

First-Principles Theory of the Rate of Magnetic Reconnection in Magnetospheric and Solar Plasmas



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Reviewer comments, first version:

Reviewer #1 (Remarks to the Author: Overall significance):

The manuscript by Liu et al. presents a theoretical analysis of the plasma physics mechanism of magnetic reconnection in collisionless electron-ion plasma. The key result is an estimate (Eqs. 6,9,12) of the reconnection rate, which is the inflow velocity of the reconnecting magnetic field lines, a key parameter for measuring the efficiency of magnetic reconnection, indeed an elusive long-sought fundamental objective. The physical problem is rather complex, as it involves two nested diffusion regions - a larger one for the ions (IDR) and a smaller one for the electrons (EDR) - embedded within an MHD environment, in which different approximations to the non-ideal Ohm's law need to be considered. The analytical calculations are not exact, but they are compared with the results of 2D kinetic numerical simulations. The result appears to be original and the argumentation presented is quite convincing. The two "key points" presented in Sec. 1 para. 3 seem trivial, so one could wonder why wasn't such result obtained much earlier. This work builds upon several previous works by the same group, in particular refs. [32, 54]. These previous works document a continuous sustained effort that led to this manuscript. Overall, I think this is a solid and important work. Whether this should be a Nature Physics (or other Nature journal) article, I'm not sure.

Specific comments:

1. The abstract should state clearly that the main result concerns non-relativistic low-beta electron-ion plasma. It is not clear how general is this result. The short discussion of the pair plasmas uses different arguments, which shows that this is not a general theory of the reconnection rate, as the title suggests. Would the main theory apply also to the relativistic reconnection?

2. In the abstract there is quite a lot of emphasis on the historical debate between Sweet-Parker and Petschek models. In my opinion this would rather belong to the textbooks and to the introduction, unless it can be claimed that this is the last missing piece of the Petschek model. The abstract should make it clear what is new in this work. A formula for the reconnection rate as function of the separatrix

inclination on MHD scales appeared already in ref. [32]. The argument that pressure depletion can be a localization mechanism appeared in ref. [54] in the context of pair plasma. In my understanding the really new results here are:

- demonstrating pressure depletion on IDR scales using the new $\vec{J} \cdot \vec{E}_{\text{Hall}}$ argument (Eq. 4);
- estimating the magnetic field scaling between IDR and EDR (Eq. 6).

3. There are two critical assumptions that need to be highlighted and considered critically: (1) that S_{lope} is constant between EDR and MHD scales, (2) that EDR is in equipartition ($P_{\text{zz}(0)} - P_{\text{zz}(d_e)} \simeq B_{\text{xe}}^2/8\pi$). With accurate kinetic numerical simulations, this should be rather straightforward. Regarding (2), Fig 3e could have an answer, but the d_e scale is too small to see. More importantly, assumption (2) should apply equally to the pair plasma with a single DR, but that appears to be in contradiction with the argument for pressure depletion in pair plasmas presented in lines 392-408. Would the rate R change significantly if the EDR pressure would be reduced by factor 2?

4. That the particle mass ratio is reduced to 400 in the PIC simulation we learn somewhat late in the manuscript. This important information should be mentioned at the beginning of Section II, along with the background plasma beta.

Reviewer #2 (Remarks to the Author: Overall significance):

Report on: “First-Principles Theory of the Rate of Magnetic Reconnection” by Yi-Hsin Liu et al.

This paper faces the problem of magnetic reconnection by investigating locally the physics in the reconnection layer. In particular the authors address the estimation of the reconnection rate following theoretical basic arguments applied to a 2D PIC kinetic simulation of an initial Harris-sheet like configuration.

The paper is well written and addresses a very delicate and important point about local energy conversion and dissipation during a reconnection event. This problem has been and is presently matter of strong debate in the plasma physics community. The discussion presented in the paper is interesting and the results important. On the other hand, the title seems to me not appropriate since I would not call “first principles” the arguments given to obtain the final reconnection estimation. Moreover, all the discussion is based on local balance emerging from a single 2D simulation. Nevertheless, I have no doubts about the interest of this paper which merits to be published even if I have a main concern about the choice of the journal. Indeed, the main result presented in this manuscript impacts a specialized topic rather than something of “broad interest”, i.e. outside the plasma physics reconnection context. On top of that, let me say that a weak point in this respect is the 2D configuration force-equilibrium of

the system when invoking “first principles”. In any case I let the Editors to decide about that.

Before ending, I would like to raise a few “not so major” points before considering the paper for publication whatever the Journal will.

List of questions.

1. The initial equilibrium is not a Vlasov equilibrium but a force equilibrium with, if I have well understood, no guide field. Therefore, in particular nearby the X point where the magnetic field is absent, can one distinguish ballistic effect induced by the initial non-Vlasov equilibrium and those due to the dynamics as discussed by the authors?
2. I would avoid citations such as 1,2 since concerning magnetic reconnection there is a huge amount of literature and textbooks decades before that should be cited when introducing reconnection as a process that converts... Probably better to avoid any citation.
3. Pag.3: I don’t understand what the author mean with “the Hall electric field E_z ballistically accelerates ions...” The Hall field is a fluid term.
4. Pag.3: “forming counter- streaming ion beams and thus this phase space hole...” I don’t understand this sentence. I imagine that there is an electric field that accelerate ions (but why not electrons!) and consequently a return current. Then there must be a kinetic instability at the end forming the phase space holes. If not, the authors should better motivate the presence of such holes.
5. I suggest a simulation of the equilibrium alone to check the possible ballistic effects of the initial equilibrium to prove that there is no contribution to the dynamics observed.
6. Eq. 3: why $dx dy dz$ if the simulation is 2D?
7. How can one argue that all these arguments related to a local numerical analysis of different terms would be valid in 3D?
8. From the EDR discussion one could argue that the dynamics is of EMHD-type. Is that correct? Are EMHD invariants conserved except that in the X point?

In Conclusion, I think that the paper merits publication after clarifying the points above but I let the Editor to decide if the paper is appropriate for the Journals where it has been submitted, namely Nature Physics, Nature Communications, and Communications Physics, or for a more specialized Journal.

Reviewer #2 (Remarks to the Author: Reproducibility):

see the report

Author rebuttal, first version:

[Reviewer #1]: The manuscript by Liu et al. presents a theoretical analysis of the plasma physics mechanism of magnetic reconnection in collisionless electron-ion plasma. The key result is an estimate (Eqs. 6,9,12) of the reconnection rate, which is the inflow velocity of the reconnecting magnetic field lines, a key parameter for measuring the efficiency of magnetic reconnection, indeed an elusive long-sought fundamental objective. The physical problem is rather complex, as it involves two nested diffusion regions - a larger one for the ions (IDR) and a smaller one for the electrons (EDR) - embedded within an MHD environment, in which different approximations to the non-ideal Ohm's law need to be considered. The analytical calculations are not exact, but they are compared with the results of 2D kinetic numerical simulations. The result appears to be original and the argumentation presented is quite convincing. The two "key points" presented in Sec. 1 para. 3 seem trivial, so one could wonder why wasn't such result obtained much earlier. This work builds upon several previous works by the same group, in particular refs. [32, 54]. These previous works document a continuous sustained effort that led to this manuscript. Overall, I think this is a solid and important work. Whether this should be a Nature Physics (or other Nature journal) article, I'm not sure.

We thank the reviewer for the time and effort in reviewing this work, for understanding the difficulty of this problem and for recognizing this work as "original", "quite convincing", "solid" and "important".

About whether the article belongs in *Nature Physics*, the referee commented later in their review that they were unsure if we were claiming to solve the Petschek reconnection problem. We would argue that we have done so in this study. This is why we felt justified in submitting this manuscript to *Nature Physics*. Given this new information in this response, it would be helpful if the reviewer could share with the editors whether they feel this manuscript warrants publication in *Nature Physics*.

We would also like to clarify the reviewer's point on "not being exact". This is certainly a valid point; exact solutions of the type the reviewer is referring to are few and far between in nonlinear, kinetic plasma phenomena. Our analysis uses no empirical results and only relies on first principles, so it represents a major step forward in understanding what localizes the reconnection diffusion region and how that leads to an analytical prediction of the reconnection rate. We believe the reviewer would agree with this statement.

Specific comments:

1. The abstract should state clearly that the main result concerns non-relativistic low-beta electron-ion plasma. It is not clear how general is this result. The short discussion of the pair plasmas uses different arguments, which shows that this is not a general theory of the reconnection rate, as the title suggests. Would the main theory apply also to the relativistic reconnection?

We agree that the main theory primarily focuses on the reconnection rate in non-relativistic low-beta electron-ion plasmas, which are most frequently associated with strong energy release in the magnetosphere and solar atmosphere. For this same reason, such plasma is also the primary concern of the classic Petschek model and Sweet Parker model, both of which fail to provide the full solution for kinetic plasmas presented in our manuscript. Based on the reviewer's suggestion, we have modified our title to "*First-Principles Theory of the Rate of Magnetic Reconnection in Magnetospheric and Solar Plasmas*".

About whether the theory applies to pair plasmas, yes, we do believe that the argument of the energy conversion near the X-line determines whether reconnection has an open (Petschek) or elongated (Sweet-Parker-like) layer does apply to pair plasmas (and resistive plasmas) as well. In fact, the importance of *pressure-depletion* (i.e., one of the two key points) was first realized/observed in extremely relativistic reconnection in pair plasmas last year; i.e., Ref. [54].

While the theory does apply to pair plasmas, a first-principles analytical model for the reconnection rate in such a system was not pursued here and would require future work. It will need to involve modeling kinetic heating in a pair plasma current sheet (lines 404-420 in the revision), which is different from the case dominated by the Hall effect because the IDR is the same as the EDR in pair plasmas. One of our students at Dartmouth is currently working on this project under an NSF grant.

2. In the abstract there is quite a lot of emphasis on the historical debate between Sweet-Parker and Petschek models. In my opinion this would rather belong to the textbooks and to the introduction, unless it can be claimed that this is the last missing piece of the Petschek model. The abstract should make it clear what is new in this work. A formula for the reconnection rate as function of the separatrix inclination on MHD scales appeared already in ref. [32]. The argument that pressure depletion can be a localization mechanism appeared in ref. [54] in the context of pair plasma. In my understanding the really new results here are:

- demonstrating pressure depletion on IDR scales using the new $\vec{J} \cdot \vec{E}_{\text{Hall}}$ argument (Eq. 4);
- estimating the magnetic field scaling between IDR and EDR (Eq. 6).

Yes, we do believe this is the last missing piece of the Petschek model, as it captures the physics of the localization mechanism needed for fast reconnection in kinetic electron-ion plasmas. The reviewer understands the relation between Refs. [32], [54] and this new work correctly. Refs. [32] and [54] had laid out some important, general groundwork for this study, but they had not resulted in a self-consistent solution to this longstanding problem. Specifically, Ref. [32] does not determine the separatrix opening angle, leaving it as a free parameter, which was thought to be maximized in naturally occurring systems. Ref. [54] recognized the importance of pressure depletion in localizing the diffusion region, but being a study about pair plasmas, it did not elucidate the key role that the Hall electromagnetic fields play in depleting the pressure in collisionless reconnection in electron-ion plasmas. Through these new insights/calculations presented in this work (i.e., Eqs. 4 & 6 & 7, parametrized using carefully designed Figs. 4 (a) & (b) & (c), respectively), we finally are able to derive a full first-principles/self-consistent solution to predicting the magnetic reconnection rate in electron-ion plasmas.

p.s., Refs. [32] and [54] referred by the reviewer (and used in this response) correspond to Refs. [31] and [54] in the revised version.

The referee is correct that the abstract was written poorly, so we have completely rewritten it from scratch as follows:

The rate of magnetic reconnection is of the utmost importance in a variety of processes because it controls, for example, the rate energy is released in solar flares, the speed of the Dungey convection cycle in Earth's magnetosphere, and the energy release rate in harmful geomagnetic substorms. It is known from numerical simulations and satellite observations that the rate is

approximately 0.1 in normalized units, but despite years of effort, a full theoretical prediction has not been obtained. Here, we present a first-principles theory for the reconnection rate in non-relativistic electron-ion collisionless plasmas, and show that the same prediction explains why Sweet-Parker reconnection is considerably slower. The key consideration of this analysis is the pressure at the reconnection site (i.e., the x-line). We show that the Hall electromagnetic fields in antiparallel reconnection cause an energy void, equivalently a pressure depletion, at the x-line, so the reconnection exhaust opens out, enabling the fast rate of 0.1. If the energy can reach the x-line to replenish the pressure, the exhaust does not open out. In addition to heliospheric applications, these results are expected to impact reconnection studies in planetary magnetospheres, magnetically confined fusion devices, and astrophysical plasmas.

3. There are two critical assumptions that need to be highlighted and considered critically: (1) that S_{lope} is constant between EDR and MHD scales, (2) that EDR is in equipartition ($P_{\text{zz}}(0) - P_{\text{zz}}(d_e) \simeq B_{xe}^2/8\pi$). With accurate kinetic numerical simulations, this should be rather straightforward. Regarding (2), Fig 3e could have an answer, but the d_e scale is too small to see. More importantly, assumption (2) should apply equally to the pair plasma with a single DR, but that appears to be in contradiction with the argument for pressure depletion in pair plasmas presented in lines 392-408. Would the rate R change significantly if the EDR pressure would be reduced by factor 2?

Thank you for these very important questions. The figure below plots the magnetic field lines on top of B_z from the MHD scale down to the EDR. The aspect ratio of the plot is realistic, i.e., 1 unit in the horizontal direction is the same size as 1 unit in the vertical direction. The relevant slope is captured by the field line that approaches closest to the reconnection x-line. The short dashed lines plotted in the EDR, IDR, and MHD regions have the same slope, and they represent a reasonable approximation to the field line slopes at the different regions. The reason behind this similar slope is that magnetic field lines tend to straighten themselves out (if they can). Even if the EDR slope introduces a factor of 2 in Eq. (5), the scaling in Eq. (6) still holds, and B_{xe}/B_{xi} remains small in the large mass ratio limit—this smallness is the key feature used in our theory.

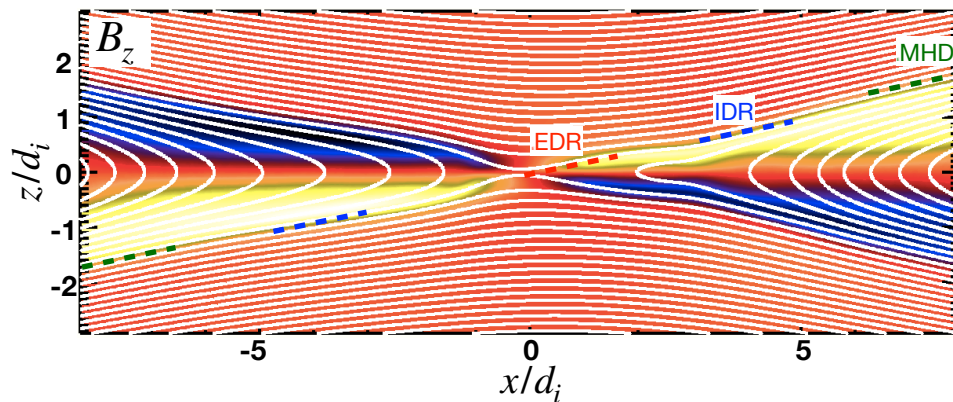


Figure: The field line closest to the x-line shows the relevant slopes at the EDR, IDR and MHD region.

The figure below plots the z-cuts of pressures near the EDR, marked by the transparent red. Note that both the horizontal and vertical scales are much smaller than in Fig. 3(e) of the paper. The magnetic pressure (in black) at the EDR boundary reaches a value ~ 0.024 (which is very close to the prediction by Eq. (6) indicated by the horizontal magenta line). The total plasma pressure difference (in green) between the peak value and the value at the EDR boundary is slightly smaller than the magnetic pressure difference, indicating that the electron pressure mostly does balance the magnetic pressure as we used in our model, though there is a small degree of imbalance.

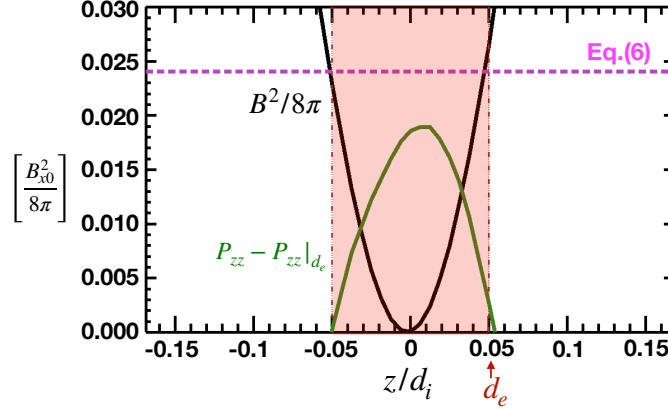


Figure: The pressure balance close to the EDR.

In electron-ion plasmas, knowing the exact degree of pressure-balance within the EDR is not critical as long as the associated magnetic pressure is small. This smallness is consistent with the scaling in Eq. (6), the magnetic pressure $B_{xe}^2/8\pi$ at the electron-scale is 0.048 times the magnetic pressure $B_{xi}^2/8\pi$ at the ion-scale (for $m_i/m_e = 400$ in this run). The change in rate R is negligible even if the EDR pressure is completely depleted. This motivates that the treatment in the manuscript is valid despite this small imbalance.

The reviewer points out an important aspect of pair plasmas, that was only briefly hinted in lines 336-339 (in the original revision). In pair plasmas ($m_i/m_e = 1$), the EDR is the same as the IDR, thus $B_{xe}^2/8\pi = B_{xi}^2/8\pi$. We do believe that a pressure reduction by a factor of 2 would localize the EDR and therefore explain the fast rate in this situation. A plausible mechanism for having a depleted pressure in pair plasmas is the inefficiency of kinetic heating (i.e., no ballistic acceleration to increase ΔP_{zz}) and the energy competition with bulk flow kinetic energy associated with the current carriers. These were discussed in lines 404-420 (in the revision), as also noted by the reviewer.

We have rewritten the discussion at lines 344-351 (in the revision) to explicitly acknowledge this pressure-imbalance within EDR and note its importance in pair plasmas.

4. That the particle mass ratio is reduced to 400 in the PIC simulation we learn somewhat late in the manuscript. This important information should be mentioned at the beginning of Section II, along with the background plasma beta.

We have copied this information from the METHODS section to line 101 (in the revision), at the beginning of section II.

[Reviewer #2]: Report on: “First-Principles Theory of the Rate of Magnetic Reconnection” by Yi-Hsin Liu et al. This paper faces the problem of magnetic reconnection by investigating locally the physics in the reconnection layer. In particular the authors address the estimation of the reconnection rate following theoretical basic arguments applied to a 2D PIC kinetic simulation of an initial Harris-sheet like configuration.

The paper is well written and addresses a very delicate and important point about local energy conversion and dissipation during a reconnection event. This problem has been and is presently matter of strong debate in the plasma physics community. The discussion presented in the paper is interesting and the results important. On the other hand, the title seems to me not appropriate since I would not call “first principles” the arguments given to obtain the final reconnection estimation. Moreover, all the discussion is based on local balance emerging from a single 2D simulation. Nevertheless, I have no doubts about the interest of this paper which merits to be published even if I have a main concern about the choice of the journal. Indeed, the main result presented in this manuscript impacts a specialized topic rather than something of “broad interest”, i.e. outside the plasma physics reconnection context. On top of that, let me say that a weak point in this respect is the 2D configuration force-equilibrium of the system when invoking “first principles”. In any case I let the Editors to decide about that.

We thank the reviewer for the time and effort in reviewing this work!

We used the phrase “First-Principles” here to refer to the fact that our theory does not rely on any empirical inputs taken/measured from the PIC simulations. In other words, the complete theory derivation starting from line 247 to line 373 (in the revision) can be a stand-alone theoretical piece. The simulation results in Fig. 1 and Fig. 3 are included in this paper to show key features that confirm our idea and theoretical prediction, which also help communicate the ideas to a more general audience. It is important to note that such a nonlinear solution of collisionless reconnection did not exist before. We have clarified the meaning of “first-principles” in lines 71-73 (in the revision).

On the referee’s point that the discussion is based on a single 2D simulation, we agree that this is true of the present manuscript, but it does not take into account the numerous simulations in previous works by this group and others that the theory explains. If the referee has reason to believe that the predictions here would not apply to the most interesting plasmas relevant to the strong energy release in the magnetosphere and solar atmosphere, we invite them to be specific about which parameters they think are important, and we would be happy to address them.

About whether this manuscript is a specialized topic or of broad interest, we disagree that it is too specialized to be of broad interest. Magnetic reconnection is arguably one of the most important energy conversion and transport processes in space and astrophysical plasmas. Among other effects, reconnection enables the entry of solar wind plasmas into Earth’s magnetosphere and the dissipation of magnetic energy. In the solar atmosphere, magnetic reconnection allows topological changes in large-scale magnetic fields, which release magnetic energy (in the form of solar flares) and drive coronal mass ejections. Magnetic reconnection has also been suggested to play a key role in heating the solar corona and is known to cause sawtooth crashes in fusion devices. For astrophysical systems, the last decade has seen a dramatic surge of interest in the potential role of magnetic reconnection in stellar flares, super-flares in the Crab Nebula, accretion disks and jets emanating from rotating black holes. For the

many researchers studying these phenomena that are not reconnection experts, the main piece of input they seek from reconnection theory is typically the rate, as it controls the time scales of solar flares, the speed of the Dungey convection cycle, etc. This paper, for the first time, produces a self-consistent calculation of the rate of reconnection. We, therefore, have no doubt that providing the first-ever theoretical solution to the reconnection rate (i.e., how efficiently it processes magnetic flux) in kinetic plasmas will be of broad interest, and we sincerely ask the referee to reconsider their position on this.

Finally, since the topic of the initial equilibrium is brought up below, we will respond to it there. In short, we believe the referee has misunderstood our initial conditions.

Before ending, I would like to raise a few “not so major” points before considering the paper for publication whatever the Journal will.

List of questions.

1. The initial equilibrium is not a Vlasov equilibrium but a force equilibrium with, if I have well understood, no guide field. Therefore, in particular nearby the X point where the magnetic field is absent, can one distinguish ballistic effect induced by the initial non-Vlasov equilibrium and those due to the dynamics as discussed by the authors?

There apparently has been some confusion - we are employing the standard Harris sheet solution [Harris, 1962], which is a Vlasov equilibrium. We have now clarified this point in the METHODS section.

2. I would avoid citations such as 1,2 since concerning magnetic reconnection there is a huge amount of literature and textbooks decades before that should be cited when introducing reconnection as a process that converts... Probably better to avoid any citation.

We have taken the reviewer’s suggestion to avoid citing references here.

3. Pag.3: I don’t understand what the authors mean with “the Hall electric field E_z ballistically accelerates ions...” The Hall field is a fluid term.

The referee is correct that the Hall field can be described purely in the fluid model, but that does not mean that in a fully kinetic system that the electric field does not accelerate particles. At length scales where the Hall electric field is significant, the ions are not frozen into the plasma and can therefore be accelerated by the Hall electric field. The Hall electric field E_z shown in Figure 1(b) accelerates ions through the Lorentz force, $\mathbf{F} = q(\mathbf{E} + \mathbf{V} \times \mathbf{B})$. It is “ballistic” for inflowing ions because ions are already demagnetized within the IDR, and they see Hall electric field E_z as a DC field. This explains the phase-space structure in Fig. 3(d).

4. Pag.3: “forming counter- streaming ion beams and thus this phase space hole...” I don’t understand this sentence. I imagine that there is an electric field that accelerate ions (but why not electrons!) and consequently a return current. Then there must be a kinetic instability at the end forming the phase space holes. If not, the authors should better motivate the presence of such holes.

The ballistically accelerated inflowing ions from the two inflow sides are shown by the population of negative V_z at $z > 0$ and the population of positive V_z at $z < 0$ in Fig. 3(d). They gain speed

and pass through each other (in the absence of collisions) at $z=0$, leaving no particles near $V_z \simeq 0$ at $z \simeq 0$, which explains the phase space hole formation. This phase space hole does not arise from a kinetic instability other than reconnection itself. The first paper that discusses this ballistic ion acceleration in in-situ Cluster observation is Wygant et al., JGR 110, A09206 (2005); thus, we only provided a short description.

As to the inflowing electrons, they indeed are also affected by the Hall electric field. Because of their opposite charge, they lose energy through this Hall E_z because the work done on them is $-eV_{ez} \cdot E_z < 0$. More discussion can be found between lines 247-255 (in the revision).

5. I suggest a simulation of the equilibrium alone to check the possible ballistic effects of the initial equilibrium to prove that there is no contribution to the dynamics observed.

As discussed in the response to question 1, the reviewer was mistaken about the initial condition, which is a Vlasov Harris sheet equilibrium. The Harris equilibrium contains no electric fields. Consequently, there is no possibility for there to be ballistic particle acceleration in the equilibrium. In fact, the initial distribution functions, shown in Fig. 3(a) and 3(b), do not reveal any ballistic acceleration, and the distributions remain similar until reconnection takes place. Therefore, there would be nothing gained by performing the simulation suggested by the referee.

6. Eq. 3: why $dx dy dz$ if the simulation is 2D?

As with numerous analyses going back to the textbook derivation of the Sweet-Parker model, we introduce “ dy ” for the out-of-plane (translational invariant) direction primarily because the meaning is clearer when one uses 3D terms like “Volume” & “Surface”...etc to discuss those Gaussian integrals formulated in this paper. This length dy (or ℓ_y) cancels out in the derivation, so it has no impact on the reported results.

7. How can one argue that all these arguments related to a local numerical analysis of different terms would be valid in 3D?

Thank you for making this point, as we were not clear enough on this in the manuscript. In this study, we are focusing on a 2D analysis, since the age-old question of predicting the reconnection rate is typically treated in 2D. However, it is important to consider whether the results here would still apply to 3D. We argue that the result here does apply to 3D systems, because the Hall effect arises whenever the current sheet thins down to the ion kinetic scale. Thus even in a thin 3D current sheet, the argument based on the two key points laid out in lines 52-73 (in the revision) still holds. Large-scale 3D PIC simulations in fact suggest that broader turbulent current sheets can collapse to thin reconnecting layers at the kinetic scale [F. Guo et al., ApJ, 919, 111 (2021)]

It also bears mentioning that the reconnection process is often quasi-2D in nature. This has been ascertained very clearly from an abundance of data from the MMS mission, which has unprecedented spatial and temporal resolutions. In particular, 2D simulations have done an excellent job reproducing detailed dynamics observed in reconnection EDR events [e.g., Torbert et al., Science 362, 1391 (2018); R. Nakamura et al., JGR 124, 1173 (2019); T. Nakamura et al., JGR 123, 9150 (2018)]. Moreover, it has been known since Parker (1973, ApJ, 180, 247) that a reconnection rate of the order of 0.1 would explain the rates inferred from observations of solar

flares and geomagnetic substorms, and this provides excellent evidence that the 0.1 reconnection rates found in two-dimensional simulations, as explained in the present manuscript, are relevant to three-dimensional systems in Nature.

We have added a new paragraph between lines 509-527 in the revision to discuss these points.

8. From the EDR discussion one could argue that the dynamics is of EMHD-type. Is that correct? Are EMHD invariants conserved except that in the X point?

No, EMHD cannot describe the EDR itself, as it is a fluid model, and there are manifestly kinetic effects taking place in the EDR. EMHD may be a valid approximation between the EDR and IDR regions. However, note that the ballistic ion acceleration and phase space hole in kinetic plasmas would not be captured by EMHD. It would be outside of the scope of the present study to investigate whether the reduced model EMHD has any bearing on the full kinetic physics studied here, but we agree that it could be an interesting limit for follow-up work.

In Conclusion, I think that the paper merits publication after clarifying the points above but I let the Editor to decide if the paper is appropriate for the Journals where it has been submitted, namely Nature Physics, Nature Communications, and Communications Physic, or for a more specialized Journal.

Reviewer comments, second version:

Reviewer #1 (Remarks to the Author: Overall significance):

The Authors' response to the first round of reviews and the revised manuscript address most of my concerns. I received a decision made by the Editors to consider this manuscript for publication in the Communications Physics - it is a good idea. I only have two relatively minor comments, otherwise I find this manuscript to be fine.

1. Regarding the pressure balance across the EDR in the electron-ion case: the Authors maintain that the magnetic pressure is balanced by the gas pressure (line 344), although their PIC simulations show imbalance at the level of 20%. I would like to suggest that this imbalance may be significant, otherwise how is the EDR localized within the IDR? The Authors demonstrated that the IDR can be localized because of the $(2/3)$ factor in Eq. (4). If the pressures would be balanced exactly across the EDR, one could try to apply the localization argument illustrated in Figure 2 to suggest an alternative situation with an unlocalized Sweet-Parker-like EDR inside a localized Hall-type IDR. Should the purity of first principles be maintained, such an additional $\sim(4/5)$ factor would have to be justified analytically.

2. Regarding the Authors' response to Reviewer #2, Comment 7 on whether these results apply to reconnection in 3D, I find it strange to invoke in this context the prediction of Parker that reconnection rate should be of order 0.1 (line 521). I find this argument invalid, especially because here we are concerned with physical details, the first principles, rather than the most general outcomes that may coincide between 2D and 3D. The MMS argument is a good one, and there should be many more 3D kinetic simulation results that could be cited here.

Reviewer #1 (Remarks to the Author: Impact):

I support the decision by the Editors to invite this manuscript to be resubmitted to the Communications Physics.

Reviewer #2 (Remarks to the Author: Overall significance):

Report on: "First-Principles Theory of the Rate of Magnetic Reconnection" by Yi-Hsin Liu et al.

The revised paper has addressed all my concerns. I still have minor comments to add but I don't need to see the paper again and I recommend it for publication.

Minor comments

1. Title, First principles. It is now specified what the authors mean even if I would not use that word but just “A new theory... or On the theory...” which by definition cannot be based on a single measurement. If the word theory is a somewhat abused, as correctly stated by the authors, this cannot be an argument. A principle is not related to what existed before! Nevertheless, no problems if the authors want to maintain the title as it is.

2. About 2D I know well and appreciated the excellent and very important contribution of the group involved in the present work. Furthermore, I’m not saying that the theory does not apply to 3D. I’m just saying that the authors presented a 2D simulation to introduce the theory and defining possible 3D parameter range where the theory would apply, I would say, is something left for future work. In other words, this point should be addressed by the authors not by the Referees! I am aware the 2D is a basic step, a fundamental framework to study reconnection, but still it is 2D and a step forward 3D is not obvious, at least in my opinion.

3. About “specialized” I was just referring to the kind of approach. No doubts reconnection is of broad interest.

4. About Harris equilibrium, if so, I obviously agree with the authors (since by construction there is no electric field). I strongly appreciated the Harris paper citation avoiding any misunderstanding as for the Referee at first round!

5. About Hall ballistic what I tried to say is that the Hall term accelerates particles which then behave ballistically. In other words “..the E field accelerated particles “ would be preferable if the authors want.

Author rebuttal, second version:

[Reviewer 1]: The Authors' response to the first round of reviews and the revised manuscript address most of my concerns. I received a decision made by the Editors to consider this manuscript for publication in the Communications Physics - it is a good idea. I only have two relatively minor comments, otherwise I find this manuscript to be fine.

1. Regarding the pressure balance across the EDR in the electron-ion case: the Authors maintain that the magnetic pressure is balanced by the gas pressure (line 344), although their PIC simulations show imbalance at the level of 20%. I would like to suggest that this imbalance may be significant, otherwise how is the EDR localized within the IDR? The Authors demonstrated that the IDR can be localized because of the $(2/3)$ factor in Eq. (4). If the pressures would be balanced exactly across the EDR, one could try to apply the localization argument illustrated in Figure 2 to suggest an alternative situation with an unlocalized Sweet-Parker-like EDR inside a localized Hall-type IDR. Should the purity of first principles be maintained, such an additional $\sim(4/5)$ factor would have to be justified analytically.

We thank the reviewer for being persistent on this subtle point. We have reworked the theory to avoid assuming a pressure-balanced EDR. The conclusion on the dominance of the Hall localization remains the same because of the smallness of B_{xe}^2/B_{xi}^2 . The updated calculation is highlighted in between lines 344-356, which is more general for a finite B_{xe}^2/B_{xi}^2 value. The previous derivation between lines 344-356 is right only in the $B_{xe}^2/B_{xi}^2 \rightarrow 0$ limit. This 20% pressure imbalance within the EDR can now be explained by the new Eq. (10).

Nicely, the model is now more accurate and will avoid the potential misuse of Fig. 2(b) to infer a Sweet-Parker-like EDR within a localized IDR, as indicated by the reviewer. Thanks! Most importantly, the conclusion remains the same that through cross-scale coupling between the EDR, IDR and the upstream MHD region, the fast reconnection rate of order $\mathcal{O}(0.1)$ is derived (Eqs. (13),(9),(6)) from first principles for the first time.

2. Regarding the Authors' response to Reviewer #2, Comment 7 on whether these results apply to reconnection in 3D, I find it strange to invoke in this context the prediction of Parker that reconnection rate should be of order 0.1 (line 521). I find this argument invalid, especially because here we are concerned with physical details, the first principles, rather than the most general outcomes that may coincide between 2D and 3D. The MMS argument is a good one, and there should be many more 3D kinetic simulation results that could be cited here.

We removed the discussion on Parker's 1973 estimation and cited a few more 3D kinetic simulations in the end of the discussion section.

[Reviewer 2]: The revised paper has addressed all my concerns. I still have minor comments to add but I don't need to see the paper again and I recommend it for publication.

Minor comments

1. Title, First principles. It is now specified what the authors mean even if I would not use that world but just "A new theory... or On the theory..." which by definition cannot be based on a single measurement. If the word theory is a somewhat abused, as correctly stated by the authors, this cannot be an argument. A principle is not related to what existed before! Nevertheless, no problems if the authors want to maintain the title as it is.

We would like to keep the same title, since we are not aware of any complete theory that can make the prediction of nonlinear reconnection rate in collisionless plasmas.

2. About 2D I know well and appreciated the excellent and very important contribution of the group involved in the present work. Furthermore, I'm not saying that the theory does not apply to 3D. I'm just saying that the authors presented a 2D simulation to introduce the theory and defining possible 3D parameter range where the theory would apply, I would say, is something left for future work. In other words, this point should be addressed by the authors not by the Referees! I am aware the 2D is a basic step, a fundamental framework to study reconnection, but still it is 2D and a step forward 3D is not obvious, at least in my opinion.

In the end of the discussion section, we now explicitly state that the 3D reconnection process remains an interesting open question for future exploration.

3. About "specialized" I was just referring to the kind of approach. No doubts reconnection is of broad interest.

Okay.

4. About Harris equilibrium, if so, I obviously agree with the authors (since by construction there is no electric field). I strongly appreciated the Harris paper citation avoiding any misunderstanding as for the Referee at first round!

Agree.

5. About Hall ballistic what I tried to say is that the Hall term accelerates particles which then behave ballistically. In other words "...the E field accelerated particles..." would be preferable if the authors want.

We added more discussion to make this point clearer in line 197.

"Note that the acceleration is ``ballistic" for inflowing ions because ions are already demagnetized within the IDR and they see the Hall E_z as a DC field."