A critique of
Luca Lambertini
Differential Games in Industrial Economics
Cambridge University Press, 2018
Sections 1.2 “Optimal control theory” and 3.1: “Sticky Prices”

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The book

This book, while interesting by its many applications, suffers from serious technical weaknesses, making it a verty misleading introduction to the topic of optimal control and differential games applications in (industrial) economics. I only quote here my remarks concerning two subsections of particular interest to me.

1 Chapter 1, section 1.2: Optimal control theory

To start with, I must point once more to a very harmful mistake that pervades the whole book, as well as many other articles on the application of optimal control theory in economics. In page 4, the author is providing the solution of the simplest optimal control problem of maximising with respect to a control function $u(\cdot)$ a pay-off Π given by

$$\dot{x} = f(x,u,t)\,, \quad x(0) = x_0\,, \qquad \Pi(x_0,u(\cdot)) = \int_0^T \pi(x(t),u(t),t)\,dt\,.$$

He rightly introduces an adjoint variable $\mu(t)$ obeying the classical adjoint equation. The first line of p. 4 specifies that, I quote, “the terminal condition $x(T)$ is left free.” Then comes the formal statement of the Pontryagin maximum principle where,

- on the one hand, the fact that the optimal $u^\star$ maximizes the hamiltonian is *not* formally quoted as part of the theorem (however an ambiguous sentence stated before the formal theorem is “the constrained optimization problem [. . .] is formally equivalent to maximising the Hamiltonian [. . .]”)
- on the other hand, the transversality condition given is **plain wrong**. I quote: $\mu(T)x^\star(T) = 0$.

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According to this statement, trajectories ending at $x^\star(T) = 0$, but not *constrained* to do so, may have an arbitrary $\mu(T)$. Obviously wrong. (Incidentally, varying μ at 0 would generate a new field of exremals intersecting with the primary field, requesting a further choice among candidate optimal trajectories from each initial condition.)

I have seen this mistake in several other articles in economics. This does not make it correct.

I should also mention that the footnote saying that Isaacs' Tenet of transition is "equivalent" to Pontryagin's Maximum principle is also wrong. The Tenet of transition is the game equivalent to Bellman's Optimality principle, a sufficient condition, while the Maximum principle is a necessary condition, embodying the Erdman-Weierstrass corner condition which, incidentally, does not apply to closed-loop differential games. (And therefore the Maximum principle either.)

2 Chapter 3, section 3.1: Sticky prices

In a previous note[2] I detailed the accumulation of gross mistakes in the article [Cilleti and Lambertini, 2007] on the topic of this section 3.1. That article is *not* quoted by the present book.

2.1 The problem

The problem considered is what one would expect, with a classical weak point of the Economics literature: for instance, the market clearing price is written as

$$\hat{p} = a - q_1 - q_2 \,,$$

where the q_i, $i \in \{1, 2\}$ obviously are the production (rates) of the two players. This is a very unfortunate "simplification" of what I would like to write:

$$\hat{p} = a - b(q_1 + q_2) \,.$$

As a matter of fact, $\hat{p}$ is a price, in unit of currency per unit of production, while the q_i, are production rates in unit of production per unit of time. These two quantities cannot be directly compared, summed or equated. The same occurs with the production cost unfortunately written $C(q) = cq + q^2/2$. With such notations, there is no way to check whether formulas are dimensionnaly correct. And this explains several caculation mistakes in the rest of the text.

[2] available upon request

Therefore, I translate the problem at hand as:

$$\hat{p} = a - b(q_1 + q_2)\,,$$
$$\dot{p} = s(\hat{p} - p)\,,$$
$$C(q) = cq + \frac{d}{2}q^2$$

and the profits of the two players are (equation 3.6 in the book):

$$\Pi_i = \int_0^\infty \mathrm{e}^{-\rho t}\left(p - c - \frac{d}{2}q_i\right) q_i \,\mathrm{d}t\,. \tag{1}$$

The reader may remember that the book is written with $b = d = 1$ (The notation $b = d$ does not violate my homogeneity dictum, because they happen to have the same dimension !)

The author rightly points out that this problem only makes sense if $a > c$.

2.2 The analysis

2.2.1 The classical results

Subsection 3.1.1 gives an allmost correct analysis of the open-loop equilibrium of the game. Yet, in obvious reference to the wrong transversality condition of the first chapter, it uses the transversality condition at infinity $\lim_{t\to\infty} \mathrm{e}^{-\rho t}\lambda(t)p(t) = 0$. It has no sound mathematical basis either. See [Cannarsa and Frankowska, 2018].

Subsection 3.1.2 gives the classical analysis of the linear feedback solution of this LQ differential game, in terms of the Algebraic Riccati Equation (ARE) and related linear equations for the nonhomogeneous terms. One may just notice that the author does not seem to be aware of the fact that everybody knows that the solution of the ARE sought is the minimal one. He writes "For the moment, we may confine our attention to . . . ", and never comes back to the question. But the solution given is correct, and the ensuing discussion of the limit cases interesting.

2.2.2 Nonlinear solutions

The author then turns to the application to that problem of [Tsutsui and Mino, 1990], referred to in the sequel as T&M, an article and a theory I did not know previously, exhibiting a continuum of nonlinear state feedback equilibrium strategies.

The subsection starts with the surprising self-contradictory sentence "[procedures] which depart from imposing symmetry accross firms in the FOC, and then solving it w.r.t. $V'(p)$." Of course, if there only is one Value function V (without index) this means that a symmetric problem is considered. (To tell the truth, the article T&M is somewhat misleading in that respect. It does start with a non-symmetric game, but quickly switches to the symmetric case.)

After quoting a sentence of T&M referring to a "folk-theorem-like" property, as if it applied to the problem considered so far, comes a paragraph alluding to "restrictions in the state space", with no more precision. (A game theoretist would ask which player is responsible for ensuring this "restriction", if it were made explicit.)

The fact, not explicitely mentioned in the book, is that **the problem solved in T&M is *not* finding a Nash equilibrium for the criteria** (1).

In contrast, the problem considered in T&M, nowhere stated in the book, is as follows: with the same dynamics, given a compact interval $\mathcal{I}$ and its boundary $\partial\mathcal{I}$, for any $p(0) = p_0 \in \mathcal{I}$ and any pair of controls $(q_1(\cdot), q_2(\cdot))$, let

$$T_{\mathcal{I}}(p_0, q_1, q_2) = \inf\{t \mid p(t) \in \partial\mathcal{I}\}.$$

That is, $T_{\mathcal{I}}(p_0, q_1, q_2)$ is the first time when the price reaches $\partial\mathcal{I}$ under the action of the controls, possibly $+\infty$ if it never does. The pay-off of player i is

$$\Pi_{\mathcal{I}}(p_0, q_1, q_2) = \int_0^{T_{\mathcal{I}}(p_0,q_1,q_2)} \mathrm{e}^{-\rho t} \left(p(t) - c - \frac{d}{2} q_i(t) \right) q_i(t) \,\mathrm{d}t\,.$$

Now, this is quite a different problem. The very fact that it admits linear state feedback Nash equilibrium strategies requires some thinking.

Then, the book follows the begining of the development of T&M, but stops after solving a necessary condition for a (twice continuously differentiable) function to solve Isaacs'equation (itself a sufficient condition), as if being the solution of a necessary condition were being a solution of the problem. This necessary condition being obtained by one more differentiation than usual, the reader is not surprised to find an *unknown* constant, treated in the book as being an *arbirary* constant in the solution of the problem at hand.

T&M goes beyond and explicitely constructs a sufficient condition for the problem considered for various values of the integration constant (some real, some complex, a fact *not* mentioned in T&M). It does exhibit a continuum of non linear equilibrium stategies. The maximum extension of the interval $\mathcal{I}$ depends on the particular non linear strategy considered, but a smaller fixed interval may correspond to an infinity of strategies and pay-offs. But the "folk-theorem-like" statement quoted by the book does not hold for a unique interval $\mathcal{I}$.

In chapter 7, the book claims to derive an infinity of non linear Nash equilibrium strategies for a very similar linear quadratic problem, with no restriction on the state space. I did not check that claim, but I refer to corollary 2 of theorem 3 in T&M which (in an ambiguous way) states that the only "global" state fedback Nash stategy is the classical linear one, as the graphic clearly shows. (See my note about T&M.)

References

[Cannarsa and Frankowska, 2018] Cannarsa, P. and Frankowska, H. (2018). Value function, relaxation, and transversality conditions in infinite horizon optimal control. *Journal of Mathematical Analysis and Applications*, 457:1188–1217.

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