Isaacs, Breakwell, and their sons

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My dictum is that the emphasis for two-player differential games with full information should be on singular surfaces. Through them will the theory be completed.
Rufus Isaacs, 1969

Abstract

I try to give an account of the early years of Differential Game (DG) theory as seen from the perspective of investigation of the singularities of Isaacs' equation. This is very much autobiographical since I worked on my PhD thesis with John Breakwell from spring 1969 to autumn 1970, and had many interactions with him until his death in 1991. True historians might give a different account... This is also why I opted for the unusual "I" instead of the modest "we".

There is no true bibliography attached. Almost all the papers I quote can be found in one — or both — of the extensive bibliographies I quote, or in the bibliography of [3].

Foreword

This is the as yet unpublished text of a conference given at the 8th ISDG International symposium on the theory and applications of differential games, Chateau Vaalsbroek, Maastricht, NL, and very slightly augmented at the request of Valerii Patsko with new footnotes, and an appendix jointly written by him, whom I thank here, and me.

1 Prehistory

In the 50's, game theory was blossoming, together with Operations Research. While the early work by Borel (1927), Von Neumann (1928) had not raised much

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enthusiasm, the book of Von Neumann and Morgenstern "Theory of games and economic behaviour" (1944) marked the beginning of an intense research effort. To put things in perspective, just recall that Nash's basic papers were published between 1950 and 1953, and that games in extensive form, although first mentioned by Von Neumann in his 1928 paper, were introduced in their current form in a paper of Kuhn, also in 1953. In that paper, — "Extensive games and the problem of information" — the concept of *information* is investigated.

The new theory was seen as part of mathematical economy. Should I adhere to my friend Ivar Ekeland's definition of pure mathematics that they are "*applied* mathematical economy"? Of course, this is witt, but the point is that the status of "applications" of mathematics is very different within the communities of economists and mathematical engineers. We (I belong to the latter) are accustomed to having mathematical models that are precise enough so that the control laws, say, computed from them can actually be applied and give results close to what simulation predicted. Economists have long given up the hope to have such a concept of model. To them, the model is a metaphor that captures one, or some, crucial feature(s) of the economic behaviour of agents, and tries to understand the effect of rational behaviour relative to that feature. A model is more an experiment of thought than a reliable simulation tool.

This state of affairs may explain why there was little communication between the two communities at that time. Of course, engineers were aware of linear programming, but the book of Karlin, to quote one, "Matrix games, programming, and mathematical economics" (1959) was quoted in my engineering courses as a book on linear programming, not for its content in game theory. Yet that book contained Kuhn's description of games in extensive form, the true forerunner of Isaacs' differential games, and many other fascinating aspects of game theory.

One point of divergence between economists' game theory and the emerging theory of DGs was their interest in n-person non-zero-sum games, while Isaacs was interested in military applications, and noticeably pursuit-evasion games, and therefore in two-person, zero-sum games. Our feeling in the late 60's about n-person non-zero-sum games was that these people were busy discussing *what* they should be looking for (solution concepts) while we were practical people, advancing the way to actually compute solutions. This is only one instance of the different attitude towards applications, of course, but I shall come back to similar feelings concerning other mathematical lines of thought. The insistence on actually computing solutions of "practical" examples played a very important role in how DGs evolved.

As always in that type of situations, the founding father was much less ignorant of the parallel developments in Economic theory than his followers. Witness of that is the fact that Rufus Isaacs did discuss his concept of strategies (to be known as state feedback) with Samuel Karlin because he was concerned that state feedback meant instantaneous measurement and processing, an unrealistic assumption. He shortly addressed these difficulties, and called his concept of piecewise constant strategies, that plays essentially no role in the rest of the development, K-strategies, after Karlin.

In 1962, the book "The mathematical theory of optimal processes" by Pontryagin, Boltyanskii, Gamkrelidge and Mishchenko appeared that contains the famous Pontryagin's maximum principle. But more importantly to us here, it also contained a section by Kelendzheridze entitled "A pursuit problem". Whether this must be considered a forerunner of Isaacs' book is debatable. On the one hand, although Isaacs' book was to appear three years later, we know it was finished by then. On the technical side of the matter, in keeping with the rest of Pontryagin's book, Kelendzheridze's work is entirely variational and thus open loop. It deals with a max-min, i.e., a situation where the pursuer knows the whole evader's future control history. As such, it is in a completely different world, even though Pontryagin's adjoint equations are, of course, Isaacs' backward path equations. We shall come back to that later.

2 Isaacs

Rufus Isaacs is the undisputed founding father of the whole field of Differential Games, a name he coined around 1954. His book "Differential games: A mathematical theory with applications to warfare and pursuit, control and optimization" appeared in 1965. But the main content was finished by 1956. The most part appeared in RAND reports dated 1954, and I have personally seen a reprint of the RAND report dated 1951 that contains the "tenet of transition", Isaacs' form of the so-called "optimality principle". I shall come back to the issue of priority and resemblance in the second subsection of this section, after the first analyzing the book in terms of my topic: singularities of Isaacs' equation.

2.1 The book

It should be mentioned that Isaacs had everything to invent, begining with the role of first order differential equations as the appropriate tool. The very concept of state variables and control variables had not been sorted out, much less that of state feedback that he had to invent, with some hesitations as to its being realistic enough. This he did during the late 40's. In 1951, the basic "tenet of transition" was written, and in 1954, the Main Equation — Isaacs' equation as we call it now — and its characteristics, the retrograde path equations, were derived and recognized by Isaacs as the basic tool of investigation of DGs.

Therefore, he tried to determine fields of extremals, and what we now analyze as singularities of the Value function appeared as defects of regularity in the coverage of the state space by the extremals, either because areas of the state space appeared not to be accounted for, or because some parts were covered twice (or more). One then had to "invent" ways of constructing trajectories that would abide by the dictum of minimaximizing the Hamiltonian (this word is not in the book, which Isaacs later regretted), and "repair" these defects. Of course, a lot of intuition of what was going on and what the players "should" do entered in the guess process that led to the description of the proposed field of optimal trajectories.

The most baffling finding was that a defect in the coverage of one region of the state space by the field of trajectories could invalidate a whole part, sometimes remote, of the field thus far constructed. This was a strong showing of the "global" character of DGs. The way this was understood was that the presence of a second player obliged each one to consider "what if" the opponent did some strange things, over a wide range of possibilities, potentially visiting the whole state space.

I feel obliged at this point to include a digression. We still see papers that pretend to "solve a differential game" problem (often times in nonlinear robust control) by computing a state trajectory and adjoint vector trajectory jointly solving the celebrated two point boundary value problem. These papers often begin with "we apply a two sided version of Pontryagin's maximum principle...". Isaacs was already aware of the fact that the trajectory and controls thus computed may, and probably do, have no relationship to the solution of the game, as many simple examples can show. This because of the global character he had discovered.

The first singularities that Isaacs met were barriers. They were first discovered as the limit between a capture zone and an escape zone. This raised the difficult question of the relationship between the "game of kind" (*qualitative game* in the parlance of Blaquière, Gérard, and Leitmann) and the "game of degree" (*quantitative game*). But the real surprise was to find barriers *inside* the capture zone. To tell the truth, this phenomenon was known in the literature, for the problem of Bolza of the calculus of variations, the ancestor to control theory. It can be found in the book of Caratheodory "Calculus of variations and partial differential equations of the first order", the German edition of which dates back to 1924, in a problem attributed by Caratheodory to Zermelo. But for lack of a description in terms of control theory, the development in Caratheodory is difficult to follow. As we shall see, it was clearly not known by Isaacs.

Barriers are borne by "semipermeable surfaces". Isaacs had understood that this concept could be used as the basis of the quantitative theory by augmenting the state space, an avenue which was later followed by Blaquière et al. The book contains a second derivation of the Main Equation based on that approach.

Then universal and dispersal lines and surfaces came. Universal lines are clearly the equivalent of the singular arcs of control theory, although the viewpoint is slightly different. But in both cases, the gradient of the Value function (or the costate or adjoint vector) is continuous across the singularity, not the optimal control, because the maneuverability domain (Isaacs' "vectogram") has a "flat" boundary normal to this gradient. This "flat" boundary arises from a linear vectogram. Isaacs further shows the link with calculus of variations. (This happened before Pontryagin's theory was published). His analysis remains fascinating by the number of examples he shows, and the depth of understanding he displays of each of them. It is interesting to know that one of the justifications for the name "Universal" he gave them is based upon an analysis of the problem to be known later as the "Reeds and Shepp" problem of maneuvering a car.

Dispersal surfaces separate two pieces of the field of extremals that would otherwise overlap. Understanding and computing them is rather easy. But the origin of the problem may be diverse, often involving a conjugate point somewhere in the field downstream. One cannot say that conjugate points were really understood by Isaacs, although he showed many game solutions correctly dealing with such a singularity.

The most amazing occurrence of conjugate points in Isaacs' book is undoubtedly what he termed "termination of barriers". He found 3D games (e.g. the Isotropic Rocket game) where the barrier trajectories have an envelope. We know that in calculus of variation this is a locus of conjugate points. We are not surprised that beyond a conjugate point the trajectory is no longer optimal. The difficulty here was to recognize that fact. It involves a cusp in the barrier surface while all the paths (the only thing we compute) are C^∞. (Isaacs was helped in "guessing" such a possibility by 2D examples.) He also gave a construction of an "envelope barrier" where, although he probably did not recognize it, the junction itself is such a locus of conjugate points on the incoming barrier. It was my luck to work on that problem and give a theory of junction of barriers, inspired by this example.

This is also the only instance I am aware of a singularity of co-dimension 2 that has been elucidated. In fact, in the equivalence between qualitative and quantitative games, it corresponds to Breakwell's switch envelope, a singularity of codimension 1, exactly as the other type of barrier junction corresponds to the equivocal surface.

Finally comes the most amazing of all, "equivocal surfaces". This is a case where the Erdmann–Weierstrass corner condition of the classical calculus of variations — that the adjoint vector must be continuous along an optimal trajectory — is violated, hence a phenomenon that can have no counterpart in calculus of variations. Isaacs states this last fact in the book, although he rests upon a deeper understanding of what really happens to ascertain it, not recognizing the violation of

Erdmann–Weierstrass. Yet that discovery is absolutely crucial, since it definitively rules out an analysis of Isaacs' equation based solely on variational arguments and open loop controls, in the fashion of the theory of feedback synthesis as developed in control.

I quote from the book, p. 282:

These surfaces, unlike many types already studied, have no counterpart in the calculus of variations. They cannot occur in a one player game. The implication is that the underlying theory of differential games must essentially depart from any mere extension of classical ideas.

2.2 The man

The first time I met Rufus Isaacs was at the "First International Symposium on the Theory and Applications of Differential Games" held in Amherst in 1969, largely due to the initiative of Yu-Chi Ho and George Leitmann. He was obviously invited to give the opening talk. It went roughly as follows.

In the program, I see many papers dealing with existence of solutions, or other very interesting theoretical properties. May I suggest that each author who gives a talk on general theory tells us how it applies to the small example I now give...

Then the Obstacle Tag Chase game and its seeming paradox came, taken from the book (p. 134; the "research problem 6.10.1", p. 152). I shall tell later a historically important side effect of that talk.

This is to insist on the fact that he did not believe in general theory. He has a paragraph in the book to say that it is not feasible. Of course, we now have pieces of theory, useful existence theorems for viscosity solutions for instance, and so on. But this mood played a central role in the development; I shall come back to it.

At that time, he was rather bitter. He had the feeling that both Bellman's optimality principle and Pontryagin's maximum principle were particular cases of his theory, the first one stolen from him since Bellman attended his seminars at RAND. He was wrong concerning Pontryagin's maximum principle, clearly, since it gives a necessary condition of optimality, something we still do not have for DGs.[1]

As for the precedence, one is obliged to recognize that the use of the Hamilton–Jacobi equation as a tool to construct a sufficient condition of optimality (Isaacs' Verification theorem) is in Caratheodory, as a follow on to ideas of Weierstrass which are also at the starting point of Pontryagin's theory, and Breakwell's work on conjugate points. The Value function is called there the Principal function.

[1] I disagreed on that point with my late friend Austin Blaquière.

2.3 Last remarks

Somewhere in between the book and the man who wrote it, one must say a few more words about this exceptional book. For one thing, it took people like Breakwell, Ho, Fleming and the like to be able to read it. Larry Ho published a famous review [1] where he proposes "a battle plan for reading the book". Then he guides the reader through a seemingly tortuous tour of the book, that makes it readable, if not without pain, to a control theorist. It takes no less than that. Still according to Larry Ho's review, I quote:

It also helps to be an English major when reading the book. On the first 44 pages one finds words such as "adumbration, assuage, aliquot, outre, cogeries, jejune, hoary, desultory, consort, pristine, and proscription".

In 2D coordinates, it measures angles positive from y-axis to x-axis. At one point, it discretizes a 2D state space on an hexagonal mesh. (Mathematical morphology has given a great status to that trick.) Examples abound. But they are taken at one point, continued three chapters later, sometimes finishing with problems left to the reader, and "research problems" several of which were the PhD topic of Breakwell's students, including Tony Merz and myself.

Before I switch to Breakwell's contribution, a last remark is in order. It is to point out that all this fascinating work of Isaacs is made relying solely on closed form integration of the differential systems involved. This is quite a feat considering the wide variety of examples treated in the book. I do not resist the pleasure of quoting from the book (p. 246), although there is some cheating involved here...

It is about the retrograde path equations of the Isotropic Rocket game. The equations are as follows, in Isaacs' notations (or almost)

$$\begin{aligned}
\frac{\mathrm{d}x}{\mathrm{d}\tau} &= F\frac{yU}{v^2\rho_1} - w\frac{\nu_1}{\rho_2}, & \frac{\mathrm{d}\nu_1}{\mathrm{d}\tau} &= F\frac{\nu_2 U}{v^2\rho_1}\\
\frac{\mathrm{d}y}{\mathrm{d}\tau} &= -F\frac{xU}{v^2\rho_1} - w\frac{\nu_1}{\rho_2} + v, & \frac{\mathrm{d}\nu_2}{\mathrm{d}\tau} &= -F\frac{\nu_1 U}{v^2\rho_1}\\
\frac{\mathrm{d}v}{\mathrm{d}\tau} &= F\frac{\nu_3}{\rho_1}, & \frac{\mathrm{d}\nu_3}{\mathrm{d}\tau} &= F\frac{U^2}{v^3\rho_1} - \nu_2.
\end{aligned}$$

Here, F and w are parameters, as well as l and s that enter into the terminal conditions,

$$U = \nu_1 y - \nu_2 x, \quad \rho_1 = \sqrt{U^2/v^2 + \nu_3^2}, \quad \rho_2 = \sqrt{\nu_1^2 + \nu_2^2}.$$

Here the quotation begins:

The closed integration of this system, with the foregoing initial conditions, presents its elementary difficulties, but the result is (we use as a convenient ab-

breviation $W = FT - w$):

$$\begin{aligned} x &= \frac{\pm\sqrt{s^2-w^2}}{v}\left[l - w\tau + \frac{1}{2}F\tau^2\right], & \nu_1 &= \frac{\pm\sqrt{s^2-w^2}}{v} \\ y &= \frac{1}{v}\left[\left(\frac{1}{2}F\tau^2 - l\right)W + (s^2-w^2)\tau\right], & \nu_2 &= -\frac{W}{v} \\ v &= \sqrt{s^2-w^2+W^2}, & \nu_3 &= \frac{W\tau}{v}. \end{aligned}$$

End quote.

3 Breakwell

Among the few scientists who understood the depth of Isaacs' work, John V. Breakwell was almost the only one who dared continue in the same track, attempting to close problems left open in the book and solving new examples via the construction of complete fields of trajectories, thus discovering new singular surfaces. But to that problem he brought the power of the computer. With it, we were able to "see" fields of trajectories (though we had no graphical tools, except patience and the ruled paper), and infer new phenomena.

3.1 Breakwell's contribution

I shall again focus here on the theory of singular surfaces. The first such surface discovered by John Breakwell, I believe, was the switch envelope.

This was before I started working with him in 1969. I am not sure in which game he discovered it. Was it the Homicidal Chauffeur? What I do know is that the first published account is in his joint paper "Toward a complete solution of the homicidal chauffeur game" with Antony W. Merz at the Amherst conference (see below). This is a very important discovery, since it is the other case where the Erdmann-Weierstrass condition is violated. Knowing both cases was the starting point of my own piece of theory concerning corners in differential games. That theory shows that, under fairly general conditions, these are the only two ways in which the gradient of the Value function can be discontinuous along an optimum trajectory. But within that apparent simplicity, it was later discovered that many subcases can show up. As recently as 1995, Lipman, for instance, working with Josef Shinar, unveiled new forms of equivocal lines.

Then the First International Symposium on the Theory and Applications of Differential Games came, held in Amherst in autumn 1969. (This was the first scientific meeting I ever attended.) It was organized at least in part by Yu-Chi (Larry)

Ho and George Leitmann. In his opening talk, Rufus Isaacs challenged any later speaker who would talk about existence, unicity, regularity and such properties of differential game solutions, to say how this applied to the example of the Obstacle Tag Chase and its apparent paradox. John Breakwell knew the book better than anybody else, but apparently this problem had not caught his attention before. He later admitted that he did not listen to any other at the conference in Amherst (he attended them all!), because he was too puzzled and thinking about that example.

In 1967, in "Pursuit around a hole", Naval research quarterly, Isbell had proposed a solution, which John Breakwell found to be in error. Yet it contained a good idea. I know this from Breakwell's later papers on the topic. But I do not know when he became aware of that work. Breakwell was intrigued by the "perpetuated dilemma", and this was an instance of one. So he might have noticed Isbell's paper early. Anyhow, he would later call his first conjectured solution "Isbell's modified solution".

When we came back from Amherst, John was still thinking about this problem. At that time Tony Merz was trying to finish up the Homicidal Chauffeur game, a game with seemingly endless new difficulties. He was busy trying to "fill" a small part of the state space, still unaccounted for after a couple of dispersal lines and switch envelopes. One day he came to John with a conjecture on how to fill it. Immediately John understood that there lied the solution, not only of the Homicidal Chauffeur, but also of the Obstacle Tag and a couple of other puzzles he had. This was what Tony was to call a focal line. As far as I know, the first written account is in his PhD thesis, held shortly before mine in 1970: "The Homicidal Chauffeur, a differential game". (The Stanford Report is dated 1971.)

This was the beginning of the theory of focal lines, by no means its end. On the one hand, in a later paper[2] Breakwell shows that a further condition — indeed quoted in my thèse d'État, but I had never tried to apply it anywhere — invalidates part of that construction. He attributes that discovery to W. Rzymowski, private communication, but does not manage to find somebody else's name for the new condition (apart from quoting my thèse d'État). This is a very strong indication that he did it himself. He then gives a revised solution to the Obstacle Tag, involving two switch envelopes, and, ironically, no more focal line[3].

On the other hand, my attempt at a general theory of singular surfaces (first published in part in "Differential games and applications", edited by Oslder, Haggedorn, and Knobloch, the proceedings of the Twente symposium of 1977, — another important symposium in the history of DGs —) predicted the existence of "singu-

[2]"Sufficient conditions in zero-sum differential games", 1980? My copy is undated, and I could not find a reference to it anywhere. It quotes my thèse d'Etat, 1979.

[3]The absence of a focal line in the solution of this game is a consequence of Melikyan's theory as presented in this symposium, as he pointed out to me.

lar focal surfaces", that should be reached non tangentially by the optimal paths, contrary to all known examples. However, I had failed to find an example. In 1988, John visited me in Sophia Antipolis, and I talked to him about that question. He quickly explained to me where I mistook in my attempts, and how to modify the Obstacle Tag to produce such an instance. And after a few weeks, he had an all new example, simple enough that everything would integrate in closed form.

Yet another problem arose. In many singular surfaces, one of the players has to react instantaneously to his opponent's control. Or more precisely, to construct the paths lying on the surface, we need to let that player's strategy depend on the opponent's control in addition to the state. (What we would term an upper or a lower Value in modern terms.) John had long considered this too unrealistic, and had, in each case, shown how to approach the Value with purely state dependant controls, sometimes with hysteresis. Yet, in the case of focal surfaces, the argument was subtle and crucially depended on the fact that optimal paths reach the focal line tangentially. How about singular focal lines then?

We were the more puzzled that a general theorem by Avner Friedmann showed that such ε-efficient state-feedback strategies should exist. We took Friedmann's proof, and carefuly applied it to that problem. That way, we ended up with a refined approximating scheme, which did the job, without hysteresis. (I am convinced that the same trick can be used to remove the dependence on hysteresis in other cases as well.) This was, as far as I am concerned, the first instance I knew of a collaboration between those general theories, that we did not overestimate, to say the least, and the practical construction of game solutions. All that appeared in a joint paper in JOTA, 1990.

Meanwhile, barriers also underwent some further refinements. Isaacs had shown the "natural barrier", the "envelope barrier", and variations on the theme of natural barriers. While working under the supervision of John Breakwell, the Isotropic Rocket forced me to invent a type of barrier where the constituting trajectories all meet at a single point. I was also led to the investigation of junction of barriers, to discover that Isaacs' envelope barrier was an instance of more general phenomena of barriers joining along an envelope of the incoming paths. This is the equivalent of switch envelopes. An equivalent of equivocal surfaces may also exist in junction of barriers. One was found by Breakwell and Lewin in their surveillance game, published in 1975.

Breakwell was also at the inception of the theory of games where the role of pursuer and evader are not predetermined, to model aerial dogfight duels. A first paper with Geert Jan Olsder, "Role determination in an aerial dogfight" appeared in 1974. This is on the border of our topic, though, since the contribution there is more about new concepts — win zones, draw zone, mutual kill zone — than on new singular surfaces. They are in effect separated by barriers. However, these barriers may present some original characters. This was further investigated by Davidowitz and Shinar (1985–1989), for instance.

3.2 The man

John Breakwell was a dynamical astronomer and a musician. A game theoretist was only as a hobby. A dedicated American citizen — he gave up his series of Citroën 2CV in favor of a Chevy to "buy american" —, he was born (in Switzerland) British, and had kept a British sense of humour. He was an impressive singer. Give him any score, he would at once sing it in his great bass voice and play it on the piano with no rehearsal. He would play as well any score of classical music of course, his favorite being Frederic Chopin. (He composed some piano pieces which he attributed to "Frederic Breakwell".) Famous for his imitations of foreign accents, he enjoyed life and would call himself a lazy man.

John Breakwell would hardly bother to publish general theories. Only problem solutions. He was fully aware of the general applicability of his ideas. But he found it so obvious that it was not worth writing. His 1959 paper "On the optimization of space trajectories" contained most of Pontryagin's maximum principle. Yet, I never heard him request any precedence over Pontryagin, to whom he was friend. He also implemented in effect a Kalman filter to do the tracking of Explorer One in 1958. He even used the bias he observed in the residue (the innovation process) of a particular measuring point to correct the location of the Pacific island it was taken from, thus pioneering space geodesy. But he would not talk much of his precedences, and that work, on "high altitude trajectories" (as orbits were code named at Lockheed for security reasons) was classified.

It was always difficult to attribute a result to him in a paper. He would always find somebody else to whom give the credit. His theory of switch envelope he would present as an application of "Bernhard's condition" — I noticed above that he did it before I knew anything about DGs and research —, and so on. I never heard him tell badly of anybody.

His last paper on DGs was a posthumous one with Tony Merz, "Football as a differential game" in the Journal of Guidance and Control, 1991. Tony had remained a close friend of him, going for long mountain hikes with him, and served as a link among the former students of John, who all were his friends.

4 And their sons

(I am not a male chauvinist, I just happen to know no female student of either two.[4]) I know of only one student of Isaacs in DGs, P.L. Yu. He proposed "bang-bang-bang surfaces", where both controls are discontinuous[5]. I just learned from Steve Alpern that his work on Princess and Monster was done as Isaacs' post doc.

John Breakwell had many students working with him in DGs. I know at least Tony Merz, myself, Josef Lewin, Geert Jan Olsder, Bernt Järmark.

Josef Shinar cannot be described as a student of Breakwell. A friend surely. And his work in differential games started with his interaction with John. He is one of those who had the largest number of students working in that same direction (Davidowitz, Lipman, Farber, others ?...) all contributing to variations on the themes developed here.

Olsder is here to tell us whether he had many students in that domain. I do not think so.

Järmark published with Hilberg (is he his student?).

I had a couple of students working on the singularities of Isaacs'equation: J.-F. Abramatic, J.-F. Masle, P. Colleter, Anne-Laure Colomb, Odile Pourtallier, S. Le Menec, A. Rapaport.[6]

All these people added to our understanding of singular surfaces, and to the incredible variety of figures and arrangements they can form. The state of affairs is that solving a 3D differential game that way remains a formidable task for any new example. I believe it is time we try to turn that art into technique, and this is what might result from what follows.

5 Melikyan

A major late contributor to that theory is Arik Melikyan, who brought to bear the theory of singular characteristics of a first order PDE. He and his student Naira Hovakimyan used that theory to construct singular surfaces on manifolds more complicated than $\mathbb{R}^n$, or $\mathbb{R}^n \setminus B$ (B is the unit ball, this is the case of the Obstacle Tag Chase game).

I wish I were able to write more about it. They have a very powerful tool at hand. We expect to use it. Arik and I want to solve an open problem in the theory of focal manifolds. This is time I mention that this theory is still unfinished

[4]Footnote 2015: John Breakwell had scientific "granddaughters".

[5]At the time this appeared (in the French translation of the book), I had doubts over that analysis. I did not look at it since then.

[6]and later Frédéric Hamelin with whom we discovered bi-singular fields of trajectories. Note of 2015.

as compared to the other singularities. An open question remains: is it possible that there exist on the same manifold two different fields of optimal trajectories, one related to each side of the manifold? Arik conjectures that this is possible, but maybe for nonlinear PDEs that cannot arise as Isaacs' equation of a game. I conjecture it is not possible in games. We are trying to find the means to make a coordinated attack on that problem. Because of that unknown, I was not able to extend to focal surfaces the construction I gave for other singular manifolds[7].

6 Practical mathematics

Nobody today would challenge the fact that Isaacs' work was a genial piece of mathematics. Yet it lacked the mathematical rigor of modern mathematics. By "pure maths" standards, none of his theorems is really proved, yet all his inventions stand. He was aware of that fact, as a short section of his book, "A perspective on precision", shows. It is immediately followed by the section entitled "A perspective on progress", to mean that in the solution of practical problems are the really interesting questions.

The main problem, in fact, is not alluded to in the book. It stands as follows. Isaacs understood early the need for state feedbacks, or strategies. The blind cat does not catch mice. He sought optimal strategies, of course, but then optimal *among what set*? The problem is that if one sticks with continuous strategies, the solution is lost, since it so often is discontinuous, but giving up continuity leaves us without guarantee that a solution to the dynamical equation will exist. The playability concept was invented by Blaquière, Gérard, and Leitmann in part to solve that problem, but only at the expense of giving up the zero-sum character of the game, and with it the normative value of the strategies.

It can well be said that the choice to ignore that problem was one of the main features of genius of Isaacs, since all those who tackled it were led to considerations light years away from practical construction of solutions. I am thinking here of the work of W. Fleming — which was at the starting point of viscosity solutions, quite important 30 years later —, A. Friedman — I already said in what circumstances I recognized its practical value —, N.N. Krasovskii and A.I. Subbotin — still of major influence in the strong Russian school —, etc. All these works proved their worth way later. But pressing forward without a formal setup proved to be extremely fruitful.

[7] Note of 2015. Melikyan's conjecture was proved correct, and published with an example as a joint paper: A. Melikyan and P. Bernhard: "Geometry of optimal trajectories around a focal singular surface in differential games", Applied Mathematics and Optimization. Vol. 52, no. 1, pp. 23–37, 2005.

As a matter of fact, John Breakwell knew the key point to answer that problem. But as usual he would not bother to write such simple things. The first written account I know of it is due to Berkovitz, and I call this fact the Berkovitz lemma. I am not sure whether this was in his Annals of mathematical studies of 1964, but surely not later than his "Lectures on differential games" in "Differential games and related topics" (1971). The point is that *it suffices to test the optimality of the feedback strategies against open loop controls of the opponent*. Once this is understood, it is clear that the problem has vanished. I used that fact to propose a setup where the Isaacs–Breakwell theory is fully warranted, but this is only formal play.

Breakwell added to that "practical" viewpoint by admitting the result of numerical computations as "evidence", if not "proof". When we saw a field of trajectories clearly cutting into some manifold for some values of a parameter, clearly avoiding it for others, we took it for granted that indeed, this was so, and that there was thus a limiting parameter value, that we tried to determine numerically, for which the trajectory was tangent to that manifold. We had no quantitative bounds on the precision of our numerical schemes but we did some simple experiments with the step size to convince ourselves that indeed we were contemplating the "true" behaviour of the solution. Again, this practical viewpoint was what made these investigations possible, and I believe all that is correct, if not really proved.

References

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[3] E.Y. Rodin: "A pursuit-evasion bibliography—Version 1". Computers and Mathematics with Applications, **13**, pp. 275–340, 1987.

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Appendix: Explanations of terms

1. Obstacle Tag problem was formulated in the book by R. Isaacs. Isaacs refers to the kids' playground game of "tag". He made a pool as the obstacle.

How do the pursuer P and evader E act? It is clear that a dispersal surface appears in the phase space of this problem (Fig. 1). But whether this surface is the only singular surface or not?

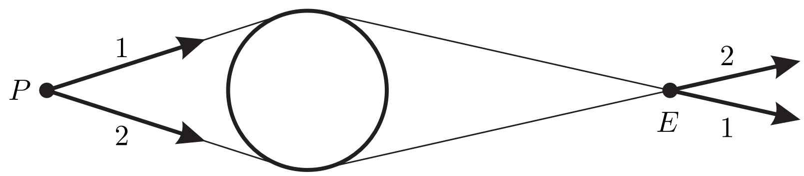

Figure 1: Obstacle Tag problem, dispersal situation

The paradox is as follows. Take two starting positions not aligned with the center of the pool. Say that the line joining them lies "above" the center of the pool (Fig. 2). The "obvious" solution is that P should run toward the upper limb of the pool, on the straight line joining his initial position tangentially with the pool's circumference, while E should run away from the pool, along the line tangentially joining her initial position to the upper limb of the pool. When P reaches the pool, he should run along its circumference until he reaches the tangency point of the line along which E is fleeing away, and chase her along that straight line.

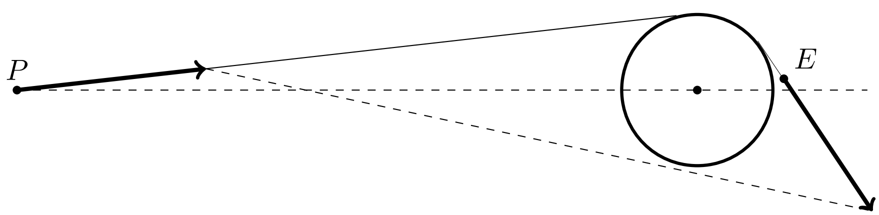

Figure 2: Paradox in the Obstacle Tag game: ends of thick arrows show positions after one time unit along the (erroneous) "obvious solution". (P is faster than E.)

The paradox comes when one shows intial positions, with E close to the pool and P rather far away, where this chase would lead at a later time to positions where P "sees" E directly under the pool, and should therefore be chasing her in a completely different way, which shows that E has not behaved optimally in that "obvious" optimal strategy.

2. In Fig. 3, one can see geometry of optimal trajectories in some small neighborhood of singular lines of different types in differential games in the plane. The symbols p_0 and p_1 denote the gradient vectors of the value function on the different sides of the lines.

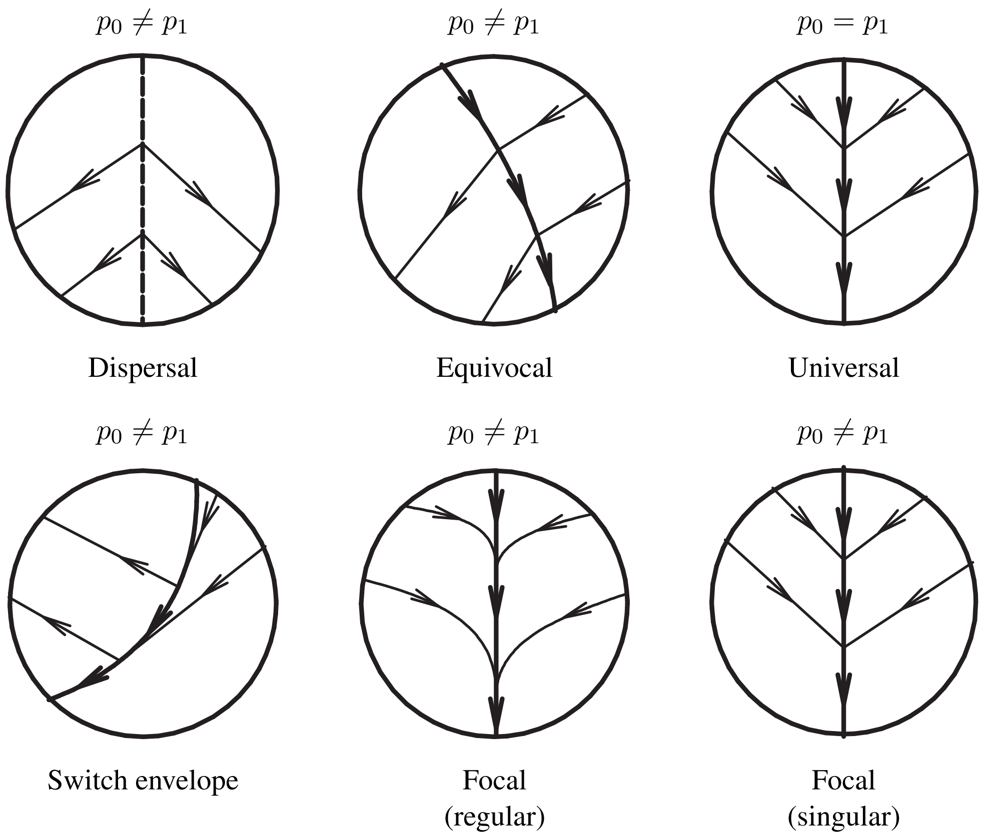

Figure 3: Main types of singular lines