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Discussion on: "Equivalence of Different Realization Methods for Higher Order Nonlinear Input–Output Differential Equations"

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When different approaches to a certain subject are ripe enough, this is usually a good moment for an attempt to compare them and to unify different paths into one well-paved way. However this usually requires a hard work and is often a nonprofit activity. The hard thing is finding a common language and good notation, in which all these different approaches could be nicely expressed. And often it appears that what one gets looks like a simple exercise or is a part of the unwritten folk knowledge. Nevertheless someone has to do such a job, as this is a necessary step in building up a foundation of a higher level theory.

Kotta and Mullari have undertaken a job of comparing different realization methods for higher order nonlinear input–output differential equations. By realization of the equation

$$y^{(n)} = \varphi(y,\, \dot{y}, \ldots, y^{(n-1)},\, u,\, \dot{u}, \ldots, u^{(s)}) \tag{1}$$

they mean a state space representation of the form

$$\dot{x} = F(x,u) \tag{2a}$$

$$y = h(x,u). \tag{2b}$$

where the state x evolves in $\mathbb{R}^n$. Moreover they want to express x as a function of $y, \dot{y}, \ldots, y^{(n-1)}, u, \dot{u}, \ldots, u^{(s)}$, which constitutes a more algorithmic part of the theory. However the authors do not say precisely when (2) is a realization of (1). This is probably caused by a more technical or operational understanding of the subject, which is present throughout the paper. As a mathematician I would rather recall van der Schaft's definition which can be found in Ref. [4] – one of the sources of the discussed article. Namely, (2) is a realization of (1) if the (external) behaviors of the two systems coincide, where the behavior of (1) or (2) is the set of all pairs (u,y) that satisfy (1) or (2) (for some trajectory x), respectively. The functions u and y may be defined on the whole real line as in Ref. [4], or only for $t \geq 0$, or on different intervals depending on (u,y). Another vague concept is *locality* of the realization, which appears in Ref. [4] and is repeated in the paper by Kotta and Mullari (Theorem 2). "Locally" means in a neighborhood of a point, but it is not clear which point.

An interesting problem is the class of functions that describe the systems and appear in later constructions. The function φ in (1) is assumed to be smooth, but nothing is said of F and h in (2). This is one of the weak points of the paper. Sewing up two different languages, this of Ref. [4] using vector fields and that of Ref. [3] relying on differential forms, appears not as seamless as it could be. The smooth category is natural in the first case, while the differential forms exploited in Ref. [3] have coefficients in the field of meromorphic functions of variables $y, \dot{y}, \ldots, y^{(n-1)}, u$, $\dot{u}, \ldots, u^{(s+1)}$. To make sense of the construction of the spaces $\mathcal{H}_k$ (Section 3.1 of the discussed paper) one has to assume that the vector field

$$f = \dot{y}\frac{\partial}{\partial y} + \cdots + \varphi(\cdot)\frac{\partial}{\partial y^{(n-1)}} + \dot{u}\frac{\partial}{\partial u} + \cdots + u^{(s+1)}\frac{\partial}{\partial u^{(s)}}$$

(understood as a differential operator) has meromorphic coefficients, so φ should be meromorphic.

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Moreover, the consistent language of Lie derivatives (of vector fields, differential forms and functions) used in most of the paper is compromised in the definition of $\mathcal{H}_k$, where the time derivative of differential one-forms is used. To get it work one would have to consider also all the derivatives of the variable v. This of course leads to infinitely many variables, but such a construction is a commonly used solution to this problem (in particular in other Kotta's papers). The time derivatives appear also in the proof of Lemma 1, but now also the distributions spanned by vector fields are differentiated with respect to time. This operation was never defined. Looking at calculations one could guess that this is the same as the Lie derivative L_f. Thus the time derivative (of geometric objects) seems to be one of the unnecessary concepts that makes the language of the paper imperfect and makes it difficult for the reader to enjoy reading.

Once the reader straightens up the bends of the language, she/he can appreciate the main results of the paper. One says that two characterizations of realizability are equivalent. Another states that two different algorithms for computing bases of the spaces $\mathcal{H}_k$ give the same results. A detailed example shows almost all the objects that appear in the paper (no time derivatives). The function φ in the example is polynomial and so is the realization. A natural question arises: is it incidental or generic? The realization is global, contrary to locality asserted in Theorems 1 and 2. How often does this happen?

One of the things that the authors forgot to write about is the regularity assumption. The distributions S_k and the codistributions $\mathcal{H}_k$ must have constant dimensions (may be locally) if the statements of Theorems 1 and 2 are to hold. Also observability asserted in these theorems should be understood in the (local) regular sense (i.e. fulfilment of the Hermann–Krener rank condition [1]). If these regularity assumptions are violated, the theory becomes more complicated. For example, if the dimension of the observability codistribution is not constant, one may not be able to reduce the state space in order to obtain an observable realization as this would lead to a state space that is no longer a manifold.

Only realizations of higher-order differential equations were studied in the paper. Another problem, much older and more studied, is the problem of realization of an input–output map. Also for this problem many different approaches have appeared. The paper of B. Jakubczyk [2] studies connections between these approaches and plays a role in similar to the in role the discussed article.

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