Dear editor,

We like to express our appreciation to the reviewers for their comments.

We are resubmitting the revised version of the paper number **JR17-1447.** We have studied the comments of the reviewer carefully, and have changed the text according to the comments they have listed. Below we refer to each of the reviewer's comments / suggestions.

The text of the paper was resubmitted in two corrected versions: with and without underlining of the modifications. We have also submitted additional figures: revised Fig. 3(b), and Fig. 3(d).

Editor's Comments:

Reviewer Comments:
Reviewer #1 Evaluations:
Does the manuscript present original and timely results that significantly advance the knowledge in applied physics: No
Does the manuscript report on convincing and rigorous data methods and analysis: Yes
Is the manuscript clearly written in correct English well organized and free from ambiguities: No
Is the title descriptive of the contents concise interesting and free of acronyms: Yes
Does the abstract adequately and clearly describe the contents (problem approach findings) of the paper: Yes
Are the figures in the manuscript necessary adequate well presented and clearly labeled: No
Is the reference list appropriate: Yes

Reviewer #1 (REMARKS to AUTHOR(s)):

The authors describe a technique to extract room temperature parameters of Schottky diodes when barrier height is voltage dependent. This problem (characterization of Schottky barriers) has been exhaustively studied for the last six decades and will continue due to the importance of this issue to devices developing.
The manuscript describes a (new?) method for current-voltage characteristics analysis based on the differential slope of the experimental I-V curves. Some references cited in the manuscript (Refs. 17-23) already give a route for a similar analysis: using the early text book models (Physics of Semiconductors Devices by S. M. Sze or the brilliant E. H. Rhoderick's Metal Semiconductor Contacts) and the differential slope of the experimental I-V curves one obtain the results presented here. In this sense, it appears to me that the paper does not present original results but instead only a different way (some parts already published, as cited in the manuscript) to extract out Schottky parameters.

Reply: To the best of our knowledge, there is no published paper which describes a technique that can discriminate between different thermionic emission mechanisms and that enables the extraction of parameters from experimental I-V characteristics measured at **room temperature**. We have revised the Introduction and added a paragraph which surveys the different methods of parameter extraction in Schottky diodes. These methods analyze the forward bias I-V characteristics. In contrast, our technique employs the reverse bias I-V curve. This enables first to discriminate the main mechanisms of thermionic emission (among several possibilities). Following this, it is possible to extract, in a second stage, the zero-bias barrier height, its variation with voltage, and subsequently the behavior of ideality factor with applied bias. We demonstrate by the I-V characteristic of a single Schottky diode measured in both bias polarities, simultaneous extraction of the real bias dependent parameters and the series resistance.

To conclude, we believe strongly that the paper is an important addition to the literature and is not a variation of known techniques.

Specific comments:

Comment 1. The authors claim that "The method was verified for MS, MSM and MISM structures with symmetric or asymmetric electrodes by measurement of the current voltage characteristics (I-V) at room temperature under different illumination regimes" but being a very studied subject why authors did not present a comparison with published or other "conventional" models?

Reply: See reply to the main comment (above).

Comment 2. From eq. (1) to eq. (2) there is a gap: eq. (1) describes only thermionic mechanism while eq. (2) present a combination of the thermionic and diffusion theory! Js1 or Js2 have not to account diffusion theory to be correct or to be used in eq. (1).

Reply: The corresponding corrections were done.

Comment 3. Authors wrote: "The Schottky barrier height is reduced in the dark due to both a variation of image force potential and a dipole layer, which results from an interfacial layer with surface trap states and or fixed charges at the MS boundary [1, 15, 16]. The barrier height reduces further under illumination [10]". The barrier reduces in dark conditions and reduces further under illumination. When does it increase?

Reply: In this paper, we discuss reverse bias I-V characteristics for the system whose current flow mechanism can be characterized by thermionic emission. Therefore, the answer is trivial; barrier height rise takes place only in the forward bias regime (see references: 1, 2, 9, 27, 28 and equation (3) in the paper).

Comment 4. Authors wrote: "...transmission coefficient of the barrier to be smaller than unity [15, 16]).....". Is that reasonable for the diodes studied here?

Reply: In the thermionic emission case with and without the consideration of image force potential, the transmission coefficient is usually assumed to be about 1, which may reduce from this ideal value under large electric fields (>> 1 MV/cm) due to auto electron emission phenomenon [8, 38]. The actual probability of carrier penetration through the barrier is a complex issue and is beyond of the scope of the present paper. However, we note that changing the transmission coefficient from 0.1 to 1, results in an error of less than 12 %, in the determination of the zero bias barrier height. Note that the value of Θ does not influence the experimental estimation of $\Delta\boldsymbol{\varphi}$. The text, clarifying the reviewer's comment is added in the revised manuscript.

Comment 5. Experimental part:
Comment 5a. A commercial Schottky diode was used: which one?
Comment 5b. A figure of the devices built for this work should be presented.

Reply to 5a: The Schottky diode: MA40147-213 was used in measurement.

Reply to 5b: The schematic configuration of the structures studied in present paper is given in paper [3].
A sentence was added in text.

Comment 6. When describing the results, authors wrote " The perfect coincidence testifies to the strength of the proposed method, which is simpler than the complicated current versus temperature technique of extraction, zero bias barrier height of single Schottky diodes [1]. " Why complicated? It has been used since 60´s without problems!
Also, the coincidence claimed by the authors is related to two equations described in the manuscript. Why there is not a comparison with published results?

Reply: First of all, we are not aiming for the extraction of zero-bias barrier height only; second, the temperature measurement takes much more time and is cumbersome as it requires a vacuum and a cooling system. Moreover, our method allows the extraction of the parameters from a single I-V measurement in dark and at room temperature. Finally, a measurement at room temperature allows for many types of materials (solid, liquid, organic, nonorganic). The proposed method also provides the dependence of the barrier height on applied bias and other parameters, unlike the temperature measurements.

We are unsure about which two specific equations the reviewer is referring to. A comparison of the parameters extracted by different methods (Werner method [18] and $\boldsymbol{\alpha}$-V method [22]) using forward bias characteristics has been reported in ref. [22].

Comment 7. Authors wrote: "The reason is that interfacial states would lead to an exponential dependence of the reverse saturation current on applied voltage and therefore a linear increase of α with applied voltage, as predicted by equations (2) or (8) taking into consideration (3) and (4)." What distribution shape was authors using to describe the density of states? Constant density? Gaussian distribution? Please check Barret C. Thesis University of Paris-Sud, Orsay (1981); Barret C and Vapaille A, Solid State Electron. 18, 25 (1975).

Reply: We were unable to find the recommended thesis online. However, we have revised the text to note that the models which we use (see references 1, 26-28) is mono energetic and with uniform distribution in energy within the bandgap with constant density.

Comments: Minor points:
1. English should be improved: some phrases are confuse.

Reply: The **t**ext was revised.

2. In page 5, after eq. (7) authors wrote: "...respectively, for n and p type semiconductors.", but only in eq. (7) n and p type semiconductors are distinguished.

Reply: We have revised the text accordingly.

3. In page 5, authors give a description of a lot of constants. These constants are well known from literature and a reference is good enough to address the reader to their meanings. After all, authors do not work out these constants all through the paper.

Reply: We believe it is a common and useful practice to define symbols/constants used in a paper. Therefore, we would like to retain the original text defining the symbols/constants in our equations, for clarity.

4. The authors wrote "The technique we propose does not require to resort to complicated graphical differentiation technique [24, 25] or to technique, which are only valid under limited conditions [26]." They should re-phrase this.

Reply: We rephrased this sentence (see page 7).

Reviewer #2 Evaluations:
Does the manuscript present original and timely results that significantly advance the knowledge in applied physics: Yes
Does the manuscript report on convincing and rigorous data methods and analysis: Yes
Is the manuscript clearly written in correct English well organized and free from ambiguities: Yes
Is the title descriptive of the contents concise interesting and free of acronyms: Yes
Does the abstract adequately and clearly describe the contents (problem approach findings) of the paper: Yes
Are the figures in the manuscript necessary adequate well-presented and clearly labeled: Yes
Is the reference list appropriate: Yes

Reviewer #2 (REMARKS to AUTHOR(s)):

The paper is devoted to the new method of the current mechanism determination and barrier height extraction. Authors are bringing into community the method, which is based on using of the differential slope of the reverse current-voltage characteristic. Conclusions and abstract reflect main results of the article. References are appropriate. The work could be interesting for JAP. I have minor comments:

Comment 1. The term "kT/q" in equations, located in the last column of Table 1 (second, third and fourth rows) must be replaced by "kT" (or "qΔφ, eV" must be replaced by "Δφ, V" in the first row).

Reply: The correction was done.

Comment 2. At many real MS interfaces, the ideality factor is included in the description of the junction current. The greater-than-unity ideality factor (and bias dependence of a SBH) can be caused by not only image force and interface states but also thermionic field emission, generation-recombination, SBH inhomogeneity. The expected behavior of the differential slope of the I-V characteristics in the last cases would be interesting.

Reply: The reviewer is correct. In the present paper, we have concentrated on three specific and common mechanisms, which frequently take place in MSM or MIS structures, and could be verified experimentally. The reviewer's suggestion is very interesting, and may be done in the future.

Comment 3. "V" must be replaced by "Vr" in the Eq.(4), Fig.3(c).

Reply: The corrections were done.

Comment 4. The reason for selection of illumination wavelength (365 nm) should be discussed.

Reply: This wavelength was not chosen specially. We have randomly taken the data from I-V characteristics of photodetectors measured in a wide wavelength range from 365 nm to 880 nm.

In this wavelength range, the spectral responsivity of structures changes, while the current limitation mechanism can be identical.

Comment 5. Possibly, the semi-log scale is more relevant for the Fig.3(b).

Reply: We don't think so, as logarithmic presentation usually smoots the extracted power exponent. In order to clearly show the actual behavior of power exponent in this regime we have added an insert plotted in an enlarged ordinate scale.

Comment 6. If the dependence on Fig.3(c) is described by the Eq.(6) then it is possible to determinate the Vbi and Nd values. So, the comparison of the extracted donor concentration and built-in voltage with known sample parameters can be important part of the experimental verification of the introduced method.

Reply: The reviewer is correct, but it requires solving the transcendent equation (6), which is difficult. Note, that V_{bi} can be extracted from the experimental value of $\Delta\varphi_0$ if N_D is known. The deviation of extracted V_{bi} from simulated I-V characteristics at known N_D was about of 0.2 % from its fitting value.

Comment 7. It is claimed (page 10, first and second paragraphs from the bottom) that "ideality factor ...in the present case is 1.12... The small deviation of the ideality factor from a value of unity indicates that the current flow by thermionic emission is disturbed by the effect of the voltage dependent image force potential".
However n=1.12 is too much for image force influence. In this case
n=1+0.25*(q^3 Nd / 8 Pi^2 (eo es Vbi)^3)^0.25. One can calculate, that for n=1.12 and silicon Nd=1.1e27 (Vbi)^3 1/m^3. Therefore, semiconductor is degenerate, or diffusion potential is too small, or ideality factor value is not determined by image force. The last version is most probable. It is important to report some details about this situation in the text.

Reply: An ideality factor value of about 1.12 was estimated from the maximum of alpha-V plot in accordance with method described in Ref.22, which assumes the Schottky barrier height to be independent of the applied voltage. Therefore, the extracted ideality factor value is an approximate one. The real voltage dependence of the ideality factor, in accordance with [1], is expressed as: $n \approx 1/(1 - {}^{d\Delta\varphi}/_{dV})$. A correct curve depicting the dependence of the ideality factor on applied bias is shown in the Fig. 3(d), which takes the voltage-dependence of the extracted image force potential into account (see Fig. 3(c)). It is seen that *n* reduces from 1.027 at $V > 0.05\,V$ to 1.005 at $V = 0.4\,V$. Therefore, the small deviation of the ideality factor from 1 can certainly be attributed to the influence of an image force potential. A similar procedure can be used to establish the behavior of the ideality factor with applied bias in the common case (see Eq. (4)) when interfacial states are present with or without incorporation of the image force potential.

We have added this interpretation in the revised text, in page 11 (second paragraph).

Comment 8. On my humble opinion, while referring on equation from table it is better to use (number) than raw and column.

Reply: We insert the equations number in table 1 accordingly with reviewer's recommendation.

Comment 9. Figure 4 (e) shows the barrier height alteration under illumination. On the one hand, quasi-Fermi level position can be calculated by eqs.(4) and (7). On the another hand, quasi-Fermi level position depend on non-equilibrium carrier concentration, which is determined by illumination power. The comparison of results, which obtained by this different way, would be interesting.

Reply: This is very interesting suggestion, but this comparison requires an additional analysis, which can be done in the future.

Comment 10. Page 12, 3-rd paragraph from the top: "In the dark and at small illumination powers, $\Delta\varphi o$ values were extracted by the equation of raw II, column IV in table 1 while at large illuminations, we used the equation of row I, column IV." Which is rigorous criterion for every equation using (or separation of small and large illuminations)?

Reply: The choice of probable limitation mechanism in reverse biased I-V characteristics is carried out in accordance with behavior of the $\boldsymbol{\alpha}$–V characteristics, which have clearly a different dependence on applied voltage. The $\boldsymbol{\alpha}$–V dependence for different mechanisms is clearly demonstrated in Fig. 2(b) and are detailed considerably in the text (see for example page 6 and below).

Comment 11. Page 13, 1-st paragraph from the top: "(see inset in Fig. 2 (b))" must be replaced by "(see inset in Fig. 6 (b))".

Reply: Consequent correction was done.

Comment 12. Figure 7 presents zero bias barrier height. At such condition Vr equals to zero. Therefore, expressions in row I and row III are equivalent. Why are results obtained from reverse and forward branches different?

Reply: In dark the "zero bias" barrier heights are indeed almost identical (see Fig. 7): of about 0.7 eV and 0.71 eV. These values were extracted from I-V characteristics relating to MIS and MS junctions, respectively. Under illumination, the net current in different branches (MIS and MS) are different (due to non-identical junctions and asymmetric illumination conditions: see text). A significant deviation is clearly observed at small illumination powers of about $1.0\text{x}10^{-9}$ W, as seen in the Fig. 7.

Comment 13. The word "raw" (pages 12, 15) must be replaced by "row".

Reply: We change these words by the number of equations.

Reviewer #3 Evaluations:
Does the manuscript present original and timely results that significantly advance the knowledge in applied physics: Yes
Does the manuscript report on convincing and rigorous data methods and analysis: Yes
Is the manuscript clearly written in correct English well organized and free from ambiguities: Yes
Is the title descriptive of the contents concise interesting and free of acronyms: Yes
Does the abstract adequately and clearly describe the contents (problem approach findings) of the paper: Yes
Are the figures in the manuscript necessary adequate well presented and clearly labeled: No
Is the reference list appropriate: Yes

Reviewer #3 (REMARKS to AUTHOR(s)):

The presented work is good and interesting. The method presented is nicely explained with technical details and comparisons. The figures size is too big. Suggest changing the format.

Reply: The merged pdf file may have resulted in certain figures appearing larger than others. However, this will be taken care of during the publication process.