Homoclinic splitting. Comment on a paper of Rudnev and Wiggins in print on Physica D.

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Abstract: Assuming the results in the mentioned paper the similar results of later papers do not follow without extra work.

We have recently shown, [GGM], that a trivial computational error in the proof of item c) theorem 3 of a previous paper ([CG], p.71) could be corrected and, in fact, the stated result could even be improved ("the homoclinic splitting is even larger that the estimate in [CG]").

We were giving credit to intervened papers and we were modestly claiming only that our method was different (in fact a remarkable improvement of the one in [CG]) and that it had intrinsic interest. This has not been appreciated by the authors of the paper [RW] and by a colleague of them, Dr. P. Lochak, who expressed harsh and public criticism referring repeatedly to [RW] to support it.

Although we felt that the method we used was new and interesting enough to warrant its publication we were forced to read more carefully reference [RW] to defend our work from what we consider unjustified attacks. In so doing we have developed comments on it. Since the authors of [RW] seem to us to want that the issue be decided by the "scientific community" rather than by normal scientific discussion between us, we write the comments to their work: a full version of their critiques and our unanswered questions can be found in: `http://ipparco.roma1.infn.it`.

After a first reply in which one of us tried to start a detailed discussion, our letters are very short attempts at posing clear technical questions; unfortunately the answers were long and, we feel, never to the point.

We use notations and symbols of [RW] and we do not use our work in the discussion that follows. This is quite easy because the paper [RW] *is based on the previous work* by one of us, [G3]. Of course we do not take into account papers that have become available after our paper. But that would not change the issues as the reason we are forced to go into this unpleasant matter is because our paper risks being rejected by referees believing the claims that our work [GGM] follows from [RW] are right.

The present comment is on the supposed implication [RW] $\to$ [GGM]. Here we assume that theorems 2.1 and 2.2 of the Physica D paper [RW] are correct. Possibly a strong assumption as we shall see on another occasion: but the only one that appears to have become relevant for the future of our paper (surprisingly for us).

Under the assumtions of our paper, we claim that it is not true that without substantial further work theorem 2.1 (and even theorem 2.1 together with theorem 2.2, the main result in [RW]) imply the results of [GGM]. Not even in the "simple" case of the model (8) of [RW].

Using the notations of [RW], a point in phase space is $\vec{\Gamma}=(x,\vec{\phi},y,\vec{I})$ and the homoclinic splitting is the difference in the action coordinates $\vec{I}$ of the points $\tilde{\Gamma}^u(\vec{\alpha})$ and $\tilde{\Gamma}^s(\vec{\alpha})$ which are on the stable and unstable manifolds of an invariant torus and which at time $t=0$ have $x,\vec{\phi}$ coordinates $x(0,\vec{\alpha})=\pi+\xi(\vec{\alpha},\mu),\vec{\phi}(0,\vec{\alpha})=\vec{\alpha}+\vec{\zeta}(\vec{\alpha},\mu)$ where μ is the perturbation parameter; so the homoclinic splitting is $\vec{\Delta}(\vec{\alpha},\mu)=(\Gamma^s(\vec{\alpha}))_I-(\Gamma^u(\vec{\alpha}))_I$.

The $\xi(\vec{\alpha},\mu),\vec{\zeta}(\vec{\alpha},\mu)$, "initial data", were chosen 0 in [G3],[GGM] but in [RW] they are chosen differently. However everything is analytic in μ for μ small.

The difficulty, in comparing the above with [G3], is that $\xi(\vec{\alpha},\mu),\vec{\zeta}(\vec{\alpha},\mu)$ are *not* zero. To compute the quantity called $\vec{\Delta}(\vec{\alpha})$ in [GGM] one needs therefore to compute:

$$S_{t_u(\vec{\alpha})}(\Gamma^u(\vec{\alpha})),\qquad S_{t_s(\vec{\alpha})}(\Gamma^s(\vec{\alpha}))\tag{1}$$

where S_t denotes the flow solution of the equations of motion, and then evaluate the $\vec{I}^{u,s}$ coordinates of the points thus obtained:

$$S_{t_u(\vec{\alpha})}(\Gamma^u(\vec{\alpha}))_I, \qquad S_{t_s(\vec{\alpha})}(\Gamma^s(\vec{\alpha}))_I \tag{2}$$

here we are obliged to introduce $t_u(\vec{\alpha})$, and the notation in Eq. (2), because in [RW] this discussion is missing; it is *defined* as the time necessary for $\Gamma^u(\vec{\alpha})$ to evolve into a datum with $x = \pi$. Likewise is defined $t_s(\vec{\alpha})$.

Of course, since in [RW] $\xi(\vec{\alpha},\mu), \vec{\zeta}(\vec{\alpha},\mu)$ are *not* zero, the angular coordinates $\vec{\phi}$ will be different at such instants. Hence to compute the splitting at a point with angular coordinates $x = \pi$ and $\vec{\phi}$ we must solve an implicit functions problem and find which $\vec{\alpha}_u, \vec{\alpha}_s$ are such that $\Gamma^u(\vec{\alpha}_u)$, $\Gamma^s(\vec{\alpha}_s)$ evolve in points with $x = \pi$ and the *same* $\vec{\alpha}$. Calling $\vec{\alpha}_u(\vec{\alpha})$ and $\vec{\alpha}_s(\vec{\alpha})$ such points we arrive finally at an expression of the splitting studied in [G3] and in [GGM]; calling the latter $\vec{D}(\vec{\alpha})$ we have:

$$\vec{D}(\vec{\alpha}) = S_{t_u(\vec{\alpha}_u(\vec{\alpha}))}(\Gamma^u(\vec{\alpha}_u(\vec{\alpha})))_I - S_{t_s(\vec{\alpha}_s(\vec{\alpha}))}(\Gamma^u(\vec{\alpha}_s(\vec{\alpha})))_I \tag{3}$$

The quantity studied in [RW] is *instead* the much simpler:

$$\vec{\Delta}(\vec{\alpha}) = \Gamma^u(\vec{\alpha})_I - \Gamma^s(\vec{\alpha})_I \tag{4}$$

which coincides with (3) if $t_u(\vec{\alpha}) = 0$, $t_s(\vec{\alpha}) = 0$, $\vec{\alpha}_s(\vec{\alpha}) = \vec{\alpha}$, $\vec{\alpha}_u(\vec{\alpha}) = \vec{\alpha}$. Of course in the formalism of [G3], [GGM] it is $\vec{D}$ that is "much simpler", as conservation of difficulties should imply.

Then one has to compare *the Fourier transforms of (3) and (4)* in $\vec{\alpha}$ and *possibly* infer bounds on the transforms of (3) from those of (4) (which naively do not seem directly related).

Nowhere in the paper [RW] there is any mention of the problems of computing:

$$t_u(\vec{\alpha}), t_s(\vec{\alpha}), \vec{\alpha}_u(\vec{\alpha}), \vec{\alpha}_s(\vec{\alpha}) \tag{5}$$

let alone their Fourier transforms and the necessary compositions. This is a non trivial problem *even* if $t_u(\vec{\alpha}) = t_s(\vec{\alpha}) = t(\vec{\alpha})$ and $\vec{\alpha}_s(\vec{\alpha}) = \vec{\alpha}_u(\vec{\alpha}) = \vec{a}(\vec{\alpha})$ because $t(\vec{\alpha}), \vec{a}(\vec{\alpha})$ are certainly non trivial functions.

We have tried to perform the mentioned Fourier transforms and, even assuming theorems 2.1 and 2.2 of [RW], we do not get anything better than what is *already* implied by (8.1) and (8.2) of ref. [G3]: a paper based on old fashioned pertubation theory. This is nowhere near the new and strong bounds in [GGM].

We asked the authors of [RW] about how our new result in [GGM] could possibly follow from their theorems (as they seem to claim, and as Dr. Lochak seems to say in his apparently vague comments).

We still do not see that [RW] is *any* improvement over [G3], while [GGM] is. Certainly, *if [RW] is right*, in [RW] one proves something interesting. But that is a long way away from a "trivial conjugacy" behind our work! as, *instead*, [RW] and Dr. Lochak appear to believe and even to write. Besides we do not understand why the theorems in [RW] are correct; but this is irrelevant for what concerns our work and its interest; it will be the subject of another comment.

Needless to say the case studied in [GGM] goes much further (because the hypotheses on the frequencies are quite different) and discussing the relation between $\vec{D}$ and $\vec{\Delta}$ is in our opinion much harder to see in the new case; in which we do not even see that the results in [RW] imply that the homoclinic angles are bounded proportionally to an exponential of $-\epsilon^{-1/2}$, *i.e.* in the rapid frequency. The work of [GGM] solves, instead, *completely* the generic system with polynomial perturbation in the class of the considered three time scales problems.

[CG] Chierchia, L., Gallavotti, G.: *Drift and diffusion in phase space, Annales de l' Institut H. Poincare'*, **60**, 1–144, 1994.

[G3] Gallavotti, G.: *Twistless KAM tori, quasi flat homoclinic intersections, and other cancellations in the perturbation series of certain completely integrable hamiltonian systems. A review*, Reviews on Mathematical Physics, **6**, 343– 411, 1994.

[GGM] *Pendulum: separatrix splitting*, in mp_arc@math.utexas.edu, # 97-472; submitted to Communications in Mathematical Physics as: "Separatrix splitting for systems with three time scales"

[RW] Rudnev, M., Wiggins, S.: *Existence of exponentially small sepratrix splittings and homoclinic connections between whiskered tori in weakly hyperbolic near integrable Hamiltonian systems*, preprint in mp_arc@math.utexas.edu # 97-4. In print on Physica D.

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