Horowitz's comments and on our reply to further improve this area of research. Three aspects deserve particular attention.

First, this exchange has highlighted the need to investigate further the role that the internal dynamics of organizations play in affecting the adoption of disruptive/radical innovations.

[37] Gilli and Gilli, "The Spread of Military Innovations," 532.

[38] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 6.

[39] Horowitz, "Adoption Capacity and the Spread of Suicide Bombing," 6.

The literature in military innovations has largely neglected Christensen's other major works, namely *The Innovator's Solution* and *Seeing What's Next*,[40] and thus their insights concerning how organizations can eschew the innovator's dilemma – irrespective of their internal features. Future research should build on these as well as other contributions in management to deepen further how the interaction between the size and age of terrorist organizations affects the adoption of disruptive/radical innovations.[41]

Second, in his response Horowitz focuses his attention on a fundamentally important aspect: how exposure to an innovation affects adoption. In our article, we refer to a seminal work on the diffusion of innovations by H. Peyton Young which discusses different diffusion mechanisms such as contagion, learning and social influence.[42] In his original article, Horowitz contributed to the literature in military innovations by testing the effect of contagion (i.e., connection with groups that have mastered the know-how necessary to employ suicide attacks). Unfortunately, because of the nature of the data, we could not test the other two mechanisms. Further research should explore more in depth these diffusion paths and test their empirical implications to enhance our understanding of these processes.

Third, future scholarship should try to gather more precise data on the size and on the age of terrorist organizations in order to test both Horowitz's insights and ours more accurately. It should also gather data on post-9/11 level of mechanization in order to test whether the correlation we found holds. Even though this coding procedure will bias the correlation, this analysis will then represent an important test for our results.

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[40] Clayton M. Christensen and Michael E. Raynor, *The Innovator's Solution: Creating and Sustaining Successful Growth* (Cambridge, MA: Harvard Business School Press, 2003); Clayton M. Christensen, Erik A. Roth, and Scott D. Anthony, *Seeing What's Next: Using Theories of Innovation to Predict Industry Change* (Cambridge, MA: Harvard Business School Press, 2004). For a partial application in this direction, see Terry C. Pierce, *Warfighting and Disruptive Technologies: Disguising Innovation* (London: Frank Cass, 2004).

[41] For works that address this topic, see for instance Will Mitchell, "Whether and When? Probability and Timing of Incumbents' Entry into Emerging Industrial Subfields," *Administrative Science Quarterly* vol. 34, No. 2 (June 1989), pp. 208-230; Glenn R. Carroll, Lynda S. Bigelow, Marc-David L. Seidel and Lucia B. Tsai, "The Fates of De Novo and De Alio Producers in the American Automobile Industry 1885-1981," *Strategic Management Journal* vol. 17 (Summer, 1996), pp. 117-137; Constance E. Helfat and Marvin B. Lieberman, "The Birth of Capabilities: Market Entry and the Importance of Pre-history," *Industrial and Corporate Change* vol. 11, n. 4 (2002), pp. 725-760; Elena Cefis and Orietta Marsili, "A Matter of Life and Death: Innovation and Firm Survival," *Industrial and Corporate Change* 14, no. 6 (2005): 1167-1192; Elena Cefis and Orietta Marsili, "Survivor: The Role of Innovation in Firms' Survival," *Research Policy* 35, no .5 (2006): 626-641.

[42] For our discussion Gilli and Gilli, "The Spread of Military Innovations," 328. The article we cite is H. Peyton Young, "Innovation Diffusion in Heterogeneous Populations: Contagion, Social Influence, and Social Learning," *American Economic Review* 99 no. 4 (2009): 1899–1924.

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