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Moshe Sniedovich. 2014. The elephant in the rhetoric on info-gap decision theory. *Ecological Applications* 24:229-233.

Appendix A. A perspective on a nonexistent gap in the state of the art.

A.1 Introduction

This appendix is a self-contained technical supplement to Sniedovich (2014), which responds to the Letter to the Editor *Information-gap decision theory can fill a gap in ecological applications*, by Burgman and Regan (2014), henceforth *Letter*. In turn, the *Letter* responds to the article *Fooled by local robustness: an applied ecology perspective* (Sniedovich 2012) which exposes the misleading rhetoric in the literature on info-gap decision theory (IGDT), specifically the unfounded rhetoric which attributes IGDT (Ben-Haim 2001, 2006, 2010, 2010a) the capability to manage a severe, non-probabilistic, unbounded uncertainty.

The main objective of Sniedovich (2014), and of course this appendix, is to make it abundantly clear that the *Letter*'s stated aim (as indicated by its title), to identify a gap for IGDT to fill, is not only pointless, it is in fact misguided.

This is so because, as formally proved in Sniedovich (2010, 2011, 2012a, 2012b), and as reiterated in Sniedovich (2012, 2014), IGDT's robustness model is a reinvention of a well-established generic model that is known universally as *radius of stability* (circa 1960) which, in turn, is a simple model deriving from Wald's famous maximin paradigm (circa 1940). This means that any gap in the state of the art that the *Letter* claims to have identified for IGDT to fill, had already been filled decades ago. But more than this, not only that IGDT's robustness model has therefore nothing new to contribute to the state of the art, staple robustness models that have been available for decades, to perform that which IGDT does, are far more powerful and flexible than IGDT's robustness model. This means that these models can do all that which IGDT's robustness model can possibly do, and a great deal more. In other words, as pointed out in Sniedovich (2014), as a simple maximin model, IGDT's robustness model is far more limited in its capabilities than many other maximin models deriving from Wald's famous maximin paradigm.

The implication is therefore clear: ecologists and conservation biologists already have at their disposal tools that can perform all that which IGDT can possibly presume to do, and a great deal more than that.

To show then that the *Letter*'s attempt to carve out a gap for IGDT to fill is indeed pointless and misguided, it is essential to clarify the relation between the three models defined in the next section. Making this relation clear will bring to light the profound incongruity, in the IGDT literature, between the formal, mathematical definition of IGDT's robustness model and the rhetoric about this model. This incongruity, which distorts the hard facts about IGDT's basic characteristics and capabilities, and its relation to other decision theories, gives an utterly misleading picture of the nature of IGDT, and its place in the state of the art. Clarifying these issues will provide the requisite background for exposing the unfounded rhetoric in the *Letter* regarding the meaning of the term "severe uncertainty" in ecological applications of IGDT. This clarification will also show for what it is the *Letter*'s claim that IGDT's robustness model being a radius of stability model "reinforces the context and consistency of the approach" to uncertainty adopted by IGDT.

This done, it will be straightforward to show that IGDT fully deserves the title *voodoo decision theory* given it in Sniedovich (2012, 2014), and to suggest a course of action for dealing with the Elephant in the IGDT room discussed in Snieodvich (2014). Note that, as indicated in Sniedovich (2014), in the context of this discussion the epithet "voodoo" has the second meaning given to this term in *The American Heritage Dictionary of the English Language* (Fourth Edition, 2000), namely: "Based on unrealistic or delusive assumptions: *voodoo economics*".

A.2 A tale of three models

Consider the following three mathematical models:

$$\text{Maximin}: \quad z^* = \max_{x\in X} \min_{s\in S(x)} \{f(x,s) : const(x,s), \forall s \in S(x)\} \tag{A.1}$$

$$\text{Radius of stability}: \quad \rho(q) = \max_{\alpha\geq 0} \{\alpha : const(q,u), \forall u \in U(\alpha,\tilde{u})\}\ ,\ q \in Q \tag{A.2}$$

$$\text{IGDT robustness}: \quad \hat{\alpha}(q) = \max_{\alpha\geq 0} \{\alpha : r_c \leq r(q,u), \forall u \in U(\alpha,\tilde{u})\}\ ,\ q \in Q \tag{A.3}$$

The first model is a generic maximin model, namely a model derived from Wald's famous maximin paradigm (Wald 1939, Rawls 1971, Resnik 1987, French 1988, Sniedovich 2008a). As is well known, this paradigm is the foremost framework for modeling decision problems subject to a non-probabilistic uncertainty. This paradigm yields a variety of models, all of which are based on a *worst-case approach* to uncertainty/variability. For the purposes of this discussion it is convenient to consider the version specified in (A.1), where

$$X = \text{set of } \textit{alternatives} \text{ available to the decision maker.} \tag{A.4}$$

$$S(x) = \text{set of } \textit{states} \text{ associated with alternative } x. \tag{A.5}$$

$$f(x,s) = \textit{payoff} \text{ associated with the } (x,s) \text{ pair.} \tag{A.6}$$

$$\text{const}(x,s) = \text{list of } \textit{constraints} \text{ imposed on } (x,s) \text{ pairs.} \tag{A.7}$$

Note that admissible alternatives are ranked according to their worst-case payoff, namely according to their *security levels*, defined as follows:

$$SL(x) := \min_{s\in S(x)} \{f(x,s) : const(x,s), \forall s \in S(x)\}\ ,\ x \in X. \tag{A.8}$$

The larger $SL(x)$ the better, hence an optimal alternative, say $x^* \in X$, is one whose worst-case payoff, $SL(x^*)$, is as good (large) as the worst-case payoff of any other admissible alternative. The admissibility condition for an alternative x is the worst-case requirement $const(x,s), \forall s \in S(x)$.

As for radius of stability models (Wilf 1960, Milne and Reynolds 1962, Zlobec 1987, Hinrichsen and Pritchard 1990, Hinrichsen and Kelb 1993, Anderson and Bernfeld 2001, Hodgson and Townley 2004), these are staple models that are used in many fields to measure **local** robustness, namely robustness against (small) perturbations in a nominal value of a parameter. Hence, the constituent elements of the radius of stability model specified in (A.2) can be interpreted as follows:

$$u = \textit{uncertainty parameter.} \tag{A.9}$$

$$\tilde{u} = \text{nominal value of } u. \tag{A.10}$$

$$Q = \text{set of } \textit{decisions.} \tag{A.11}$$

$$\text{const}(q,u) = \text{list of constraints on } (q,u) \text{ pairs.} \tag{A.12}$$

$$U(\alpha,\tilde{u}) = \textit{neighborhood} \text{ of size } \alpha \text{ around } \tilde{u},\ \alpha \geq 0. \tag{A.13}$$

Let $\mathscr{U}$ denote the smallest set containing all the neighborhoods $U(\alpha,\tilde{u}), \alpha \geq 0$. We refer to this set as the *uncertainty space*. It represents the set of all the possible/plausible values of u under consideration in the robustness analysis.

By definition then, the radius of stability of decision q at $\tilde{u}$, denoted $\rho(q)$ in (A.2), is equal to the size (α) of the largest neighborhood $U(\alpha,\tilde{u})$ around $\tilde{u}$ all whose elements (u) satisfy the constraints in $\text{const}(q,u)$.

The third model (A.3) is IGDT's robustness model, the central pillar of IGDT (Ben-Haim 2001, 2006, 2010). Here

$$\tilde{u} = \text{point } \textit{estimate} \text{ of the true value of u.} \tag{A.14}$$

$$r_c = \text{critical } \textit{performance level.} \tag{A.15}$$

$$r(q,u) = \text{performance level of decision } q \text{ given } u. \tag{A.16}$$

By definition then, the IGDT robustness of decision q at $\tilde{u}$, denoted $\hat{\alpha}(q)$ in (A.3), is equal to the size (α) of the largest neighborhood $U(\alpha,\tilde{u})$ around $\tilde{u}$ all whose elements (u) satisfy the performance requirement $r_c \leq r(q,u)$.

This is illustrated in Fig. A1, where the IGDT robustness of two decisions, q' and q'', is depicted graphically. The shaded areas represent the sets of acceptable values of u associated with the respective decisions and the circles represent neighborhoods around the point estimate $\tilde{u}$. The IGDT robustness of decision q, denoted $\hat{\alpha}(q)$, is then equal to the radius of the largest circle contained in the shaded area associated with decision q. Observe that, according to IGDT, decision q' is less robust than decision q'' at $\tilde{u}$ for the given value of r_c under consideration.

The formal, rigorous analyses in Sniedovich (2007, 2008a, 2010, 2011, 2012, 2012a, 2012b) of the relationship between these three models yield the following result.

Theorem A.1 *IGDT's robustness model (A.3) is a radius of stability model (A.2), and both are simple maximin models. That is, both (A.3) and (A.2) are simple instances of the generic maximin model (A.1).*

The function of the qualifier *simple* is to call attention to the fact that compared to many other instances yielded by the generic maximin model (A.1), which are far more powerful than the instances specified by (A.3) and (A.2), the latter two are significantly more limited in their capabilities.

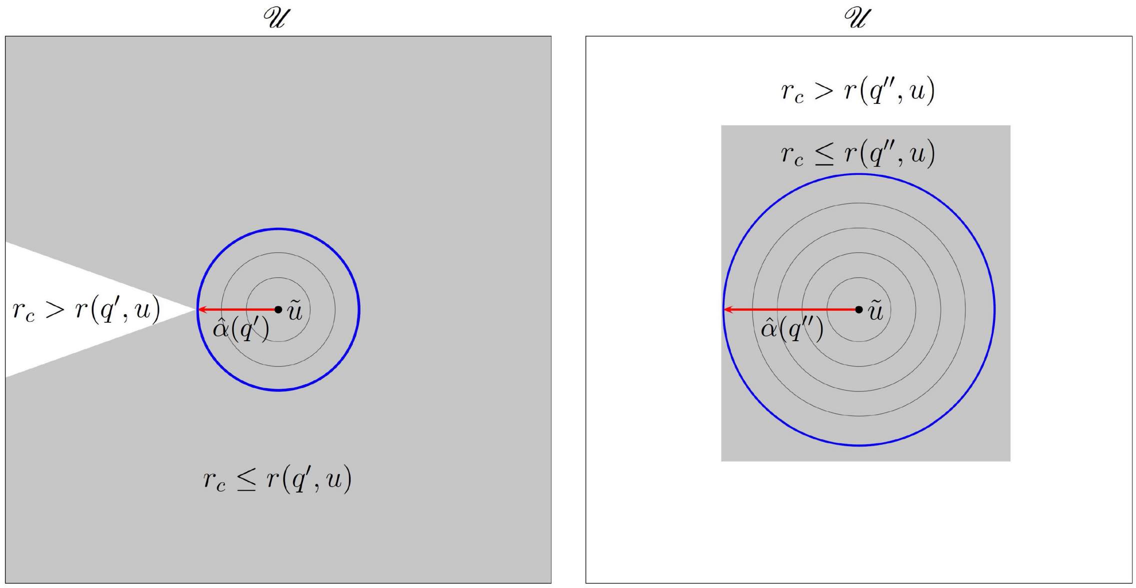

Fig. A1: IGDT robustness of two decisions, q' and q'', at $\tilde{u}$

A.3 The questions

Given the assertion made by the *Letter*'s title, I call the reader's attention to the following two immediate implications of Theorem A.1:

COROLLARY A.1 *Any ecological application that is suitable for treatment by IGDT's robustness model (A.3), is necessarily suitable, for the same treatment, by the radius of stability model (A.2).*

COROLLARY A.2 *There is no gap in the state of the art that IGDT can possible fill, or does fill, or is called upon to fill.*

Indeed, considering some of the claims made in the *Letter*, it is necessary to give these results an even blunter formulation:

COROLLARY A.3 *IGDT's robustness model is a reinvention of the wheel.*

This characterization of IGDT's robustness model brings out more forcefully that the radius of stability model has been a basic tool of the trade in many fields at least since 1960, decades before it was reinvented as IGDT's robustness model in the 1990s. And so, the following two intriguing questions immediately suggest themselves:

QUESTION A.1 *What could possibly be the rationale that motivated a search for a (non-existent) gap in the state of the art for IGDT to fill?*

QUESTION A.2 *What exactly is the gap in the state of the art that the Letter claims to have identified for IGDT to fill?*

The answer to the second question is straightforward: the *Letter* has not identified any gap in the state of the art for IGDT to fill. All that the *Letter* does do is to assert that IGDT provides a framework for dealing with (potentially) many practical ecological problems. But, the point is, in view of the above, that such a framework providing precisely an identical treatment for these practical problems is already offered by radius of stability models. The implication is then that IGDT does not furnish applied ecologists and conservation biologists any tools that they don't already have. So, what gaps are we talking about?

The answer to the first question is more involved as it requires a detailed discussion of the way in which IGDT was embraced, uncritically, by a small group of ecologists and conservations biologist in 2005-2006, and of the way it has been advocated since then in the applied ecology literature. Such a discussion will inevitably takes us to the Land of the Black Swan (Australia) where IGDT has found followers and where it has been promoted by a number of research centers (see Burgman 2008, Fox 2008, Sniedovich 2008, 2008b, 2011).

As we shall progressively see, the *Letter*'s rhetoric on a (non-existent) gap in the state of the art for IGDT to fill, in fact comes down to an attempt to justify, *ex post facto*, the unfounded rhetoric in the IGDT literature on the capabilities of the theory and its role and place in the state of the art. This unfounded rhetoric was identified for what it is at the end of 2003, it was exposed in public at the end of 2006 (e.g. Sniedovich 2006), and it had been discussed in the peer-reviewed literature since 2007 (e.g. Sniedovich 2007, 2008, 2010, 2012, 2012a, 2012b). It was first discussed in the peer-reviewed applied ecology literature in Sniedovich (2012) and subsequently in Hayes et al. (2013).

To appreciate the issues raised by QUESTION A.1 requires a basic familiarity with the criticism directed at IGDT (e.g. Sniedovich 2007, 2008, 2010, 2011, 2012, 2012a, 2012b; Hayes 2011; Hayes et al. 2013). This in turn calls for an examination of the rhetoric in the IGDT literature about the theory's (alleged) extraordinary abilities to deal with severe uncertainty and the (alleged) radically different approach that the theory adopts for this purpose.

Our next task is then to take a look at this rhetoric.

A.4 The role rhetorics in the IGDT literature

By way of introduction, I remind the reader that verbal narratives about mathematical objects and the relationships between them have an important role in the discourse in the sciences. Indeed, verbal narratives can be of crucial importance especially in areas of expertise where analysts who are not sufficiently mathematically savvy may not have the skills to follow the mathematical discourse on the method/theory/technique that they use. But, it is equally clear that verbal narratives in the sciences can be flawed, misleading, and in some cases may even do more harm than good.

That said, I propose the following seemingly self-evident (some might say superfluous) propositions, as guiding assumptions for this discussion:

ASSUMPTION A.1 *Progress in science is not achieved by means of unfounded, unscientific rhetorics.*

ASSUMPTION A.2 *The challenges posed by the severe uncertainty that ecological applications are subject to cannot be tackled with unfounded, unscientific rhetorics.*

Assumption A.3 *Unfounded, unscientific rhetoric about the three mathematical models specified by (A.1)-(A.3) is unlikely to give ecologists and conservation biologists a clearer, better idea of the capabilities and limitations of these models. To the contrary, such rhetoric is more likely to impede their appreciation of these models' role and place in the state of the art, and the relationship between them.*

I urge readers to keep these guiding assumptions in mind when they come across statements such as the *Letter*'s title *Information-gap decision theory can fill a gap in ecological applications*, or statements such as this (Ben-Haim 2001, 2006, p. xii; emphasis added):

> "Info-gap decision theory is **radically different** from **all** current theories of decision under uncertainty. The difference originates in the modeling of uncertainty as an information gap rather than as a probability. The need for info-gap modeling and management of uncertainty arises in dealing with **severe lack of information** and **highly unstructured uncertainty.**"

The point is that in view of Sniedovich (2007, 2010, 2011, 2012, 2012a, 2012b), IGDT cannot possibly be **radically different from all current theories under uncertainty** because, its central pillar, namely its robustness model (A.3), is a reinvention of the *radius of stability* model (A.2), which in turn is a simple maximin model, namely a simple instance of a generic model deriving from the most famous paradigm for decision under non-probabilistic uncertainty (A.1).

Indeed, making such a claim is akin to to claiming that the polynomial $(x - A)(x - B)$, which is a simple instance of the generic "current" polynomial $a_0 + a_1x + a_2x^2 + \cdots + a_nx^n$, is radically different from all "current" functions. Such claims have no place in peer-reviewed publications.

With this in mind, let us now turn to a number of claims made in the *Letter*.

A.5 The meaning of "severe uncertainty"

In line with Burgman (2008), the *Letter* attempts to justify past ecological applications of IGDT by creating the straw man: *the meaning of the term "severe uncertainty"*. In greater detail, the *Letter* argues that there is no universal definition of the term "severe uncertainty", what is more, the specific meaning attributed to this term in Sniedovich (2012) is in fact Sniedovich's conception, but it is not shared in the ecological applications of IGDT. The *Letter*'s point is then that because ecological applications of IGDT are not predicated on Sniedovich's (2012) conception of "severe uncertainty", they are immune to Sniedovich's (2012) criticism, which presumably goes to show that ecological applications of IGDT are in fact sound, perhaps irreproachable.

It is important, therefore, to remind the *Letter*'s authors, and to point out to the reader, that:

- The term, more precisely the concept "severe uncertainty", **as it is used in ecological applications of IGDT,** was **not** invented in the applied ecology literature.
- Indeed, the concept "severe uncertainty", **as it is used in ecological applications of IGDT**, originated in the two books on IGDT (Ben-Haim 2001, 2006) which, it is important to remember, are subtitled: *decisions under severe uncertainty*.

- In Ben-Haim (2001, 2006, 2010, 2010a), the Father of IGDT gives a clear-cut explanation of the meaning of "severe uncertainty" in IGDT, thereby leaving no room for debate as to the meaning of this term in the context of IGDT's robustness model (A.3).
- The discussion in Sniedovich (2012) makes it equally clear that the meaning it attributes to the concept "severe uncertainty" is precisely the meaning given it in Ben-Haim (2001, 2006, 2010, 2010a).

That said, I call on the *Letter*'s authors to uphold accepted academic conventions requiring participants in a discussion to substantiate their arguments/claims. That is, I call on them to back up their claim that the meaning given to the term "severe uncertainty" in Sniedovich (2012) is not shared in the ecological applications of IGDT.

To be precise, the onus is on the *Letter*'s authors to substantiate their "up for grabs" claim:

> "in Sniedovich (2012) it is assumed to mean that any possible value of the variable in question is up for grabs (see "no man's land" in Fig. 5 in Sniedovich 2012). However, this meaning is not shared in the ecological applications of information-gap decision theory."

To substantiate this claim the *Letter*'s authors must provide details on ecological applications of IGDT where the stipulation is made that not all the elements of the uncertainty space under consideration are "up for grabs", to further explain how one determines which elements of the uncertainty space under consideration are "up for grabs" and which are not.

In fact, the *Letter*'s authors must do more than that. They must explain how is it that in their own peer-reviewed articles, discussing ecological applications of IGDT (e.g. Regan et al. 2005; Burgman et al. 2010), not the slightest suggestion is made that in an ecological application of IGDT not all the elements of the uncertainty space are "up for grabs", much less is there any discussion on a mechanism enabling one to implement the "up for grabs" criterion.

For my part, I can categorically state that I am not familiar with a single publication describing an ecological application of IGDT where it is so much as hinted, much less stipulated, that some of the elements of the uncertainty space under consideration are "up for grabs", while others are not.

Indeed, if this were so, this would have meant that the publications in question attribute the concept "severe uncertainty" a meaning that is **totally at odds** with that attributed it in Ben-Haim's (2001, 2006, 2010). But, the fact of the matter is, of course, that all the articles and books describing ecological applications of IGDT that I am familiar with, adopt precisely the meaning attributed to the term "severe uncertainty" by the Father of IGDT in Ben-Haim's (2001, 2006, 2010) (by implication Sniedovich's (2012) meaning) which by necessity commits them to the supposition that all the elements of the uncertainty space under consideration are "up for grabs".

The fact that ecological applications of IGDT are indeed committed to Ben-Haim's (2001, 2006, 2010) conception of "severe uncertainty" is corroborated by the most comprehensive assessment to-date of these applications (Hayes et al. 2013, p. 609):

> "We believe, however, that most ecological applications of IGDT have taken the claims of Ben-Haim (2006) at face value. A typical example of this is the analysis

presented in Halpern et al. (2006). This paper reviews uncertainty techniques applicable to marine reserve design and provides a taxonomy presented in Fig. 6. This proposed taxonomy presents info-gap as providing a technique that can accommodate the largest amount of uncertainty and asserts that this uncertainty is unbounded."

Having said all that, it is important to be clear on how "severe uncertainty" is perceived, hence modeled in IGDT. Observe then that, according to the Father of IGDT (Ben-Haim 2001, 2006, 2010, 2010a), the severity of the uncertainty postulated by IGDT is manifested in the following properties of IGDT models:

- The uncertainty space $\mathscr{U}$ can be vast. In fact, according to Ben-Haim (2006, p. 210) "...most of the commonly encountered info-gap models are **unbounded** ...".
- The point estimate $\tilde{u}$ is poor, to the effect that it can be substantially wrong (e.g. Ben-Haim 2006, pp. 280-281). Indeed, according to Ben-Haim (2010a, p. 2) this point estimate "...is sometimes a **wild guess** ...".
- The quantification of uncertainty is **non-probabilistic, likelihood-free, belief-free, plausibility-free,** and so on (e.g. Ben-Haim 2001, p. 18; Ben-Haim 2006, p. 22).

There are therefore no two ways about it. Unless stipulated to the contrary, ecological applications of IGDT are committed to the characterization of "severe uncertainty" given in Ben-Haim (2001, 2006, 2010), which, whether one likes it or not, commits them to the fact that all the elements of the uncertainty space under consideration are "up for grabs".

All this goes to show that the *Letter*'s narrative on the meaning of the concept "severe uncertainty" informing ecological applications of IGDT is not only unfounded, it is in fact misleading. And yet, because this narrative fails to rebut the thoroughly substantiated airtight criticism of IGDT in Sniedovich (2012), it actually brings out more forcefully the fundamental failing of IGDT, which is: its inherent inability to tackle the very "severe uncertainty" that this theory claims to address.

This fact is made vivid in the next section.

> **Remark:** Prior to the publication of the *Letter* and of Sniedovich (2014), I challenged the *Letter*'s authors to substantiated their claims on the meaning of "severe uncertainty", but to no avail. I therefore challenge them again, here, to explain in what way is the meaning attributed to this term in ecological applications of IGDT different from the meaning attributed to it by the Father of IGDT in Ben-Haim (2001, 2006, 20010, 2010a) and in Sniedovich (2012). In particular, I challenge them, again, to substantiate their claim that in ecological applications of IGDT not all the elements of the uncertainty space under consideration are "up for grabs".

A.6 The No Man's Land

Recall that, by definition, the IGDT robustness of decision q, denoted $\hat{\alpha}(q)$ in (A.3), is equal to the largest value of α such that all the elements of the neighborhood $U(\alpha, \tilde{u})$ satisfy the performance requirement $r_c \leq r(q, u)$.

As shown in Sniedovich (2007, 2010, 2011, 2012, 2012a, 2012b), since the neighborhoods $U(\alpha,\tilde{u}), \alpha \geq 0$ are *nested,* namely $\alpha' < \alpha''$ implies that $U(\alpha',\tilde{u}) \subseteq U(\alpha'',\tilde{u})$, it follows that the value of $\hat{\alpha}(q)$ in (A.3) is not affected at all by values of $u \in \mathscr{U}$ that are outside the neighborhood $U(\alpha^*,\tilde{u})$, where $\alpha^* = \hat{\alpha}(q) + \varepsilon$ and ε is arbitrarily small (but positive).

This means that when determining the value of $\hat{\alpha}(q)$, IGDT's robustness model (A.3) **ignores completely** all the values of $u \in \mathscr{U}$ in

$$NML(q) := \mathscr{U} \setminus U(\alpha^*,\tilde{u}) \ , \ \alpha^* = \hat{\alpha}(q) + \varepsilon \ , \ q \in Q \tag{A.17}$$

Hence,

Definition A.1 *Consider a decision $q \in Q$, and let $\alpha^* = \hat{\alpha}(q)+\varepsilon$, where $\varepsilon > 0$ is arbitrarily small (but positive). We refer to $U(\alpha^*,\tilde{u})$ as the* Effective Domain *of the robustness analysis and to $NML(q)$ as the* No Man's Land *of the robustness analysis.*

The implication is clear:

Theorem A.2 *The IGDT robustness of decision of q at $\tilde{u}$ given r_c, namely the value of $\hat{\alpha}(q)$ in (A.3), is determined only on grounds of the values of u in the Effective Domain $U(\alpha^*,\tilde{u}), \alpha^* = \hat{\alpha}(q)+\varepsilon$. Differently put, the value of $\hat{\alpha}(q)$ is determined in total disregard of values of u in the No Man's Land $NML(q)$. It is as though these values of u do not exist.*

One need hardly point out that this property is inherent to radius of stability models. Hence, the *No Man's Land* effect created by the radius of stability model (A.2) is identical to that created by IGDT's robustness model (A.3), except that in the case of the radius of stability model $\alpha^* = \rho(q) + \varepsilon$.

These facts are illustrated in Fig. A2, where the large rectangle represents the uncertainty space $\mathscr{U}$, the small white circle represents the *Effective Domain* $U(\alpha^*,\tilde{u})$, and the large black area represents the *No Man's Land* $NML(q)$. The basic fact brought out by this illustration is that IGDT is a theory of **local** robustness, meaning that its robustness model (A.3) measures the robustness of decision q against variations in the value of u in the **neighborhood** of $\tilde{u}$. It does not measure the robustness of decision q against the variations in the value of u over $\mathscr{U}$.

The implication of this is clear: a decision that is found to be IGDT-robust may not necessarily be robust against the severe uncertainty in the true value of u, and vice versa. Put another way, the ranking of decisions according to their IGDT robustness is not equivalent to the ranking of decisions according to their robustness against the variations in the value of u over the uncertainty space $\mathscr{U}$. This means that the ranking of decisions according to their IGDT robustness is not assured to yield their ranking according to their robustness to the severe uncertainty under consideration.

This is illustrated in Fig. A1, observing that, according to IGDT, decision q' is less robust than decision q'' even though the shaded area associated with it is much larger than the shaded area associated with q''.

And the trouble here is that there is no way out of this predicament. Because, IGDT does not lay down any provisions requiring that the *No Man's Land* be explored in situations where the Effective Domain $U(\alpha^*,\tilde{u})$ turns out to be extremely small (compared to $\mathscr{U}$). This means that IGDT's robustness model determines unreservedly the robustness of

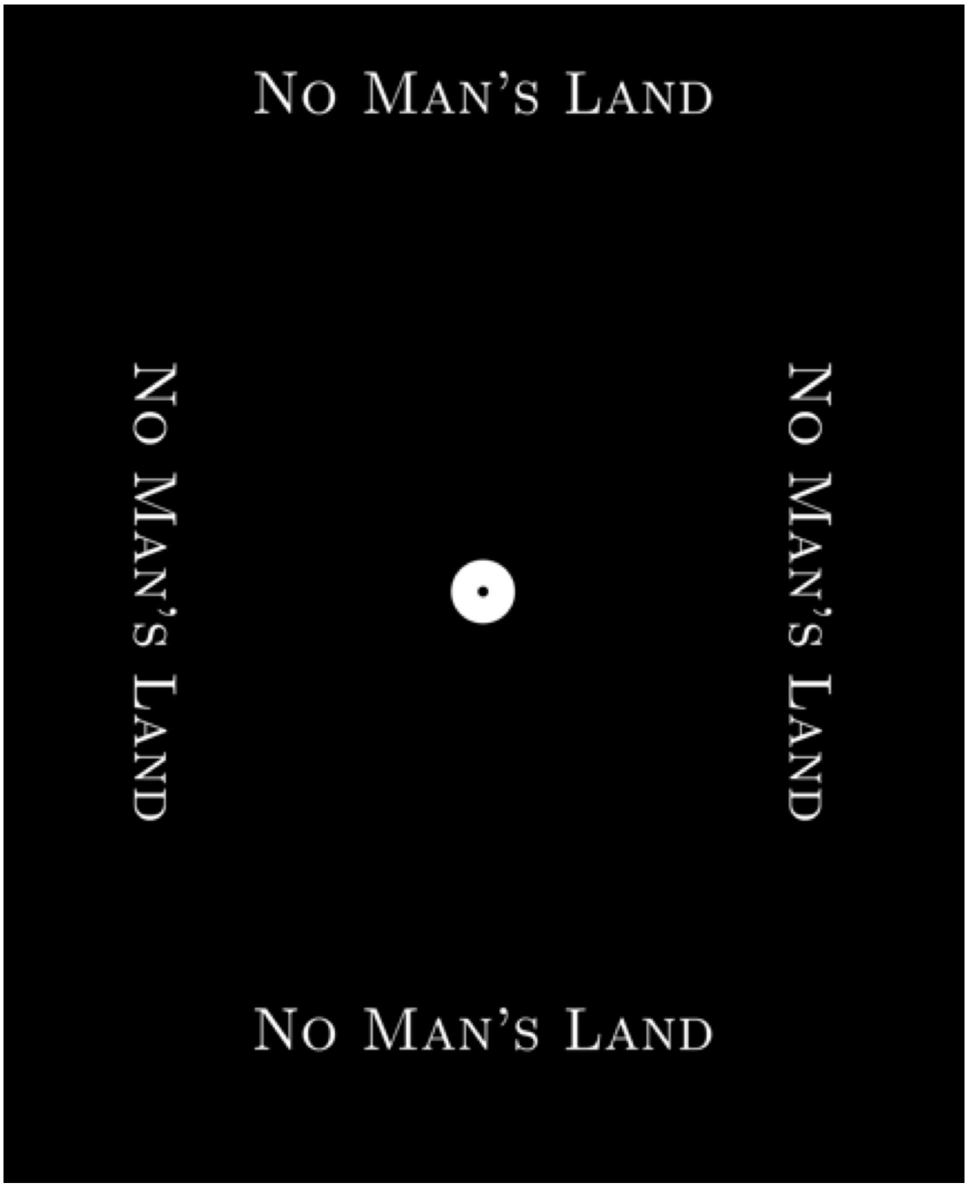

Fig. A2: The No Man's Land effect

decision q even if this determination ignores all the values of $u \in \mathscr{U}$, except those in a small neighborhood around $\tilde{u}$. In such a case the IGDT robustness of decision q stands to be an extremely poor approximation of the decision's robustness against the variations in the values u over $\mathscr{U}$.

The question therefore is: on what grounds can IGDT possibly be claimed to provide a reliable methodology for the treatment of a severe uncertainty of the type that it postulates?

In view of some of the claims in the *Letter*, it is necessary to update of COROLLARY A.3 as follows:

COROLLARY A.4 *IGDT's robustness model is a reinvention of the wheel, and a square wheel at that!*

And this is not the end of the story, because, as indicated in the next section, IGDT's prescription for measuring the robustness of decisions to "severe uncertainty" becomes entangled in an even stickier difficulty.

A.7 Voodoo decision-making

In this section I respond to the *Letter*'s protestations against the use of the term *voodoo decision-making* in Sniedovich (2012) to label IGDT's prescription for the management of a "severe uncertainty" of the type it postulates.

In brief: I remind the *Letter*'s authors, and I call the reader's attention, to the fact that far from being "disingenuous", this labeling in fact captures to a T the **very essence** of IGDT's methodology for decision-making under the "severe uncertainty" that it postulates.

To see that this is indeed so, take note that the IGDT literature emphasizes that the theory is particularly suitable for the treatment of an **unbounded uncertainty.** For instance, according to the Father of IGDT "...most of the commonly encountered info-gap models are unbounded ..." (Ben-Haim 2001, p. 208; 2006, p. 210). Consider, however, what this fact effectively implies. Clearly, it implies that in such cases, the EFFECTIVE DOMAIN in fact turns out to be **infinitesimally small** compared to the uncertainty space. So, the inevitable conclusion is that THEOREM A.2 entails the following:

COROLLARY A.5 *In most of the commonly encountered applications of IGDT, the Effective Domains of decisions are* **infinitesimally small**.

To illustrate, consider again Fig. A2, and observe that in the most commonly encountered applications of IGDT the black area is unbounded and the white circle is infinitesimally small. The question that I therefore put to the *Letter*'s authors is this:

QUESTION A.3 *What label would better describe a theory, proclaimed to have been designed expressly for dealing with a "severe uncertainty" of the type postulated by IGDT, that allows its* EFFECTIVE DOMAIN *to be an* **infinitesimally small neighborhood** *in the uncertainty space?*

To bring out the point made by this question more forcefully, let us put it in more specific terms:

Question A.4 *What label would better describe a theory, proclaimed to have been designed expressly for the treatment of a severe uncertainty of the type postulated by IGDT, that in determining a decision's robustness, takes no account whatsoever of say 99% of the uncertainty space, relying instead on a neighborhood located around a wild guess which comprises 1% of this space?*

I submit that the label *voodoo* is practically tailor-made for describing such a methodology. A quick look at Fig. A2 suffices to see that this is indeed so.

I doubt that the reader needs reminding that the label *voodoo* is being used, in many fields, to designate theories, methods, techniques etc. that are self-contradictory, and/or lack sufficient evidence or proof, and/or are unreliable, and/or make extravagant unfounded claims, and the list goes on.

Indeed, it has become common practice in many scientific disciplines to refer to such theories, methods etc. via the epithet *voodoo*: "voodoo economics", "voodoo science", "voodoo statistics", "voodoo mathematics", "voodoo accounting", and so on.

The labeling of IGDT a *voodoo decision theory* in Sniedovich (2010, 2011, 2012, 2012a, 2012b, 2014) is thoroughly in line with this practice. For the record, the term *voodoo decision theory* is due to Skyrms (1996, p. 51).

I submit therefore that the *Letter*'s protestation

> "To label the application of information-gap decision theory in ecology and conservation biology as "voodoo decision making" is disingenuous, unscientific and unhelpful. More importantly, it reveals a lack of appreciation of the issues surrounding decision making with the types of data available in ecology."

is completely unwarranted.

If anything, this label gives an extremely eloquent characterization of the very nature of IGDT. But more than this, it alerts ecologists and conservation biologists to the fact that the IGDT literature is rife with errors, misrepresentations, unfounded claims, etc. thereby implying that publications describing IGDT and advocating its use should be assessed with care. And to illustrate what I have in mind, consider the following assertion (Burgman et al. 2008, p. 8; emphasis added):

> "Information-gap (henceforth termed 'info-gap') theory was invented to assist decision-making when there are substantial knowledge gaps and when probabilistic models of uncertainty are unreliable (Ben-Haim 2006). In general terms, info-gap theory seeks decisions that are **most likely** to achieve a minimally acceptable (satisfactory) outcome in the face of uncertainty, termed robust satisficing."

Recall that IGDT is proclaimed to be a non-probabilistic, **likelihood-free**, belief-free, plausibility-free, theory. The question is then this:

Question A.5 *How can a* **likelihood-free theory** *possibly seek decisions that are* **most likely** *to achieve a minimally acceptable (satisfactory) outcome in the face of uncertainty?*

I submit that decision-making that is based on such claims amounts to *voodoo* decision-making. For consider the case where a decision-making procedure is said to be based on a **likelihood-free** theory, yet the justification given for using this theory is that the theory seeks decisions that are **most likely** to achieve a minimally acceptable (satisfactory) outcome in the face of uncertainty of the type postulated in Ben-Haim (2001, 2006, 2010, 2010a). Wouldn't this decision-making procedure fully deserve the labeling *voodoo decision-making*?

The same applies to the proposition in Wintle et al. (2010) that a theory of local robustness, such as IGDT, offers a suitable framework for dealing with the "severe uncertainty" associated with *Black Swans* and even ... *Unknown Unknowns*. If this does not amount to *voodoo* decision-making, what does?

A.8 What to do about the Elephant in the room?

As indicated in Sniedovich (2014) and reiterated here, no amount of rhetoric can explain away the fact that IGDT's robustness model is a *radius of stability* model, hence a simple maximin model. And, no amount of rhetoric can explain away the fact that this model of local robustness is utterly unsuitable for the management of a "severe uncertainty" of the type postulated by the theory. This means that the time has come for proponents of IGDT to take the bull by the horns and deal properly with the formal, rigorous, well documented criticism of this theory.

Before I proceed to propose how to do it, namely how to deal with the Elephant in IGDT's room, I want to point out what should be avoided at all cost for the sake of good science.

To be avoided at all costs is the practice to deny, or prevaricate on, the hard facts about IGDT: the fact that IGDT's robustness model is a radius of stability model, hence a simple maximin model; the fact that IGDT prescribes an egregious **misapplication** of this model, etc.

Also to be avoided at all costs is the practice to distort critical assessments of IGDT by twisting them around into endorsements, as done for instance in the *Letter*, where Sniedovich's (2012) criticism purportedly "...reinforces the context and consistency of the approach. These are good reasons to use it, rather than reject it. ..." Indeed, the formal, rigorous criticism of IGDT in Sniedovich (2012) is misrepresented in the *Letter* as "...not detrimental to the application of the theory in ecology. On the contrary, it illuminates the context of information-gap decision theory and reinforces the consistency of the approach ...".

Likewise, to be avoided at all costs is the practice to dodge an airtight criticism, that exposes IGDT for what it is, with the excuse that the criticism is merely "...haggling about terminology...".

Finally, good science demands avoiding the endorsement of a bad theory on the grounds that it is an improvement of an even worse one, as done in the *Letter*:

> "It is clear from the ecological applications to date that it is an improvement to the practice of using best estimates to rank decisions as if these estimates are certain, when no other information is available except the precept that the data available are the best estimates."

Surely, the choice available to ecologists and conservation biologists is not: either the seriously flawed IGDT, or an even more faulty methodology. Surely, the fact that IGDT might be offering an improvement on a more deficient method does not prove the credentials of IGDT. To the contrary, this in fact should motivate ecologists and conservation biologists to explore the well-established literature on decision-making under uncertainty, or consult experts in this field, in order to identify a more viable alternative to choosing between two bad options.

That said, my suggestion to proponents of IGDT therefore is to suspend their tenacious adherence to this theory, at least for a while, and consider Hayes et al.'s (2013, p. 609) sound advice:

> Promoting IGDT as a method for severe uncertainty has helped generate the interest, and controversy, that currently surrounds the theory. It is not, however, the only approach to uncertainty when one cannot reliably identify a precise probability distribution, and it is not the only non-probabilistic theory of uncertainty. It is not therefore a theory that ecologists should feel compelled to use in data-poor situations but rather a choice based on significant reflection about the context of the analysis, and the strengths and weaknesses of the theory.
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> Mathematical work by Sniedovich (2008, 2010a) identifies significant limitations to the analysis. Our analysis highlights a number of other important practical problems that can arise. It is important that future applications of the technique do not simply claim that it deals with severe and unbounded uncertainty but provide logical arguments addressing why the technique would be expected to provide insightful solutions in their particular situation."

A.9 Summary and conclusions

The *Letter*'s invention of a non-existent gap in the state of the art filled by IGDT is indicative of the ongoing effort of proponents of this theory to avoid facing up to the inevitable conclusions deriving from formal, rigorous analyses of IGDT. Hence, the brushing off of a well-substantiated valid criticism of IGDT as "haggling over terminology". This attitude is also indicative of the ongoing pointless effort to justify/legitimize ecological applications of a theory that has been shown to be seriously flawed. And, it is indicative of the effort to preserve the myths that IGDT is "radically different from all current theories of decision under uncertainty" and that it provides a reliable methodology for a severe, non-probabilistic, unbounded uncertainty.

As we saw here, the hard facts attesting to what IGDT actually is tell a different story altogether:

· Not only that IGDT is not "radically different", its robustness model is in fact a reinvention of the well-established model of local robustness/stability that is known universally as *radius of stability* (circa 1960).

· Furthermore, this model is in fact a simple maximin model, namely it is a simple instance of generic models deriving from Wald's famous maximin paradigm (circa 1940).

- The inherent local orientation of IGDT renders it unsuitable for handling the severe uncertainty that it postulates.
- Indeed, this orientation renders it doubly unsuitable for handling the severe, unbounded uncertainty that, according to the Father if IGDT, is typical of the most commonly encountered applications of the theory.
- Since in such applications IGDT ignores completely the entire uncertainty space, except for an infinitesimally small neighborhood around a poor estimate, the inevitable conclusion is that IGDT's approach to a severe, unbounded, non-probabilistic uncertainty is to . . . ignore the severity of the uncertainty.

These facts clearly earn IGDT the title *voodoo* decision theory.

The main objectives of this discussion have been to (a) expose the incongruity between the rhetoric in the IGDT literature and the hard basic facts about it so as to enable readers of this literature, and especially readers of the *Letter,* to form a clear accurate picture of what IGDT actually is and does; and (b) alert readers to the fact that great caution must be exercised when reading this literature.

And as a final note, although IGDT has managed to gain a foothold in ecology and conservation biology, courtesy of its misleading rhetoric, it is hoped that Hayes et al.'s (2013) recently published comprehensive critical assessment of ecological applications of IGDT, will enlighten applied ecologists and conservation biologists on the reasons that publications discussing ecological applications of IGDT should be assessed with care.

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